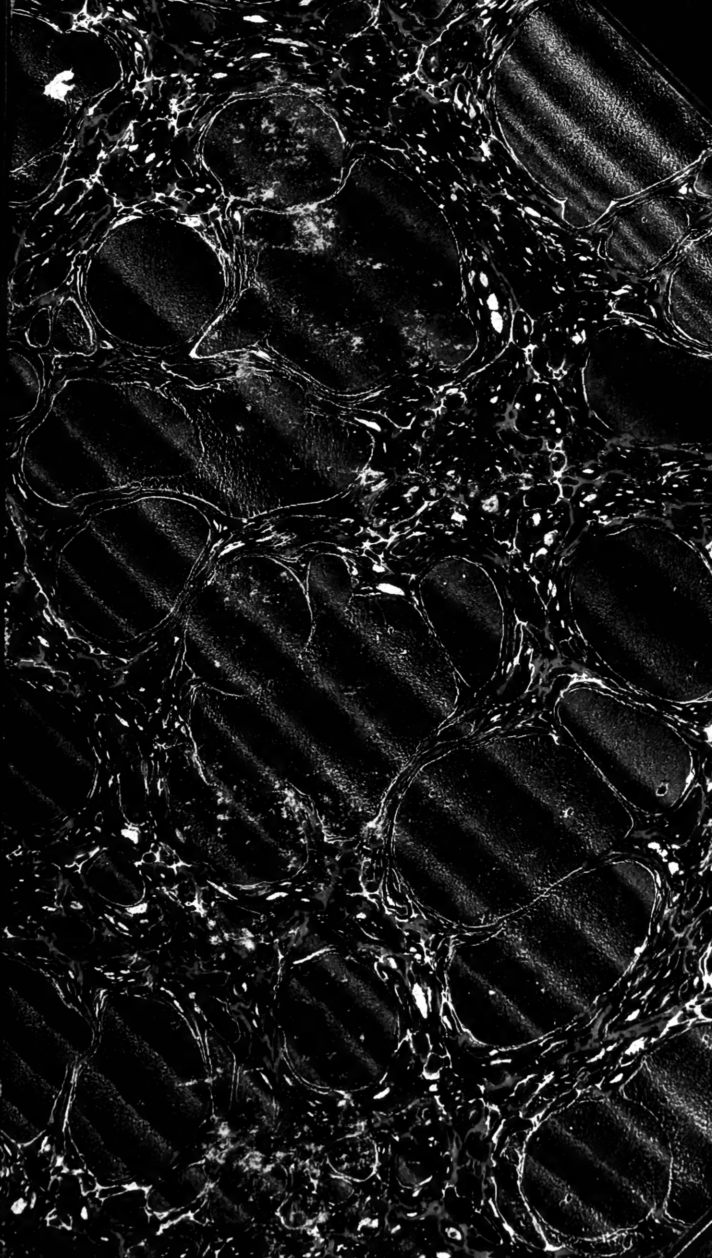

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

CONDUCTED BY

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RICHARD TAYLOR, F.L.S. G.S. Astr. S. [illegible]

SIR ROBERT KANE, M.D., F.R.S., M.R.I.A.

WILLIAM FRANCIS, Ph.D. F.L.S. F.R.A.S. F.C.S.

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JOHN TYNDALL, F.R.S. &c.

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

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

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

J. B. Pinelli ad Mazonium.

CONTENTS OF VOL. XV.

(FOURTH SERIES.)

NUMBER XCVII.—JANUARY 1858.

Page
Mr. J. J. Waterston on Capillarity and its relation to Latent Heat 1
Prof. Helmholtz on the Telestereoscope 19
Dr. Schönbein on the various Conditions of Oxygen 24
Dr. Heddle on the Crystalline Form of Faröelite 28
Dr. Schunck on the Formation of Indigo-blue.—Part II 29
M. du Bois-Reymond on the Electrical Silure 45
Mr. H. Medlock on the Reciprocal Action of Metals and the Constituents of Well- and River-waters 48
Mr. F. Guthrie on a new Form of Spirator 64
Notices respecting New Books:—Mr. C. G. Williams's Handbook of Chemical Manipulation 66
Proceedings of the Royal Society:—
Mr. T. S. Hunt on the part which the Silicates of the Alkalies may play in the Metamorphism of Rocks 68
Mr. J. Paget on the Cause of the Rhythmic Motion of the Heart 70
Dr. Hofmann on a new Mode of forming Triethylamine .. 70
Proceedings of the Geological Society:—
Mr. E. Hull on the Correlation of the Triassic and Permian Rocks of the Odenwald in the Vicinity of Heidelberg and those of Central England 72
Mr. R. B. Smyth on the Extinct Volcanos of Victoria, Australia 74
Prof. J. Phillips on Estuary Strata in Shotover Hill, near Oxford 75
Dr. Bigsby on the Mineralogical and Palæontological characters of the Palæozoic Strata of the State of New York. 76
Note on the Decomposition of certain Salts, particularly Lead-salts, by the Action of the Voltaic Current, by M. C. Despretz. 78
On the Coercive Power of Pure Iron, by A. Matthiessen, Ph.D. 80
On the Formation of Isatine by Ozone, by Prof. O. L. Erdmann. 80

NUMBER XCVIII.—FEBRUARY.

Prof. Brücke on Gravitation and the Conservation of Force... 81
Prof. Draper on the Nature of Flame, and on the Condition of the Sun's Surface 90

Page
Prof. Clausius on the Conduction of Electricity in Electrolytes. 94
Dr. Atkinson's Chemical Notices from Foreign Journals 109
Dr. Schunck on the Formation of Indigo-blue.—Part II. (*continued*) 117
Dr. Heddle on some new Forms of British Sphenes 134
Mr. G. J. Stoney on the Adjustments of the Needle of a Tangent-galvanometer 135
Notices respecting New Books:—Mr. R. Galloway's Manual of Qualitative Analysis 140
Proceedings of the Royal Society:—
  Dr. Hofmann on the History of Thialdine 140
  Dr. Hofmann on the Action of Sulphuric Acid upon Anisic and Salicylic Acids 143
  Mr. F. Field on the Separation of Iodine, Bromine, and Chlorine 144
  Profs. Andrews and Tait on the Density of Ozone 146
  Dr. Hofmann on the History of the Phosphorus-, Arsenic-, and Antimony-bases 147
  Dr. Frankland on a New Series of Compounds derived from Ammonia and its Analogues 149
Proceedings of the Geological Society:—
  Mr. H. C. Sorby on some peculiarities in the Microscopical Structure of Crystals 152
  Mr. J. Prestwich on the Boring through the Chalk at Harwich 154
  Mr. R. Godwin-Austen on a Granitic Boulder out of the Chalk of Croydon, and on the Extraneous Rock-fragments found in the Chalk 155
On the Telestereoscope, by Walter Hardie 156
On the Relations existing between certain Groups of Crystalline forms belonging to different Systems, by C. Marignac 157
On the Composition of Epidotes, Vesuvians and Garnets, by R. Hermann 159

NUMBER XCIX.—MARCH.

M. Poinsot on the Percussion of Bodies 161
Mr. A. Gages on Pseudomorphic Tremolite encrusted with Carbonate of Lime and Magnesia, being apparently the mineral described by Dufrénoy under the name of Miascite........ 180
Dr. Schunck on the Formation of Indigo-blue.—Part II. (*concluded*) 183
Dr. Lloyd on the direct Magnetic Influence of a distant Luminary upon the Diurnal Variations of the Magnetic Force at the Earth's Surface 192
Dr. Debus on the Action of Nitric Acid on Glycerine.—First Memoir 196

Page
Mr. S. Tebay on the Law of Bode, with a remarkable coincidence in reference to the Satellite System of Jupiter; and on the Rotation of a Heavenly Body 206
Mr. J. J. Waterston on the Evidence of a Graduated Difference between the Thermometers of Air and of Mercury below 100° C. derived from M. Regnault's observations on the Tension of Aqueous Vapour. (With a Plate.) 212
Proceedings of the Royal Society:—
  Dr. Hofmann and M. Cahours on a New Class of Alcohols. 226
  Prof. Bunsen and Dr. Roscoe's Photo-chemical Researches. 230
Proceedings of the Geological Society:—
  Dr. Daubeny on the Evolution of Ammonia from Volcanos. 233
  Prof. Haughton on the Granites of Ireland........... 234
  Dr. Bigsby on the Classification of the Palæozoic Strata of the State of New York....................... 234
On the Influence of Musical Sounds on the Flame of a Jet of Coal-gas, by John Leconte, M.D........................ 235
On a New Source of Electrical Excitation, by Mrs. Elisha Foote. 239

NUMBER C.—APRIL.

Mr. W. S. Jevons on the Forms of Clouds. (With a Plate.).. 241
Prof. Callan's Description of a Contact Breaker superior to any hitherto made, and of certain Effects of a Condenser on the Action of various kinds of Contact Breakers 255
Mr. F. M. Jennings on the Lakes and Rivers of Damascus.... 260
Prof. Rogers's Experiments on some Sonorous Flames 261
M. Poinsot on the Percussion of Bodies (*continued*).......... 263
Mr. J. N. Hearder on the Difference in the Amount of Electricity developed by equal surfaces of Cylinder and Plate Electrical Machines 290
Dr. Atkinson's Chemical Notices from Foreign Journals...... 299
Prof. Rankine on the Elasticity of Carbonic Acid Gas 303
Dr. Joule on the Intensity of Light during the recent Solar Eclipse 305
Proceedings of the Royal Society:—
  Mr. A. Cayley on a Class of Dynamical Problems 306
  Mr. W. Hopkins on the Conductive Powers of various Substances 310
  Mr. E. J. Cooper on the Perihelia and Nodes of the Planets. 316
Proceedings of the Geological Society:—
  Mr. C. J. F. Bunbury on a remarkable fossil Specimen belonging to the Genus *Neuropteris*................ 318
  Prof. Huxley on *Cephalaspis* and *Pteraspis* 319
  Prof. Huxley on a New Species of *Plesiosaurus* 320
  Dr. C. Forbes on the Coal found south of Concepcion, in Southern Chili 321

Page
Dr. C. Forbes on a quantity of Crabs thrown up on the beach in Payta Bay 321
Sir R. I. Murchison on the Succession of Rocks in the Northern Highlands 322
Signor Gemmellaro on the Gradual Elevation of a part of the Coast of Sicily.......... 325
Mr. T. F. Jamieson on the occurrence of transported Pebbles and Boulders at high levels in Aberdeenshire 325
On Fluorescence produced by the Aurora, by Dr. Robinson .. 326
Note on the Tangent-galvanometer, by G. Johnstone Stoney.. 327
On the daily Fall of the Barometer at Toronto, by Thomas Hopkins, Esq. 327
On the Action of the Electric Current on Aqueous Solutions of Chlorine, Bromine, and Iodine, by M. A. Riche.......... 328

NUMBER CI.—MAY.

Mr. J. J. Waterston on the Integral of Gravitation, and its consequents with reference to the Measure and Transfer, or communication of Force 329
Prof. Espy on the Law of Storms.......... 345
M. Poinsot on the Percussion of Bodies (*continued*).......... 349
Mr. D. Vaughan on the Solar Spots and the Variable Stars .. 359
Mr. A. Cayley on Cubic Transformation of an Elliptic Function. 363
Professors Tyndall and Huxley on the Structure and Motion of Glaciers 365
Mr. J. Cockle on certain Researches of Euler.......... 389
Notices respecting New Books:—The Rev. G. F. Childe's Investigations in the Theory of Reflected Ray-surfaces, and their relation to Plane Reflected Caustics.......... 390
Proceedings of the Royal Society:—
Dr. Hofmann and M. A. Cahours on the Phosphorus-bases. 393
Prof. Smyth's Report of Proceedings of the Astronomical Expedition to Teneriffe, in 1857 396
Mr. A. Claudet on the Phænomenon of Relief of the Image formed on the Ground Glass of the Camera Obscura .. 397
Proceedings of the Geological Society:—
Mr. A. R. C. Selwyn on the Geology of the Gold-fields of Victoria 400
Mr. J. Phillips on the Gold-field of Ballaarat, Victoria .. 401
Mr. W. Redaway on the Gold-diggings at Creswick Creek and Ballaarat.......... 402
Prof. Owen on some Outline-drawings and Photographs of the Skull of *Zygomaturus trilobus* from Australia 403
On Sonorous Flames, by W. B. Rogers, Esq.......... 404
Note on the Mode in which the Phosphates pass into Plants, by M. P. Thenard 405
On the Mechanical Equivalent of Heat, by P. A. Favre 406

NUMBER CII.—JUNE.

Page

M. Verdet on the Optical Properties developed in Transparent Bodies by the Action of Magnetism.—Part III. 409
Dr. Joule on an improved Galvanometer 432
Prof. Chapman on the Assaying of Coals by the Blowpipe.... 433
Mr. R. C. Carrington on the Evidence which the Observed Motions of the Solar Spots offer for the Existence of an Atmosphere surrounding the Sun.................... 444
Dr. Atkinson's Chemical Notices from Foreign Journals...... 453
M. F. Dellmann on the Observation of Atmospheric Electricity. 460
M. A. De la Rive on the Rotation of the Electrical Light round the Pole of an Electro-magnet 463
Mr. J. P. Gassiot's Description of a Ruhmkorff's Induction Apparatus, constructed by Mr. Ritchie, Boston, U.S. 466
Proceedings of the Royal Society:—
Prof. W. Thomson on the Electro-dynamic Qualities of Metals 469
Prof. W. Thomson on the Electric Conductivity of Commercial Copper of various kinds 472
Prof. W. Thomson and Dr. Joule on the Thermal Effects of Fluids in Motion 477
Proceedings of the Geological Society:—
Mr. H. Rosales on the Gold-diggings at Ballaarat 483
Mr. J. C. Moore on a protrusion of Silurian Rock in the North of Ayrshire 483
Mr. G. W. Ormerod on the Rock-basins in the Granite of Dartmoor 484
Mr. J. Leckenby on the Kelloways Rock of the Yorkshire Coast 484
Mr. N. T. Wetherell on the occurrence of *Graphularia Wetherellii* in nodules from the London Clay and the Crag. 484
Mr. S. V. Wood on the Extraneous Fossils of the Red Crag. 485
Proceedings of the Manchester Society:—
Mr. W. Fairbairn on the Strength of some alloys of Nickel and Iron, similar in composition to Meteoric Iron 486
On the Torpidity of the Marmot, by G. Valentin 488
On the Nitrurets of Tungsten and Molybdenum, by Prof. Wöhler. 488

NUMBER CIII.—SUPPLEMENT TO VOL. XV.

MM. Bunsen and Schischkoff on the Chemical Theory of Gunpowder. (With a Plate.) 489
Prof. Miller's Crystallographic Notices 512
Mr. R. H. Scott's Analysis of Anorthite from a Diorite from the Oural Mountains.......................... 518
Mr. G. Gore on the Rotation of Metallic Tubes and Spheres by Electricity 519

Page
The Rev. R. Carmichael on the Singular Solutions of Differential Equations 522

Proceedings of the Royal Society:—

Dr. Joule on the Thermal Effects of Longitudinal Compression of Solids, and Prof. W. Thomson on the Alterations of Temperature accompanying Changes of Pressure in Fluids 538
Dr. Brown-Séquard on the Influence of Efforts of Inspiration on the Movements of the Heart 543
Dr. Brown-Séquard on the Influence of Oxygen on the vital properties of the Spinal Cord, Nerves, and Muscles. 544
Mr. J. Jago on Ocular Spectres 545

Proceedings of the Geological Society:—

Prof. J. Phillips on a Fossil Fruit found in the Upper Wealden deposits in the Isle of Purbeck 551
Mr. C. J. F. Bunbury on some Fossil Plants from Madeira. 551
The Rev. T. Brown on a Section of a part of the Fifeshire Coast 552

On Euphotide and Saussurite, by T. Sterry Hunt, Esq. 553
Analysis of the Perowskite of Schelingen, by F. Seneca 554
Index 555

PLATES.

I. Illustrative of Mr. J. J. Waterston's Paper on the Evidence of a Graduated Difference between Air- and Mercury-Thermometers.
II. Illustrative of Mr. W. S. Jevons's Paper on the Forms of Clouds.
III. Illustrative of MM. Bunsen and Schischkoff's Paper on the Chemical Theory of Gunpowder.

THE
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[FOURTH SERIES.]

JANUARY 1858.

I. *On Capillarity and its relation to Latent Heat.*
By J. J. Waterston, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the following paper I offer an account of some experiments on capillarity, and certain inductions from them, suggested by the modern views of the dynamical or *work*-value of molecular force. The argument kept in view is, if the capillarity of a liquid is the exhibition of part of the cohesive force of the superficial stratum of its molecules, numerical relations with the latent heat of its vapour ought to be demonstrable if latent heat is the measure of liquid cohesion.

§ 1. As a general rule, the glass tubes and plates were washed first with sulphuric acid, then with nitric acid and with distilled water immediately previous to each set of observations. This was especially necessary when water was the liquid under observation, as it does not attach itself so readily to glass as the spirits and æthers; it is also apt to dry off the glass surface above the line of capillary adhesion. In all cases care was taken to ascertain that the surface of the glass was wet at this part. When by holding the tubes or plates vertical and gently swaying them up and down, the capillary column plays with a delicate independent motion, the results can always be depended upon if the glass surface has been perfectly cleaned with acid. When observing at temperatures higher than that of the atmosphere, this test had to be constantly applied, otherwise the column slowly descended below the point of capillary equilibrium, and no certain results could be obtained.

§ 2. The following mode of observation was employed with two tubes, A and B. The diameters were measured by a scale finely divided into hundredths of an inch, and the reading made with a watchmaker's lens.

$$\left.\begin{array}{l}\text{Tube A}=\dfrac{0{\cdot}242}{0{\cdot}270}\\[6pt] \text{Tube B}=\dfrac{0{\cdot}137}{0{\cdot}152}\end{array}\right\}$$ The numerators express the internal diameters, the denominators the external diameters in decimals of an inch.

The tube being firmly fixed in a vertical position, with its lower edge before the centre of the double lens of a photographic camera, an Argand lamp was placed in front, and the focal distance adjusted so that a distinct image of the tube magnified about 40 diameters was thrown back about 30 feet on paper stretched on a vertical board. A small capillary pipette was employed to pass distilled water upon the outer rim of the tube. By inclining the pipette at a greater or less angle, the column of water in the tube and its suspended drop could be increased or diminished at pleasure with the greatest precision.

Perfectly distinct, highly magnified profiles of the tube and pendent drop were thus obtained at different phases and carefully traced with pencil. The bottom of the cup formed by the upper surface of the liquid was also distinctly marked; the upper edge less distinctly, but the versed sine of the cup seemed constant under all circumstances, and was assumed to be so.

While the suspended volume was increased from the smallest beginning, the upper surface within the tube ascended to a certain maximum height, while the lower surface swelled out to the extent of about one-third the external diameter of the tube below the lower rim. The top surface then descended, and also the lower surface, in an increasing ratio until a certain maximum depression was attained, when the lower drop broke off.

The cycle of this action included first a *rise*, and then a *fall* of the centre of gravity of the liquid, and a contemporaneous closing and expanding of the liquid surface. The interior of the tube, being wet, is taken as *liquid surface augmented* when denuded by the descent of the column, and as *liquid surface diminished* when covered by the ascent of the column.

The question proposed for solution was,—Is the ratio between the amount of work performed (as measured by the descent of the centre of gravity) and the amount of liquid surface developed or denuded, a constant ratio; and if so, to ascertain its numerical value, taking the inch as the standard unit of magnitude?

The liquid volume suspended by the capillary attraction being assumed to be enclosed within the surface of revolution defined by the magnified profile, it was easy by an obvious process of graphical integration to compute for each of the several contours delineated,—1st, the position of the centre of gravity in the vertical axis; 2ndly, the volume of liquid suspended; and 3rdly, the extent of liquid surface. The work performed from one to another of these contours was taken as the mean volume of the liquid descending through the distance of the centres of gravity, and the corresponding surface denuded was the difference of surfaces at those times.

The contours of tube A corresponding to the highest and lowest position of the centre of gravity thus integrated and computed, gave 132·3 as the value of the quotient of the surface denuded by work performed. Expressed by symbols, this quotient $Q=\frac{a^2}{b^3c}$, in which a is the square root of difference of surfaces, b the cube root of mean volume of liquid, and c the vertical distance between the centres of gravity, all expressed with reference to an inch as the unit.

Two adjacent contours of tube B, similarly computed, gave $Q=126$. Two contours of B further removed, similarly computed, gave $Q=134$.

The atmospheric temperature at which these experiments were made was 86° (the locality being within the tropics), but the temperature of the surface of the drop was somewhat uncertain; first, the lamp being within a few inches, tended to heat it; and secondly, the atmosphere being very dry, the evaporation from its surface, sensibly rapid, tended to cool it.

Thus far it seemed proved that the tensile force at the surface of water is uniform, and that the contour of the suspended volume is determined through its complete cycle of elegant curvatures by the quotient of the differential of surface (upper and lower inclusive), by differential of work being a constant quantity.

§ 3. The maximum volume suspended at the extreme development of the drop just before it separates, was found in the case of tube C of the dimensions $\frac{0·083}{0·129}$, by the same method of observation and graphical integration as employed with tubes A and B. The quotient of the sum of the outer and inner rims of suspension by the volume suspended was 129.

The same tube C held vertical was brought down until its lower rim just touched the water in a cup, and the volume suspended by the inner surface of the tube computed: the quotient of the length of the inner circumference by this volume was 127. The temperature was about 86°, as before.

§ 4. Suppose a strip of paper, one inch wide, immersed in water and its whole surface wetted. In pulling it out we develope or denude an aqueous surface of two square inches for each inch of vertical height; and the work required to effect this would be ascertained if we could weigh the water adhering to the lower edge of the paper at the instant of separation; for this weight descending through one inch, is the work that denudes two square inches of aqueous surface. The volume of water equivalent to this weight divided by 2, ought to be about $\frac{1}{132}$ of a cubic inch, if the quotient (Q) of surface developed by the work performed is the same as in the first series of observations.

To try this, a slip of paper 10 inches long and half an inch broad, with clean cut edges, was formed into a spiral having about a quarter of an inch between each convolution. It was then fixed with lac upon a thin piece of plate glass while heated. The lower edge of the paper was thus firmly established in a plane parallel to the plate, and the whole suspended from the pan of a delicate balance, and adjusted by means of a plate and spirit-levels, so as to hang with the lower edge of the paper perfectly level. The paper spiral was now immersed a quarter of an inch in a shallow vessel of distilled water and allowed to soak; the level of the surface of the water being adjusted so that the spiral edge of the paper should separate from it at the turn of the beam. Thus arranged, the difference of weight just at the separation and immediately after, was found by repeated careful observations to be exactly 38 grains. This being the weight of 0·1505 cubic inch of water (at the temperature 86°), shows the volume raised by a water-line 20 inches long. For one inch the volume is $\frac{0{\cdot}1505}{20}=0{\cdot}00752$, or $\frac{1}{132{\cdot}9}$ of a cubic inch. The value of Q thus determined is 132·9.

§ 5. The generally recognized fact, that the power of capillary suspension resides at the water-line—that the weight of the whole column hangs, as it were, upon the water-line, and is equilibriated by the cohering energies of one ring-line of molecules,—is clearly brought out by the following experiment.

A column of water in a capillary tube was drawn out of it by pressing the lower end on an absorbing surface until only a small portion was left. It was then brought down to touch a surface of alcohol. The column rose in the tube, having a small portion of air separating the water from the spirit; and the height attained by the capillary column exceeded that of the water alone exactly in the inverse ratio of the specific gravities of the two liquids. The spirit soon drew down the small heading of water over the wet surface of the tube; and it was interesting to observe, that, at the instant when it all disappeared, the column tumbled down to the level proper to spirit, which in fine tubes is only about four-tenths that of water. In the same way, when spirit forms the heading, the column sprung up at the instant that the upper line of spirit had disappeared. This experiment was repeated with other liquids, and the result could always be anticipated from the specific gravity and specific capillarity.

§ 6. The quotient Q may be computed from the product of the diameter of a tube by the mean elevation of the capillary column, which in theoretical works on capillary attraction is assumed to be constant for the same liquid at constant temperature. Supposing the tube to be drawn up one inch, the capillary column descends one inch, denuding $1^{\text{in.}} \times \pi d$ of surface, and the work performed is the volume $\frac{1}{4}d^2\pi h$ descending one inch: we have therefore

$$Q = \frac{\pi d}{\frac{1}{4}\pi d^2 h} = \frac{4}{dh},$$

in which d is diameter of tube, and h the equalized height of the suspended column. The value of dh, computed from $Q=132$ [§ 2], is $\frac{1}{33}=0\cdot0303$, which agrees nearly with Dr. Brewster's value given in Table, p. 279, of the Report of the British Association, 1834. In small tubes, where the abnormal tendency [§ 8] is manifested, the value of Q is about $\frac{2}{3}$ of 132 or 88, and the corresponding value of dh is 0·045, which agrees nearly with that given opposite the names Martin and Morveau in the same Table. By similar reasoning, in the case of plates we have $Q = \frac{2}{DH}$, in which D is the distance of the parallel plates, and H the equalized height of the capillary column.

§ 7. The 32nd volume of the *Annales de Chimie* (1851) contains an extensive series of researches on capillarity by M. Simon. Water was the liquid chiefly employed, and the dimensions of the capillary column in tubes of different bores, and between plates at different distances, were measured with great minuteness. The most remarkable result of these observations was the discovery of the fact, that the column between plates is only about one-third the height of the column in tubes, instead of one-half, as theory had previously indicated. If we assumed this as a general law, and employed the Table of specific capillarities given in p. 15 for computing the values of Q for alcohol, æther, and carburet of sulphur, we should be led into serious error. The Table shows the comparative heights of the capillary columns in fine tubes only. Another table is required to show the comparative heights between parallel plates, in which, setting out with the height of water as unity, the numbers for carburet of sulphur, alcohol, and sulphuric æther must be multiplied by $\frac{3}{2}$. The temperature at which the observations were made is not specified, which is an omission of some consequence, seeing that there is proof that the ratios of capillarity change with the temperature [§ 10].

§ 8. The existence and extent of this abnormal feature may be ascertained with very simple appliances. I employed a tube about 3 inches long and $\frac{1}{45}$th of an inch diameter, marked with divisions cut by a diamond-point at intervals of about $\frac{1}{14}$th of an inch, each line being a guide to the eye, and the exact length of a reading being ascertained by scale and lens. There were also several thermometer-tubes with flat bore and scale engraved upon them. The glass plates employed were kept separate and parallel by inserting a piece of thin tinned plate, beaten flat and coated with lac, between them when heated. One half of their surface being thus cemented, the other half was left open for the capillary column, and their perfect parallelism could be easily tested. One of the plates was crossed with diamond lines as guides to the eye in observing, and afterwards for applying the scale and lens to obtain the exact reading.

The liquids employed were,—

1*st Class.*

Distilled water.
Sulphuric acid.
Nitric acid.
Ammonia.
Saturated solution of common salt.

2*nd Class.*

Alcohol.
Sulphuric æther.
Sulphuret of carbon.
Dichloride of sulphur.
Acetic acid.
Chloroform.
Acetic æther.
Acetone.
Turpentine.

The ratios of the capillary columns of these several liquids in the tubes and between the plates were compared, and it was found that the columns of the first five liquids (being of the nature of hydrous solutions) had a higher ratio to the others when measured in the tubes than when measured between plates. If the hydrous plate columns had changed in the same ratio as the plate columns of the other liquids, their height in tubes of small bore would have been about two-thirds the height observed. Thus if Q were estimated from the capillarity of water in small tubes, the value is about two-thirds of 132 or 88 (Q being inversely as h, § 6).

The measure of the tensile force at the surface of water and hydrous liquids is thus not to be obtained from the apparent capillary force in small tubes. I found that so small a proportion as 12 per cent. of alcohol destroyed the abnormal tendency of water. I made no observations with tubes of from $\frac{1}{45}$th to $\frac{1}{12}$th of an inch diameter; but in tubes of $\frac{1}{12}$th of an inch and larger, the abnormal feature was found not to exist (see § 3). I found also that in *all* tubes that do not touch a water surface, but that receive their supply of water at the lower rim by capillary pipette, as in the observations described in § 2, when the upper surface is at its maximum elevation, the interior capillary volume estimated above the lower rim *has* the abnormal ratio to the internal water-line. When the tubes thus filled to the capillary maximum are brought to touch a water surface, the water in the wider tubes descends to the normal level, but in the finer tubes it does not. With alcohol and its class, in fine tubes thus supplied at their lower extremities, the maximum abnormal level is never attained, but in the wider tubes the phænomena occur precisely as with hydrous liquids.

§ 9. To find the capillarity of alcohol, I tried spirits of various strengths in a thermometer-tube; and by setting off the heights of the capillary column and the specific gravities as coordinates, and drawing the curve, it was easy to obtain the height corresponding to 0·782, the specific gravity of absolute alcohol at the temperature 86° F., according to Lowitz's Table. This, compared with the height of the water-column in the same tube, gave 228 as the value of Q for absolute alcohol, after making the reduction required by the abnormal height of water in fine tubes. The value of the ratio of reduction is mainly dependent on comparative plate-observations, which are not so precise as comparative tube-observations; but it is satisfactory that it agrees closely with the recent observations of M. G. Wertheim, noticed in the Philosophical Magazine for October last.

The following are the values of Q for sulphuric æther and some other liquids taken at 86° F.:—

228.	Absolute alcohol.
261.	Sulphuric æther (of commerce).
199.	Camphene. (212)
355.	Chloroform.
225.	Acetic acid.
262.	Acetic æther.
225.	Acetone.
288.	Sulphuret of carbon.
257.	Chloride of sulphur.
143.	Ammonia.
263.	Sulphuric acid.
270.	Nitric acid.
132.	Distilled water.

The value of Q for turpentine within parentheses was determined by linear integration of the contours of two phases of the drop suspended in tube B, as described in § 2; but the circumstances were unfavourable. The capillary power of turpentine being small, the change in the position of the centre of gravity that could be observed before the drop broke off did not exceed $\frac{1}{30}$th of an inch. The evaporation between the observations must also have affected the result.

§ 10. A few observations were made with tubes and plates to ascertain the influence of heat in modifying capillary force.

In a glass retort, water mixed with small flowers was kept at a boiling temperature over an Argand lamp. The stopper being removed, a capillary tube filled with water was suspended by a brass wire within the retort; also a thermometer with its bulb close to the tube at the level of the top of the capillary column. The lower end of the tube was about half an inch above the surface of the water in the retort. The versed sine of the undeveloped drop on the lower surface of the tube was about one-fifth the external diameter*.

In this kind of observation the interior of the tube is apt to dry, and the column descends very gradually, much below the true capillary level. By tilting it up and down, it may be kept wet; and good observations may be made in this way, as the lower drop does not sensibly alter.

The temperature at the top of the column was kept at about 95° C., the atmospheric temperature being about 28° C. In a tube with round bore $\frac{1}{45}$th of an inch diameter, the column diminished $\frac{2}{19}$ for a difference of temperature of 66·5 Centigrade degrees. With thermometer-tubes of flat bore the decrement amounted to from $\frac{1}{11}$ to $\frac{1}{12}$, and with plates $\frac{1}{12}$. These ratios were confirmed by another set of observations, made by plunging the tubes into

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* The height of the column in a tube of small bore is but little affected by the size of the drop at the base. In a tube of $\frac{1}{45}$th of an inch internal diameter, and $\frac{1}{4}$th of an inch external diameter, the maximum height occurs when a drop has broken off, the versed sine of the spherical segment of water attached to the base being then about $\frac{1}{5}$th the external diameter. The following are a few of the observed heights and versed sines:—

Height of capillary column above base of tube. in.	Versed sine of volume suspended at base. in.
1·850	·025
1·856	·100
1·838	·125
1·825	·150
1·796	·200 (drop about to break)
1·875	·050 (drop just broken)

a vessel of hot water and taking the reading at the instant of withdrawal.

By a similar method observations were made on alcohol and on turpentine. Between 30° C. and 79° the column of alcohol descended $\frac{10}{113}$; the sample employed had 85 per cent. pure spirit.

Between 32° C. and 132° the column of turpentine descended $\frac{24}{77}$; between 32° and $165\frac{1}{2}$° it descended $\frac{33}{77}$. The sample was prepared by a chemist, and believed to be pure camphene.

§ 11. M. Simon (de Metz) in his 'Recherches sur la Capillarité,' has observations of the influence of temperature on the capillarity of water in tubes of from $\frac{1}{50}$th to $\frac{1}{500}$dth of an inch diameter: the details of the mode of observation are not stated, but the mean result given is a descent of one-fourth from 0° to 100° C.

The following is quoted from his paper in the *Annales de Chimie*, vol. xxxii. p. 17:—

"Ce rapport 4 : 3 ou 1·33 que donne l'expérience conduit à penser que la densité de l'eau est la cause qui produit cette différence d'ascension. En effet, un volume d'eau augmentant de 0·0466 pour 100 dégrés de température, il en résulte qu'une colonne d'eau étant 1 à 0 dégré il sera 1·36 à 100 dégrés : rapport très voisin du rapport moyen des expériences ci-dessus. L'ascension est donc en raison direct de la densité." This conclusion is so manifestly discordant with the premises, that it requires to be noted. In the *Annales de Chimie* for February and March last (vol. xlix.), there is a paper by M. C. Wolf on the influence of temperature on capillarity in tubes of round bore. At p. 259 there is a table of observations taken throughout one year at atmospheric temperatures from 0° C. to 25°·4, with a tube of $\frac{1}{100}$th of an inch diameter, in which every desirable precaution seems to have been taken to ensure accuracy. From 0°·35 to 25°·45 the descent was observed $\frac{6·31}{132·15}$, and it is stated that this agrees with the previous observations of M. Brünner (Poggendorff's *Annalen*, 1847). If this rate is continued up to the boiling-point, the descent from 0° to 100° C. would be $\frac{1}{5·25}$, the height at 0° and from $26\frac{1}{2}$° to $93\frac{1}{2}$° about $\frac{2}{15}$. In another set of observations with a smaller tube, the effect of temperature was found to be less by about $\frac{1}{10}$th. It is probable that the abnormal feature in the capillarity of water in fine tubes affects these results in a greater or less degree. As the true measure of the capillarity of hydrous liquids can only be obtained from observations with parallel plates, it would be desirable to have a set of observations made with water between plates at atmospheric temperatures between summer and winter, in the manner adopted by M. Wolf.

That heat has less effect on the plate-column than on the tube-column, is the impression I have from many observations. When we consider that the effect of adding a very small proportion of alcohol to water is several times greater in tubes than between plates, it appears as if any cause that increased the limpidity of the water had more effect on the capillarity of tubes than of plates.

§ 12. In § 4 it was shown that the weight of 38 grains descending through one inch represents the work required to denude 20 square inches of aqueous surface at the temperature of 86°F., if the value of Q is 132·9. If it is 132, as assumed from the observations in § 2, the weight must be taken at 38·262 grains descending through one inch; and to denude one square inch, it requires 1·9131 grain descending through one inch vertical. This being the weight of ·007577 cubic inch of distilled water at the temperature of 86° F., this volume of water raised through one inch is equivalent to the work of denuding a superficial stratum of one square inch, of overcoming the integral cohesion force *on one side*—the outer side—of a superficial stratum of molecules; being one-sixth of the cohesion integral of all the molecules in the stratum.

A molecule at the surface of a liquid differs from a molecule below the surface, in having that part of its cohesion force which is directed outwards, unmated—disengaged; and the force that operates in disengaging it, is the weight of liquid held in capillary suspension. On applying heat to water to convert it into vapour, we overcome the cohesion force of all the molecules, and the quantity of work which this is equivalent to, we can readily compute from the data afforded by M. Regnault and Mr. Joule, assuming the latent heat of steam to be the cohesion integral of all the molecules while in the liquid state. Thus, having the cohesion integral of *one* stratum of a cubic inch, and the cohesion integral of *all* the strata of a cubic inch, we obtain the number of strata in an inch, and hence the absolute volume of an aqueous molecule.

With similar data for other liquids we may compute the absolute volume of their molecules.

Now the quotient of atomic weight by specific gravity of one liquid compared with the similar quotient of another, gives us the *ratio* of the absolute atomic volumes.

We have thus two distinct modes of computing this ratio,—

1st, from the absolute volumes; 2nd, from the relative volumes. Do they give the same result?

§ 13. From Regnault's empirical formula, the latent heat of aqueous vapour at 86° F. is 1054. One cubic inch of water thus heated is equivalent to one cubic inch of water at temperature 60°, raised through $1054° \times 772 \times 12$ inches vertical, or 9,764,256 cubic inches raised through one inch.

From the capillary force we have, as above, ·007577 cubic inch raised through one inch as the measure of one-sixth the cohesion of one layer of molecules, and ·045462 cubic inch raised through one inch as the integral of one layer of the cubic inch. Hence,

$$\frac{9{,}764{,}256}{\cdot 045462} = 214{,}778{,}500$$

is the number of layers of molecules in one cubic inch, and the cube of the reciprocal of this is the absolute volume of one molecule of water at the temperature 86°.

The process expressed by symbols is $m^3 = \left(\frac{6}{\mathrm{QL}}\right)^3$, in which L is the product of the latent heat by 12 times 772, Q the quotient of capillarity, and m^3 the molecular volume expressed with reference to a cubic inch as unit.

The similar data for alcohol are as follows:—

The latent heat of absolute alcohol at 174° F. is 376, according to MM. Fabre and Silberman (*Ann. de Chim.* vol. xxxvii. 1853, p. 466). The specific heat is 0·64. The ratio of latent to sensible heat in alcohol is about the same as with water, according to M. Despretz, hence 415° may be taken as the latent heat of alcohol at 86°,

$$[174° - 86° = 88°, \quad 88° \times 0{\cdot}64 = 56°{\cdot}3, \quad 56°{\cdot}3 \times 0{\cdot}695 = 39°,$$
$$39° + 376 = 415°],$$

and $415 \times 772 \times 12 = 3{,}844{,}560$ cubic inches of alcohol raised through one inch vertical. From $\mathrm{Q} = 228$ we have $\frac{1}{\mathrm{Q}} =$ volume raised by a water-line one inch long: $\frac{6}{\mathrm{Q}} = {\cdot}026316$ cubic inch raised through one inch as the cohesion integral of one layer of molecules; hence

$$\frac{3{,}844{,}560}{\cdot 026316} = 146{,}092{,}000$$

is the number of layers of molecules of alcohol in one cubic inch, and the cube of the reciprocal of this is the absolute volume of a molecule of alcohol at the temperature 86°.

Comparing this absolute volume with that of water, we have the ratio of molecular volume of water and alcohol equal to 1 : 3·177.

To compute this same ratio from the relative volumes, we have the following data:—

Specific gravity of	water at 68° F.	1·000
” ”	water at 86° F.	·998
” ”	alcohol at 86° F.	·780
” ”	steam	9·
” ”	alcohol vapour	23·16

The quotient of relative volumes gives 1 to 3·294 as the value of the above ratio.

To find the change in Q that brings the first value of the ratio to an equality with the second, let $r=3{\cdot}294$, and

$$\delta r=3{\cdot}294-3{\cdot}177=0{\cdot}117.$$

It is easy to see by differentiating the equation for r, that $\frac{\delta r}{3r}=\frac{\delta Q}{Q}$*, hence $\frac{0{\cdot}117}{3\times 3{\cdot}294}=\frac{1}{84}=\frac{\delta Q}{Q}$, and $\delta Q=2{\cdot}7$ if the error is in the capillary quotient for alcohol only, or $\delta Q=1{\cdot}5$ if the error is in the quotient for water only. In short, an error of $\frac{1}{84}$th in either of the capillary constants is sufficient to account for the discrepancy. Or if these were perfectly correct, then an error of $\frac{1}{84}$th in either one of the latent heats would make the difference.

§ 14. The data for sulphuric æther are less certain. The specific gravity of the æther of commerce employed in determining the capillary quotient 261 was ·7365 at 86° F., but pure æther is only ·7063 at that temperature. The latent heat of the pure liquid, as determined by MM. Fabre and Silberman, is 163°·8 (F.) at its boiling-point. The latent heat of the æther of commerce, according to Dr. Ure, is 302°. It is this last, which, combined with the above value of Q, gives the absolute volume of a molecule of æther in the correct ratio to those of water and alcohol. The difference between the numbers for latent heat is certainly remarkable, and marks this liquid as one not adapted to test the theory. We have also to note, that the rapid evaporation of æther must keep the superficial stratum of molecules probably at a much lower temperature than the rest of the liquid; and as it is the surface particles alone that influence the capillary force, the observed value of Q may be much too small.

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* $\left(\frac{ql}{QL}\right)^3=r,\quad \left\{\frac{(q+\delta q)l}{QL}\right\}^3=r+\delta r,\quad r+\delta r=\left(\frac{ql}{QL}\right)^3+3\left(\frac{ql}{QL}\right)^2\frac{\delta ql}{QL}$

$$\frac{1}{3}\delta r=\left(\frac{ql}{QL}\right)^2\frac{\delta ql}{QL}=\frac{r^{\frac{2}{3}}r^{\frac{1}{3}}\delta q}{q}=\frac{\delta q}{q}r,\quad \frac{1}{3}\frac{\delta r}{r}=\frac{\delta q}{q}.$$

With reference to turpentine, Dr. Ure and MM. Fabre and Silberman agree nearly as to its latent heat; but the molecular volume deduced from its capillarity is not in accordance with that deduced from the specific gravity of the liquid and of the vapour; the latter would require to be twice the observed density.

With reference to acetic æther, it appears that the acetic acid separates from the æther while rising into vapour, as the specific gravity of its vapour is one half what it would be if a volume contained a volume of each of the constituents. If we assume them to be combined to form a single molecule, the absolute volume assigned to it by capillarity is in the proper ratio. And this agrees also if we consider the liquid merely as a mixture with no greater integral of cohesion between a molecule of æther and of acid than between molecules of the æther or of the acid when separate.

§ 15. The absolute volume of a molecule must augment with the temperature according to the expansion of the liquid. Now the capillary force and the latent heat both diminish as the temperature rises. Thus all the elements of the equation [§ 13] $m^3 = \left(\frac{6}{QL}\right)^3$ are observed to vary with the temperature. Is the equation maintained at all temperatures from the point of liquefaction up to the point of transition when the liquid condition suddenly terminates?

With reference to the point of transition, first observed by M. Cagniard de la Tour in liquids subjected to high temperatures in sealed tubes, I have made a considerable number of observations in graduated tubes filled with the same liquid in different proportions, so as to deduce both the density of the liquid and of the vapour when the liquid state terminates. I found that the cup-shape of the upper surface of the liquid caused by its capillarity, ceased at a temperature considerably under the point of transition. In alcohol the surface (examined closely with a watchmaker's lens) became quite flat at 422° F., and the point of transition occurred at 540°. The density of the vapour at that point being 0·222, and of the liquid 0·480, the density at 85° being unity. In sulphuric æther the surface became flat at 385°, and the point of transition at 437° F., when density of vapour was 0·230, and of liquid 0·434.

M. Wolf enclosed a capillary tube in a larger tube with æther, and, raising its temperature, observed the capillary column disappear at 190° C., and the transition of the liquid at 200° C.

The disappearance of the capillary column indicates that the liquid has lost the power of wetting the glass. Liquid cohesion must nevertheless exist up to the point of transition, and ought to give the top of the liquid a convex form; like mercury in glass and in capillary tubes the liquid should descend beyond the outer level.

This was actually remarked by M. Wolf (*Ann. de Chim.* p. 270); I have also observed the convexity. It quickly assumes a sugar-loaf aspect. It requires a steady temperature to show the surface a distinct line.

M. Wolf further observed, that the *rate* at which the capillary column of æther descended, increased at the higher temperatures (p. 271).

It seems to be almost certain that the rate at which latent heat diminishes also augments with the temperature. The following is the evidence on this point.

M. Regnault's empirical formula for the latent heat of aqueous vapour is $\lambda_w = 606{\cdot}5 - 0{\cdot}695t$ (Centigrade). If the ratio ·695 applies to alcohol, which it does nearly according to M. Despretz, then $\lambda_a = 209 - (t - 78)(0{\cdot}695)(0{\cdot}64) = 244 - 0{\cdot}45t$, in which 209° is the latent heat of alcohol at the boiling-point, and 0·64 its specific heat, according to MM. Fabre and Silberman's observations (*Ann. de Chim.* vol. xxxvii. p. 466). If the ratio 0·45 did not increase, transition could not take place until 542° C., instead of 260° or 280° C.

That the specific heat increases with the temperature, is proved by the quotient of the latent heat, by the specific heat being about 400° C., showing that the quantity of heat required to boil off a measure of alcohol is sufficient to raise it to 400° C. if its specific heat were constant.

The same remark applies to water; its latent heat alone would carry it far beyond the point of transition. But it is otherwise with æther, if we take MM. Fabre and Silberman's data. The latent heat could only raise it to 220° C., whereas transition takes place at 200°. The difference, even including the probable amount of sensible heat, gives little room for increase in the specific heat, or for acceleration in the diminution of latent heat.

The same remark applies to turpentine, which I found to undergo a chemical change before it reached the point of transition. It is probable that a portion of the latent heat is absorbed in partially overcoming the chemical forces that keep the elements of each compound molecule together. Thus we have nitrous, formic and acetic æthers boiling at low temperatures, and rising with the æther separated from the acid. Oxalic and succinic æthers, on the other hand, boil at high temperatures, and rise unchanged, the molecule of sulphuric æther still clinging to the acid in the state of vapour. Even with regard to such permanent liquids as water, we must be prepared to view as possible a certain absorption of heat in partially separating, not only the molecules, but their constituent chemical elements, because the application of extreme heat alone is capable of effecting their complete separation.

§ 16. The work expended (and its equivalent of heat made latent) in causing the *same* absolute increment of volume, diminishes as the volume increases. The evidence in favour of this is as follows:—At the boiling-point water expands ·0008 in volume for 1° C., and $\frac{7}{10}$ths of a degree are rendered latent—are expended in performing the work of expansion; hence if the same rate holds good up to the transition point, when the liquid volume is at least double, the quantity of heat required to effect such expansion would be 875°. But the latent heat is only 537°.

At its boiling-point alcohol expands ·0013 per 1°, and $\frac{7}{10}$ths of a degree are rendered latent. At transition-point the volume is doubled. This, at the rate of ·0013 per 0°·7, would require 540 C. degrees of the specific value of the heat of alcohol, or 345 if of the value of the specific heat of water. The latent heat is only 209. Thus it appears that the increment of volume corresponding to constant decrement of latent heat must increase as the liquid expands.

§ 17. I may here remark, that the curve traced out by the expansion of alcohol from −32° C. (observed by M. Pierre, *Ann. de Chim.* vol. xv. p. 354) up to the point of transition agrees remarkably well with the hyperbola, so as to admit of its equation being employed as an empirical formula. This is also the case with sulphuric æther, turpentine, sulphuret of carbon, and some other anhydrous liquids, including mercury.

The form of the equation is $(v-b)(a-t)=c^2$; in which v is the volume, t the temperature by air-thermometer reckoned from the zero of gaseous tension, b, a, and c^2 constants that may easily be computed from three observations.

§ 18. There are hardly sufficient data at present fully to test the equation $m^3=\left(\frac{6}{\mathrm{QL}}\right)^3$ [§ 13] for the same liquid at different temperatures; but it may be of advantage to follow the analysis, and trace out what are the relations amongst the data that are required by it.

As the volume of the molecule must be assumed to increase in correspondence with the volume of the liquid, the expansion of the liquid supplies the exact value of the differential of the left side of the equation at any temperature within certain limits.

At a given temperature, and for a constant increment of temperature, we have

$$\frac{\delta m}{m}=\frac{\delta\left(\frac{6}{\mathrm{QL}}\right)}{\frac{6}{\mathrm{QL}}}=\frac{\delta\mathrm{L}}{\mathrm{L}}-\frac{\delta\mathrm{Q}}{\mathrm{Q}}.$$

In § 6 it was shown that $Q=\frac{4}{dh}$; and d being constant, h is observed to diminish with the temperature [§ 10], hence $\frac{\delta Q}{Q}=\frac{-\delta h}{h}$. Also the latent heat has been observed to diminish with the temperature, so that δL requires the negative sign; and the differential equation, which expresses the relation of molecular volume, latent heat, and capillarity of the same liquid while being acted upon by heat, is $\frac{\delta m}{m}=\frac{\delta h}{h}-\frac{\delta L}{L}$.

It is obvious from this that the first condition required is that $\frac{\delta h}{h}$ should exceed $\frac{\delta L}{L}$, because $\frac{\delta m}{m}$ is positive. The *absolute* value of the difference of the decrements of capillarity and latent heat to fulfil the equation is also indicated.

The most precise observations on the decrement of capillarity are probably those of M. Wolf [§ 11]. The range of temperature was 25° C., and the middle temperature 13°: the value of $\frac{\delta h}{h}$ is ·0477: the value of $\frac{\delta m}{m}$ is ·00089 from M. Despretz's expansion of water, and $\frac{\delta L}{L}=\cdot 0293$ from Regnault's empirical formula. Thus we have

$$-\cdot 0477-\cdot 0293=\cdot 0184 \text{ instead of } \cdot 0009.$$

The first condition, viz. that $\frac{\delta h}{h}>\frac{\delta L}{L}$, is satisfied, but the absolute values do not fulfil the equation. We must keep in view that Regnault's formula assumes that the latent heat diminishes uniformly with the temperature. I have not seen the details of the experiments upon which it is founded, but it is unlikely that they can have indicated more than an average result.

At 5° C. the point of maximum density of water $\frac{\delta m}{m}=0$ and $\frac{\delta L}{L}=\frac{\delta h}{h}$. It appears certain from M. Wolf's observations, that the decrement of capillarity is very uniform, and in tubes of not less than $\frac{1}{100}$th of an inch diameter is not sensibly different from the amount put down. Between plates the decrement is probably less, on account of the abnormal feature in the capillarity of water in tubes; but it cannot be less than two-thirds of the above, or ·0318. The evidence of an increase of the decrement of latent heat and of the specific heat at high temperatures has been stated [§ 15]; there is consequently no reason to suppose that the value of $\frac{\delta L}{L}$ can be greater than ·0293, unless the peculiarity in the expansion of water affects it in an abnormal way.

Between 28° and 95° C. I observed the capillary column in flat tubes and plates to descend $\frac{1}{12}$, the height at 28° or $\frac{1}{11\frac{1}{2}}$, the height at middle temperature $61\frac{1}{2}$° [§ 10]; hence $\frac{\delta h}{h} = \cdot 087$. The value of $\frac{\delta \mathrm{L}}{\mathrm{L}}$ at this temperature is from Regnault's formula ·0785, and $\frac{\delta m}{m}$ from Despretz is ·0117. These give $\cdot 087 - \cdot 0785 = \cdot 0085$ instead of ·0117. If we assume the decrement of capillarity $\frac{1}{11}$, we should have $\cdot 0909 - \cdot 0785 = \cdot 0124$, which is very close to the observed value of $\frac{\delta m}{m}$. The value of this $\frac{\delta h}{h}$, computed from M. Wolf's empirical formula, is $\frac{1}{7}$. This formula is derived from observations made in a tube of round bore $\frac{1}{100}$dth of an inch in diameter from 0° to 25°.

Dr. Young, in his essay on the cohesion of liquids, casually mentions that he observed the capillary column descend at the rate of $\frac{1}{1000}$th for each degree of Fahrenheit's scale above 50°. This would give $\frac{\delta h}{h} = \frac{2}{17}$. In a tube of $\frac{1}{45}$th of an inch round bore I found it to be $\frac{2}{19}$ [§ 10]. The equation requires it to be $\frac{2}{21}$.

§ 19. In the cases of alcohol, æther and turpentine, we have no direct information as to the value of $\frac{\delta \mathrm{L}}{\mathrm{L}}$; but we may compute it for each of these liquids by means of the equation, from their differentials of volume and capillarity. Future observation must decide as to their accuracy. Enough for the present if they are such as may be deemed probable.

The data for alcohol (85 per cent. pure spirit) are, from 30° to 79° C. column descends from 113 to 103, or $\frac{\delta h}{h} = \frac{10}{108}$, $\frac{\delta m}{m} = \frac{184}{9716} = \cdot 0189$. Hence $\frac{\delta \mathrm{L}}{\mathrm{L}} = \cdot 0737$, and $\mathrm{L} = 253°$ (allowing for the 15 per cent. water); hence $\delta \mathrm{L} = (253 + \frac{1}{2}\delta \mathrm{L}) \cdot 0737 = 19°{\cdot}3$, and the specific heat of alcohol being ·64, we have $\frac{19{\cdot}3}{49 \times 0{\cdot}64} = 0{\cdot}61$ as the proportion of each degree that goes to diminish the latent heat, while alcohol is heated from 30° to 79° C. The corresponding proportion in water is, according to Regnault, 0·695. This proportion may be called $\delta\lambda$, it being understood that the latent heat is expressed in degrees, which have a mechanical equivalent corresponding with the specific heat of the liquid.

The data for sulphuric æther are, from 14°·79 to 34°·78 C. column descends from 35·44 to 32·14 (M. Wolf, *Ann. de Chim.* vol. xlix. p. 269), hence $\frac{\delta h}{h}=\frac{3{\cdot}30}{33{\cdot}79}={\cdot}09766$, $\frac{\delta m}{m}={\cdot}01066$ (M. Pierre, *Ann. de Chim.* vol. xv. p. 361); hence $\frac{\delta \mathrm{L}}{\mathrm{L}}=0{\cdot}0870$, and L=92°, hence $\delta\mathrm{L}=8^\circ{\cdot}4$; and specific heat of æther being 0·504, we have $\frac{8^\circ{\cdot}4}{20\times 0{\cdot}504}=0{\cdot}83=\delta\lambda$.

The data for turpentine are—

1. From 132° to 165½° C. the column descended from 53 to 44 [§ 10], hence $\frac{\delta h}{h}=\frac{9}{48{\cdot}5}=0{\cdot}1856$, $\frac{\delta m}{m}=\frac{{\cdot}0144}{1{\cdot}1288}={\cdot}0127$,

$$\frac{\delta \mathrm{L}}{\mathrm{L}}={\cdot}1856-{\cdot}0127={\cdot}1729,\ \mathrm{L}=69^\circ$$

(MM. Fabre and Silberman, *Ann. de Chim.*), and $\delta\mathrm{L}=13^\circ{\cdot}1$. The specific heat of turpentine being 0·467, we have

$$\frac{13{\cdot}1}{33{\cdot}5\times{\cdot}467}=0{\cdot}837=\delta\lambda.$$

2. From 32° to 132° C. the column descended from 77 to 53 [§ 10]; hence

$$\frac{\delta h}{h}=\frac{24}{65}=0{\cdot}370,\quad \frac{\delta m}{m}=\frac{{\cdot}0358}{1{\cdot}060}={\cdot}0337,\ \text{and}\ \frac{\delta \mathrm{L}}{\mathrm{L}}={\cdot}336;$$

$\mathrm{L}=69+13=82^\circ$, and $\delta\mathrm{L}=33{\cdot}1$. Specific heat (assumed same as last, though probably it is actually less) 0·467, we have

$$\frac{33{\cdot}1}{100\times{\cdot}467}={\cdot}709=\delta\lambda.$$

§ 20. The relation between capillarity and latent heat—assuming it to be proven—enables us to compute the latent heat from the capillarity, and *vice versâ, e. g.* mercury. M. Gay-Lussac determined the constant product of the capillarity of this metal to be $\cdot0201=\frac{4}{\mathrm{Q}}$ [§ 6], hence Q=200. From the general equation $m^3\left(\frac{6}{\mathrm{QL}}\right)^3$ [§ 13], we have $m^3\mathrm{Q}^3\mathrm{L}^3=m_1{}^3\mathrm{Q}_1{}^3\mathrm{L}_1{}^3$* and

$$\mathrm{L}_1=\frac{m\mathrm{QL}}{m_1\mathrm{Q}_1}=\sqrt[3]{\frac{9}{7{\cdot}44}}\times\frac{132\times1072}{200}=754^\circ\ [\text{Fahrenheit's scale}].$$

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* The product mQL has the same constant value for all liquids at any temperature.

Its numerical value $\left\{\begin{array}{ll}\text{for Fahrenheit's scale is} & 294330\\ \text{for centesimal scale} & 163516\end{array}\right\}$, and m^3 is expressed as quotient of atomic weight (on hydrogen scale) by specific gravity, L is latent heat expressed in degrees, and Q, as described in §§ 2 and 6, has reference to the inch as unit.

This heat applied to the metal would raise it 25133°.

The weight of a molecule of mercury being $11\frac{2}{9}$ times that of water, the cohesion integral of a molecule is $\frac{8373}{1072}$ times that of a molecule of water, while its bulk is less in the proportion of 7·44 to 9.

22 London Street, Edinburgh,
November 2, 1857.

II. *On the Telestereoscope*. *By* Prof. H. Helmholtz*.

THE image upon the retina of every human eye represents a perspective projection of the objects situated in the field of view. As the positions from which these projections are taken are somewhat different for both eyes of the same individual, the perspective images themselves are not identical; and we make use of their difference, as stereoscopic experiments teach us, to obtain an idea of the different distances of the objects represented from the eye. Now the images of the same object on the two retinæ are the more different from each other the nearer the object is to the eyes. In the case of objects, in comparison with whose distance the space between the eyes is a vanishing quantity, the difference between the two images also vanishes; and for such objects we lose the aid just spoken of, in estimating their distance and bodily figure.

The inspection of distant objects of irregular form, for example of mountain ridges which close our field of view, will convince us of this. The latter always appear to encircle us at the horizon, like an erect wall: we perceive nothing of the protuberances, indentations, nor of the different mountain chains which lie hid one behind another, except through the help of shadows, aërial perspective, or a more exact knowledge of their forms previously obtained. With objects of irregular [regular ?] form, such as buildings, &c., we are more aided by a simple perspective drawing in forming a conception of those dimensions which lie in the direction of the depth of the picture.

In the case of stereoscopic landscapes, which are now produced to such an extent by photography, this defect is remedied by the photographer choosing for the taking of his pictures two sufficiently distant positions, and thus obtaining two sufficiently different projections of the landscape. The person who views these in the stereoscope, believes that he sees a reduced model of the

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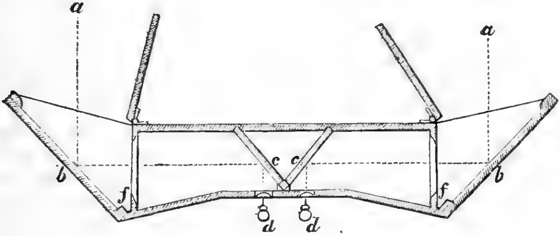
* From Poggendorff's *Annalen*, 1857, No. 9.

landscape, the dimensions of which are to those of the landscape as the distance between the two eyes is to the distance between the two positions of the camera from which the views were taken.

We have here the reason why these stereoscopic images give a much clearer representation of the form of a landscape than the view of the landscape itself, at least to a passing traveller, who is not so well acquainted as the inhabitants with the single objects of the neighbourhood. Towns, which to the observer on a high point, appear merely as a desert of roofs, resolve themselves in the stereoscope to rectangles bounded by streets; the relative height of the houses is discerned, the width of the streets, &c. In accordance with this, I find that a better idea is obtained of the gigantic dimensions of the higher Alps in the stereoscope, than by an alpine journey; because those who are unaccustomed to such mountain excursions and prospects in most cases consider the mountains too near, and hence too small, partly from the want of aërial perspective, partly because the power of judging of such great dimensions has not been exercised. Only after the labour of ascent, and after viewing the same mountain successively from different positions, is a more or less complete notion of its magnitude obtained. The advantages which the stereoscope offers in this respect have been thus far but little used, because photographers on the whole seem unjustly to avoid making use of a great distance between the points whence the views are taken. It may be perceived that, for example, bodily images of the most remote parts of the higher Alps may be obtained when points are chosen for the taking of photographic views some thousands of feet distant from each other. While inspecting good models of these mountains, I have always found that I had obtained very incomplete notions of the mountain groups from a journey through the country. In general I had squeezed them too closely together, and represented their bases too small. Here doubtless also is to be found the reason why models of mountains with exaggerated heights please us better than such as represent the elevations on a correct scale. The former correspond more to the impressions which we receive on a hasty journey through a mountain country. By means of a little instrument which I have named the telestereoscope, a portion of the advantage possessed by stereoscopic photographs may also be realized while looking directly at a landscape. The object of the instrument is to present stereoscopically united two pictures of the landscape corresponding to two points of view whose distances considerably exceed the distance between the two eyes. The annexed figure shows a mean horizontal section of the instrument, one-eleventh of its natural size.

The essential parts of it are the four mirrors, *b*, *b* and *c*, *c*, placed vertically in a wooden box, and fixed at an inclination of 45° to its longest edge. The exterior mirrors, *b*, must be large, the interior ones, *c*, may be small, and to avoid distortion all the mirrors must be of the best plate-glass. The light coming from distant objects is reflected twice at a right angle on its way, *a b c d*, and falls at *a d* into the two eyes of the observer. At *f f* diaphragms are placed, to prevent other light entering the eyes than that which has been twice reflected. In the apertures in the box through which the observer looks, it is convenient to place two very feebly concave lenses, of from thirty to forty inches focal distance, because the majority of eyes do not see very distant objects distinctly, which is here the precise point to be attained. Such weak lenses do not hinder normal eyes from seeing distinctly.



It is in general better to make the mirrors fixed, as is supposed to be done in the drawing; for certain physical experiments, particularly for the inspection of near objects, it is useful to have the four mirrors turning on vertical axes.

Each eye of the observer sees in the small mirror the image of the large one at its side, and in the large one the image of the landscape; the latter, however, he sees in the perspective projections in which they appear from the two large reflectors, by which of course he obtains a much greater difference in the two perspective views than the two eyes of the observer could furnish by looking directly at the landscape. In order to determine accurately the position from which the landscape in this case is regarded, we must seek the images of the observer's eyes given by the two pairs of mirrors, which in the figure would lie in the direction *a b*, beyond *b*, and at a distance from $b = bc + cd$. By the instrument, therefore, the distance between the observer's eyes is artificially magnified to *b b*.

The landscape appears to the observer like a reduced model. It matters not whether concave glasses are added to the instrument or not. All portions of the landscape that are not too distant, assume the same bodily appearance as in the stereoscope, and retain at the same time the whole richness of the natural colours, so that images of surprising beauty and elegance are obtained.

Distant objects certainly appear flat, but still detach themselves from their background. Thus, for example, mountains at two English miles distance detach themselves from the firmament. As in the case of stereoscopic photographs, the aspect of groups of trees is very surprising; for the single boughs, and the single twigs of each bough, detach themselves completely from each other. Low clouds also appear frequently more solid, and more separated from each other in the instrument, than when regarded with the naked eye.

The greater the distance between the two large mirrors, the further in the distance does the instrument show the bodily form of objects. Large reflectors give a large field of view. If therefore it is desired to fix the instrument so as to regard in it a certain prospect, it will be advantageous to make the reflectors as large as possible, their distance asunder a maximum, and to place the whole upon a table which admits of being turned. For general purposes, it is convenient not to make the length of the instrument greater than the width of a window, so that it may be used from the room within. We obtain, moreover, a great portion of the effect with small instruments, in which the distance of the reflectors from each other is much less.

Those who are accustomed to make optical experiments, can obtain a telestereoscopic view of a landscape without any other instrument than a small and a large mirror. The large mirror is so suspended, that when looked into at an angle of 45° the landscape is seen. The observer stands at a distance of some feet from the mirror in this direction, and holds the small reflector parallel to the large one, opposite to the eye which is nearest to the large mirror. For example, when the right side of the observer is nearest the large mirror, the landscape in the small mirror is to be regarded with the right eye, and in the large mirror with the left eye. When both images are brought to coincidence, the same optical effect is obtained as in the telestereoscope. In this way, however, it is only with difficulty that different parts of the view are successively united, and nearer objects appear to the left eye of less magnitude than to the right one.

To see near objects in the telestereoscope, the reflectors must be turned round their vertical axes so that the angle between their surfaces and the long edge of the box is somewhat greater than 45°. The objects then appear greatly reduced in size, but in surprisingly prominent relief. When the large mirrors only are turned, the small ones being left at the angle of 45°, an exaggerated relief is obtained. If the dimensions in the direction of the depth of the field of view to those on the surface are to retain their right relations, the large mirrors must always remain parallel to the small ones. The aspects of near objects, particularly of the human figure, are strikingly beautiful in the telestereoscope. The impression differs from the reduction produced by concave glasses, in the circumstance that it is not reduced pictures that the observer imagines he sees, but actually reduced bodies.

Magnifying power may easily be connected with the telestereoscope: it is only necessary to place a double opera-glass between the eyes of the observer and the small reflectors; it is still preferable for the field of view, to separate the eye-glass from the object-glass of the instrument, and so fix them in the telestereoscope that the light at each side first strikes the large mirror, then the object-glass, then the small reflector, and finally the eye-glass; so that in this arrangement the optic axis of the telescope itself is broken at a right angle. The greater the magnifying power, the greater of course must be the perfection of the plane reflectors, but then it is not necessary to choose them larger than the object-glass of the telescope.

These views, at the same time telescopic and stereoscopic, also exceed to an extraordinary degree the common image of the telescope in vividness. In the simple telescopic images, difference of distance disappears totally: the objects look exactly as if they were painted on a plane surface. By the ordinary combination of the two Galileo's telescopes, the appearance of relief for nearer objects is in some degree obtained; and hence it is that a double opera-glass gives a much livelier impression of relief than a single one. But in the usual construction of the instrument the relief is false: the objects appear as if they were squeezed together in the direction of depth. In the case of human faces, on which, for the most part, opera-glasses are directed, this is very striking. When they are regarded from the front, they appear much flatter than they really are, and when looked at in profile, they appear too narrow and sharp. In both cases the expression of the countenance is essentially altered.

When a double opera-glass is turned round and the observer looks through the object-glass, the deep dimensions of objects are magnified out of proportion. While, therefore, through a simple telescope all objects appear as paintings, through a double opera-glass, complete objects are seen as *bas-reliefs*, while by reversing the opera-glass, objects appear in high relief.

From the known laws of the telescope and stereoscopic vision, it is easy to find theoretically that a double telescope whose optic axes are parallel, and exactly the distance of the two eyes of the observer from each other, and which magnifies n times, causes objects so to appear as if all dimensions perpendicular to the axis of the telescope remained unchanged, while the distance of the objects from the observer in the line of the axis is reduced $\frac{1}{n}$th, so that the observer sees the objects in their natural size, but nearer, and compressed in the direction of depth.

While each single telescope shows this object as if it were at $\frac{1}{n}$th of the distance, the difference between the perspective view is still not so great as it would be if the observer actually saw the object at the $\frac{1}{n}$th of the distance. By combining a double telescope with a telestereoscope, this error is not removed; we only obtain a uniform further reduction of all apparent linear dimensions as they appear in the double telescope. Of single objects, indeed, which stand at a definite distance, a correct relief can be obtained by permitting the small reflectors to remain at an angle of 45°, and making the large ones reflect at an angle somewhat smaller than 45°. In this way, as before mentioned, an exaggerated relief is obtained in the telestereoscope alone, and we can thereby neutralize the opposite error of the telescopic combination.

III. *On the various Conditions of Oxygen.*
By Dr. Schönbein*.

My dear Faraday, Bâsle, Sept. 17, 1857.

I HAVE continued my researches on oxygen, that inexhaustible source of investigation, and have become acquainted with a series of novel facts, which seem to be not altogether void of scientific interest. One of those facts is singular and paradoxical enough. What do you say to a deoxidation of an oxygen compound being effected by means of oxygen itself? You are perhaps aware, that some years ago I found out a number of substances which possessed the power of transforming free $\overset{\circ}{\mathrm{O}}$ into O, *i. e.* of acting like heat. The oxides of the precious metals, and the metallic peroxides, such as those of manganese, lead, &c., belong to this category, and are oxy-compounds which contain either the whole or part of their oxygen in the ozonic condition. Now it appears that the action which takes place, for instance between free $\overset{\circ}{\mathrm{O}}$ and $\mathrm{PbO}+\overset{\circ}{\mathrm{O}}$, is reciprocal; for not only is the former converted into O, but the peroxide of lead is at the same time reduced to PbO, thus appearing to show that the $\overset{\circ}{\mathrm{O}}$ of

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* Communicated by Professor Faraday.

$PbO+\overset{\circ}{O}$ also becomes deozonized, and on that account eliminated. The same deoxidizing effect is produced upon PbO^2 by the ozonized oil of turpentine and the peroxide of hydrogen (to me $= HO+\overset{\circ}{O}$). To show these remarkable effects in a simple manner, I employ a test-paper which is impregnated, *i.e.* coloured with minute quantities of peroxide of lead. If moist strips of such paper be suspended in strongly ozonized air, within a few hours they will be completely bleached, *i. e.* PbO^2 is reduced to PbO. The said test-paper, on being put in ozonized oil of turpentine or peroxide of hydrogen, undergoes the same change. The strips enclosed will show you that effect. Now these curious facts seem to me to furnish ground for a very strange conjecture, which, extraordinary and startling as it may sound, I cannot help communicating to you. I suspect that there are two kinds of active oxygen standing to each other in the relation of algebraic magnitudes of contrary signs, *i. e.* such as will neutralize each other into inactive oxygen if brought together in equal quantities. Now, supposing that there are three kinds of oxygen, $\overset{\circ}{\oplus}$, $\overset{\circ}{\ominus}$, O, and assuming that HO^2 is $= HO+\overset{\circ}{\oplus}$, and $PbO^2 = PbO+\overset{\circ}{\ominus}$, those peroxides, on being brought into contact with one another, must be catalyzed, because the $\overset{\circ}{\oplus}$ of the one neutralizes the $\overset{\circ}{\ominus}$ of the other peroxide into O, which, as such, can no longer remain associated either with PbO or HO. For the reduction of PbO^2 to PbO being effected by free $\overset{\circ}{O}$, I am inclined to account in the same way, *i. e.* by assuming opposite states of the two portions of oxygen which act upon each other. In the two isomeric and crystallographically polar acids of Pasteur, which neutralize each other into what they call racemic acids, we have a case of an analogous kind.

I am of course far from believing that the facts above stated necessarily lead to such a conclusion, but for the present I cannot conceive any other hypothesis by which the deoxidizing effect produced by free $\overset{\circ}{O}$, ozonized oil of turpentine, or peroxide of hydrogen upon PbO^2, can be better accounted for. Be this, however, as it may, as we philosophers cannot do any notable work without having some hypothetical view in our heads, I shall place myself for some time under the guidance of the conjecture alluded to, and see what can be made out of it. If it leads me to the discovery of some interesting facts, I shall not feel ashamed of it, though it may turn out to be fallacious. We are but short-sighted men, and must be content with finding out a little bit of truth in wading through a sea of errors.

You know it is an old notion of mine, that common oxygen, as such, cannot enter into any chemical combination, and must undergo a change of condition, *i. e.* become ozonized before it acquires oxidizing powers. The oxidation of phosphorus, oil of turpentine, &c. being apparently effected by common oxygen, is typical to me, because we know, and I think with sufficient certainty, that in those cases the ozonization of common oxygen always precedes oxidation. In order to increase as much as possible the body of evidence in favour of that assumption, I have of late worked a good deal on the oil of bitter almonds (benzule + H), which, as regards its bearings to common oxygen, is certainly one of the most remarkable bodies I know; for oxygen at the common temperature, submitted to the joint influence of this oil and solar light, effects a number of oxidations, which only $\mathring{O}$, but not O, is capable of causing. Under the circumstances mentioned, iodine is eliminated from iodide of potassium, indigo solution discoloured rapidly, solution of guaiacum blued, a great number of metals oxidized, even silver not excepted, &c.

To convince yourself of this action in a simple way, add to diluted paste of starch containing some iodide of potassium, a drop or two of hydruret of benzule (free from prussic acid), shake in the dark the mixture together with ordinary oxygen, and no action will result; make the same experiment in the sun, and the liquids will almost instantaneously be turned deep blue, just as if free ozonized oxygen had acted upon the paste. The same colour will make its appearance if you treat in a similar way a recently prepared solution of guaiacum. To show that even silver is oxidized, put some drops of the oil upon a plate of pure silver, and move the essence about in direct sunlight for a minute or two; on aqueous sulphuretted hydrogen being poured upon the spot of reaction, a rather abundant precipitation of sulphuret of silver is produced, a proof of the presence of oxide of silver. I need not expressly state that the hydruret of benzule is oxidized along with the metals, in consequence of which benzoates are formed—benzoate of lead, cadmium, copper, silver, &c. A very pretty experiment may be made with metallic arsenic. Deposit round a glass tube a ring of that metal (by means of Marsh's method), drop some oil of bitter almonds upon it, and turn the tube, held in a horizontal position, round its axis: no action takes place in the dark, whilst in the direct solar light the ring will rapidly disappear under the circumstances indicated, arsenic acid being formed, just as is the case in ozonized oxygen. Rings of antimony being not acted upon, or at least but very slightly, under these circumstances, the two metals may be easily distinguished from each other by means of hydruret of benzule. The details of my researches on the oil of bitter almonds will before long be published by the Academy of Paris.

You are aware that nitrification has been these many years a matter of interest and research to me, and of late I have increased our knowledge of that subject by some novel facts. Some years ago I found out that ozonized oxygen transforms ammonia into the nitrate of that base. Last year I ascertained that inactive oxygen, on being put in contact with platinum or copper, acquires the power of oxidizing, even at the common temperature, the elements of ammonia into nitrous acid and water, nitrite of ammonia being formed. Now I have discovered that HO^2, $Mn^2 O^7$ (permanganic acid), or the salts of that acid, for instance permanganate of potash, on being mixed with aqueous ammonia, produce nitrites. A singular fact is, that free ozonized oxygen alone seems to be capable of oxidizing the nitrogen of ammonia into nitric acid, whilst the ozonized oxygen of oxy-compounds, or the oxygen rendered active by the influence of copper or platinum, produces nitrous acid. Are we to infer from these facts that the formation of a nitrite is the first stage of nitrification? One gambol more on my hobby-horse and I shall descend from the animal. I have of late succeeded in ozonizing the oil of turpentine so strongly, that one equivalent of that essence is associated to an equivalent of oxygen, and you may easily imagine the great oxidizing power of the oil. By shaking it with peroxide of lead it becomes deozonized, PbO^2 being reduced to PbO, a fact which, according to the statements above made, is a matter of course.

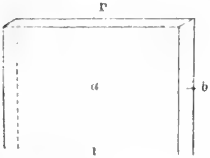
I must add a remark or two on my peroxide test-paper. I prepare it by drenching strips of thin filtering-paper with a solution of PbO^2, and that solution is produced by shaking together (for about fifteen minutes or so) two volumes of strongly ozonized oil of turpentine and one volume of *extractum Saturni* (subacetate of lead). On filtering the mixture I get a transparent liquid, coloured like port wine, which in fact is oil of turpentine holding some peroxide and oxide of lead dissolved. Upon the filter remains a yellow substance, a mixture of PbO^2 and PbO. Within twenty-four hours a similar mixture is deposited from the coloured essence. It is a remarkable fact that the test-paper is rapidly bleached in strongly insolated atmospheric air, as you will see from a strip sent, which in a bright sun was completely bleached within an hour's time. For that reason my test-paper must be kept in the dark.

I am, my dear Friend,
Yours most faithfully,
C. F. Schönbein.

IV. *On the Crystalline Form of Faröelite.*

By Dr. HEDDLE*.

WHEN in Faröe, I obtained at the cave in Naalsöe globules of Faröelite, composed of crystals large enough to afford satisfactory measurements with the reflecting goniometer. They are as follows:—



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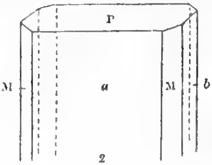

2

P *a*	90° 00′
P *b*	90 00
a b	90 00
(cleavage)—	
M *a*	116 20

Form, *a b p*.

Cleavage parallel to *a* highly perfect; parallel to *b* perfect; M imperfect. Lustre, *a* pearly; *b* P and M vitreous.

The Skye crystals are similar; but some of the Irish specimens seem to have planes truncating the edge P *b* or the solid angle *a* P *b*; these crystals, however, are too rough and too small for measurement.

I have already shown in this Journal (January 1857) that the chemical constitution of this species is quite distinct from that of mesolite, with which it used to be confounded; and the above measurements definitely set the matter at rest, as the mineral is prismatic instead of oblique, being near Thomsonite, and isomorphous with stilbite.

It stands, indeed, intermediate, and connects the chain between Thomsonite and mesolite.

Thomsonite (*normal*) . $\dot{\mathrm{C}}\mathrm{a}^3\ \dddot{\mathrm{S}}\mathrm{i} + 3\dddot{\mathrm{A}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i} + 7\dot{\mathrm{H}}.$

Soda Thomsonite . . $(\dot{\mathrm{N}}\mathrm{a},\ \dot{\mathrm{C}}\mathrm{a}^2)\ \dddot{\mathrm{S}}\mathrm{i} + 3\dddot{\mathrm{A}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i} + 7\dot{\mathrm{H}}.$

Faröelite . . . . . . $(\dot{\mathrm{N}}\mathrm{a},\ \dot{\mathrm{C}}\mathrm{a}^2)\ \dddot{\mathrm{S}}\mathrm{i}^2 + 3\dddot{\mathrm{A}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i} + 8\dot{\mathrm{H}}.$

Mesolite . . . . . . $(\dot{\mathrm{N}}\mathrm{a},\ \dot{\mathrm{C}}\mathrm{a}^2)\ \dddot{\mathrm{S}}\mathrm{i}^3 + 3\dddot{\mathrm{A}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i} + 8\dot{\mathrm{H}}.$

With Scolezite its connexion may be seen by adding—

Scolezite . . . . . . $3(\dot{\mathrm{C}}\mathrm{a}\,\dddot{\mathrm{S}}\mathrm{i}) + 3\dddot{\mathrm{A}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i} = 9\dot{\mathrm{H}}.$

With natrolite by adding—

Fargite . . . . . . $(\dot{\mathrm{N}}\mathrm{a}^2\,\dot{\mathrm{C}}\mathrm{a})\ \dddot{\mathrm{S}}\mathrm{i}^3 + 3\dddot{\mathrm{A}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i} + 7\dot{\mathrm{H}}.$

Natrolite . . . . . . $3(\dot{\mathrm{N}}\mathrm{a}\,\dddot{\mathrm{S}}\mathrm{i}) + 3\dddot{\mathrm{A}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i} + 6\dot{\mathrm{H}}.$

N.B. The crystals from which the figure was drawn were examined chemically, to determine that they were not stilbite.

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* Communicated by the Author.

V. *On the Formation of Indigo-blue.*—Part II.

By Edward Schunck, *Ph.D., F.R.S.*[*]

IN the first part of this memoir[†] I announced the discovery of a peculiar substance contained in the leaves of the *Isatis tinctoria,* to which, as I there showed, the indigo-blue obtained in the usual process of treating the plant owes its origin. Having applied to this substance the name of *Indican,* I proceeded to give a general account of its properties, and of the process of decomposition which it undergoes when subjected to the action of strong acids. I now propose to present a more detailed account of the properties of this substance, and especially of the products of decomposition derived from it.

In continuing my experiments, I soon arrived at the conclusion that the different methods of preparing indican, of which I had in the first part of this paper given a description, though they sufficed for the preparation of small quantities, were not well adapted for obtaining in a state of purity the larger quantities of the substance which I found to be necessary for the purposes of investigation. The great difficulty in the preparation of indican arises, as I have before stated, from the extreme facility with which it is decomposed, when its solutions, especially the watery one, are heated, a process of decomposition which is rapidly completed at a temperature a little below that of boiling water, and takes place even at the ordinary temperature of the atmosphere, when the evaporation occupies some time. This circumstance renders it necessary to avoid distilling the solutions or evaporating them at any but the usual temperature. On the other hand, the length of time necessary for the spontaneous evaporation of the watery solution, produces in a great measure the same effect as the evaporation of the solution at a higher temperature during a shorter period of time. It therefore became necessary to devise some means of producing a more rapid evaporation of these solutions without the application of artificial heat. This object was attained by means of a simple apparatus, in which a rapid current of air was made to pass over a large surface of the liquid to be evaporated, and which may be described in a few words.

The solution to be evaporated is poured into a dish or tray of block-tin about 16 inches square, with perpendicular sides 2 inches deep, and capable therefore of containing, when full, nearly two gallons of liquid. The dish is placed on a shelf fixed

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[*] From the Memoirs of the Literary and Philosophical Society of Manchester, vol. xiv.; read April 15, 1856. Communicated by the Author.

[†] Phil. Mag. vol. x. p. 73.

at a convenient height in a wooden box, of which *a b c d*, fig. 1, represents the front. This box is closed at the two sides, but open at the front and back from the shelf upwards. It must be sufficiently wide to allow the dish to slide easily in and out, but from front to back it must be so deep as to leave a space of about half an inch between the front and the dish. At the distance of about $1\frac{1}{4}$ inch from the back of the box, there is fixed in a perpendicular position a board *f*, the upper and side edges of which are firmly attached to the top and sides of the box. The lower edge of this board is about on a level with the upper edge of the tin dish, and is accurately fitted to a shelf, *g*, which is suspended by means of two upright pieces of wood, *h h*, $2\frac{3}{4}$ inches deep, resting on two ledges, *i i*, fixed to the sides of the box. The spaces between *h h* and the side walls of the box must be sufficiently wide to allow the sides of the tin dish to move easily up and down in them. By means of supports, *n n*, inserted between the tin dish *e* and the shelf *o*, the former may be raised so as to bring the surface of the liquid contained in it close to the shelf *g*, which is thus made to hang down within the dish. When the apparatus is to be used, the spaces *p p* left between the edges of the dish and the ledges *i i*, are closed as tightly as possible by means of flat plugs of wood, so as to cause the current of air passing through the apparatus to sweep over the whole surface

Fig. 1.

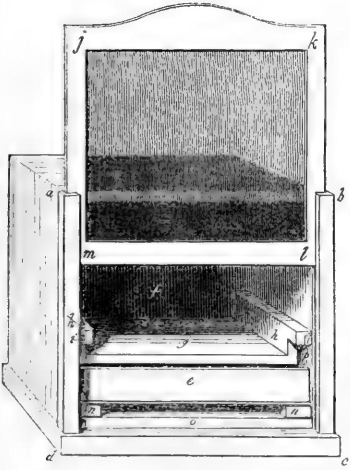
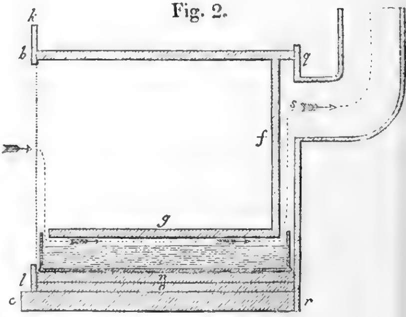


Fig. 2.



of the liquid, and the front of the box is closed by means of a frame, *j k l m*, covered with muslin and sliding up and down in grooves fixed to the sides, which in a great measure prevents the dust which is carried along by the current of air from being conveyed into the liquid. The apparatus is now placed so as to make the back fit as closely as possible to the wall, *q r*, fig. 2, in which there is an opening, *s*, communicating with a steam-boiler flue; or the back of the box may be closed with a piece of wood, having an opening communicating by means of a pipe with the flue. The section, fig. 2, shows the direction taken by the air in passing over the surface of the liquid. As the liquid evaporates, the dish is raised by means of additional supports so as again to bring the surface of the former close to the shelf *g*, and thus confine the current within a narrow space. The current of air which I employed, and which was sufficiently rapid to cause a constant ripple on the surface of the liquid, was produced by the draught of a steam-boiler flue, which carried away the products of combustion from several large fires. I think it probable, however, that the same effect might be produced by causing the whole of the air necessary for the supply of an ordinary stove or close fireplace to pass through the apparatus. By means of the current of air at my disposal, I was enabled to evaporate in this apparatus about one pint of water in the course of twenty-four hours at a temperature not exceeding 10° C., the temperature of the water being kept by means of the rapid evaporation rather lower than that of the atmosphere. The evaporation of a gallon of spirits of wine by the same means occupied only a few hours.

In preparing indican, the course of proceeding which I adopt is as follows*. The dried woad leaves are reduced to a powder,

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* In the course of the investigation I had an opportunity of confirming a statement made by the authors who have described the cultivation of the woad plant and the preparation of the dye made from it, viz. that the first crop of leaves obtained during the first year's growth of the plant is richest in colouring matter, and that each successive crop yields less than the preceding one. This may perhaps be ascribed to the lower temperature prevailing during the latter part of the year. Nevertheless if the roots be left in the ground through the winter, though the plant in the ensuing year seems to have lost none of its vigour, as may be seen by the size and abundance of rich glaucous leaves which it puts forth, and the quantity of flower-stems bearing numbers of flowers, and then of seeds which it sends up, still the leaves are as poor in colouring matter as those of the preceding autumn. The inferior quality of the dye produced from the second year's leaves, which in Thuringia went by the name of "Kompts-waid" (see Schreber's *Beschreibung des Waids*), was well known to the growers of woad in former times.

Woad is still employed by the woollen-dyers in this country, but what useful purpose it answers in preference to an equivalent quantity of indigo, I am unable to say. A specimen of the drug, as used by a woollen-dyer, which I examined, contained no trace of indigo-blue. If its use be merely to act as a ferment and reducing agent on the indigo employed at the same time, as is very probable, its place might be supplied by rotten cabbage-leaves or decaying vegetable matter of any kind.

which is passed through a hair-sieve, in order to separate the leaf-stalks and ribs of the leaves, and an extract of this powder is then made in a displacement apparatus with cold alcohol in the usual manner. The extract, which may be made stronger by passing it through fresh quantities of powder, has a lively dark green colour. It is evaporated in the apparatus just described, a little water being previously added to it, in order to facilitate the separation of the fatty matter. After a few hours, there is found at the bottom of the evaporating dish a dark green layer, consisting of fat and green colouring matter, covered by a light brown watery liquid. The latter is poured off, filtered, agitated with a quantity of freshly precipitated oxide of copper, and filtered again. It now appears of a dark green colour from oxide of copper in solution. The latter having been removed by means of sulphuretted hydrogen, the filtered liquid, which is now quite clear and of a light yellow colour, is evaporated again in the same apparatus, when it leaves a brown syrup. This syrup contains, besides indican, some products of decomposition of the latter. On being treated with cold alcohol, only a portion of it is dissolved, a part remaining undissolved in the form of a brown glutinous substance, which is a product of the combined action of water and oxygen on indican, and which will be more fully described below. The alcoholic liquid after being poured off is mixed with about twice its volume of æther, when it becomes milky and deposits a substance of a syrupy consistence, which contains an additional quantity of the body just referred to, and also some of the peculiar kind of sugar which is formed by the decomposition of indican. After the mixture has stood for several hours, there is usually found deposited on the surface of the syrup and attached to the sides of the glass a quantity of white crystalline needles, which also consist of a product of the decomposition of indican. After the æthereal liquid has become clear, it is poured on a filter and then evaporated as before, when it leaves a clear brown syrup, consisting of indican in as high a state of purity as I have been able to obtain it. The only impurity which may still attach to the indican as thus prepared, is a small quantity of fatty matter, the last traces of which it is extremely difficult to remove. When an alcoholic extract of woad is evaporated and water is added to the residue, the filtered liquid, though it may appear tolerably clear, still contains a quantity of fatty matter, in a state either of solution or, as seems more probable, of mechanical suspension. On adding acid to it, this fatty matter separates in greenish masses, which melt when the liquid is heated. The greatest part of this fatty matter is carried down by the oxide of copper used in the process just described, and the remainder is generally removed, when sulphuretted hydrogen is passed through the filtered liquid, by the precipitated sulphuret of copper. A little more separates occasionally in small white grains during the evaporation of the liquid filtered from the sulphuret of copper, especially if the temperature of the current of air passing over its surface be low, as in winter. If, however, the residue left after evaporation of the alcoholic extract be stirred up and agitated for some time with water, so large a quantity of fatty matter becomes suspended in the liquid as to render its separation without decomposition of the indican almost impossible, and it is for this reason that I have found it advisable always to add water to the alcoholic extract of the leaves before evaporation, and to pour off the watery liquid from the undissolved chlorophyll and fatty matter, instead of evaporating the extract by itself and then stirring up the residue with water.

I have very little to add to the description formerly given of indican and its properties. It is always obtained in the form of a transparent light brown syrup, and it cannot be separated from the water which it still retains in this state without decomposition. Its watery solution has a yellow colour and a purely bitter taste. Even in the highest state of purity in which it can be obtained, it produces when dissolved in water a slightly acid reaction on litmus-paper. Whether this reaction is peculiar to it, or whether it is a consequence of a commencing decomposition of this easily decomposable substance, I am unable to decide. After being prepared in the manner just described, it yields, when subjected in small quantities to the action of acids, indigo-blue, indirubine and sugar, with mere traces of other products of decomposition. When the same process, however, is performed on a somewhat larger scale, other products make their appearance from causes which I shall presently explain.

The new experiments which I have made to determine the composition of indican, confirm the conclusions at which I arrived in the first instance, and which are contained in the first part of this paper. Being unable to obtain the substance itself in a state fit for analysis, I was obliged, as before, to have recourse to its compound with oxide of lead. This compound was prepared in the following manner. Pure indican was dissolved in cold alcohol, and the solution was mixed with a small quantity of an alcoholic solution of acetate of lead and filtered from the precipitate, which was generally of a somewhat dirty yellow colour. On now adding to the liquid an excess of acetate of lead, a bright sulphur-yellow precipitate fell, which was collected on a filter, washed with alcohol and then dried, at first *in vacuo*, and then for a few hours in the water-bath. This precipitate was employed in the analyses I. and II. The liquid filtered from this precipitate gave with a little ammonia a second precipitate of a rather paler yellow colour, which was collected on a filter, washed with alcohol, and dried in the same manner as the first. The analyses III. and IV. were made with this precipitate. The following results were obtained:—

I. 1·0120 grm. burnt with oxide of copper and chlorate of potash, gave 0·7590 grm. carbonic acid and 0·1885 water.

1·0330 grm. burnt with soda-lime gave 0·0580 grm. platinum.

0·6385 grm. gave 0·5140 grm. sulphate of lead.

II. 0·8845 grm. gave 0·5040 grm. carbonic acid.

1·1100 grm. burnt with soda-lime gave 0·1260 grm. chloride of platinum and ammonium.

0·3490 grm. gave 0·3260 grm. sulphate of lead.

III. 1·3315 grm. gave 0·9380 grm. carbonic acid and 0·2535 water.

1·6295 grm. gave 0·2145 grm. chloride of platinum and ammonium.

0·8550 grm. gave 0·7005 grm. sulphate of lead.

IV. 0·9520 grm. gave 0·6345 grm. carbonic acid and 0·1715 water.

1·2490 grm. gave 0·0640 grm. platinum.

0·5335 grm. gave 0·4495 grm. sulphate of lead.

These numbers correspond in 100 parts to—

	I.	II.	III.	IV.
Carbon	20·45	15·54	19·21	18·17
Hydrogen	2·06	...	2·11	2·00
Nitrogen	0·79	0·71	0·82	0·72
Oxygen	17·47	...	17·58	17·12
Oxide of lead . .	59·23	68·73	60·28	61·99
	100·00		100·00	100·00

After deducting the oxide of lead, the relative proportions of the other constituents are expressed by the following numbers:—

	I.	II.	III.	IV.
Carbon . .	50·15	49·69	48·36	47·80
Hydrogen .	5·05	...	5·31	5·26
Nitrogen .	1·93	2·27	2·06	1·89
Oxygen . .	42·87	...	44·27	45·05
	100·00		100·00	100·00

These numbers conduct, as will be seen, to two different formulæ. The numbers of the first two determinations lead to the formula $C^{52} H^{31} NO^{34}$, those of the last two to the formula $C^{52} H^{33} NO^{36}$, as a comparison of the numbers found by experiment with those required by the respective formulæ will show.

	Eqs.	Calculated.		Eqs.	Calculated.	
Carbon . .	52	312	49·60	52	312	48·22
Hydrogen .	31	31	4·92	33	33	5·10
Nitrogen .	1	14	2·22	1	14	2·16
Oxygen .	34	272	43·26	36	288	44·52
		629	100·00		647	100·00

The first analyses which I made of the lead compounds of indican, the results of which are recorded in the first part of this paper, led to the formulæ $C^{52} H^{35} NO^{38}$ and $C^{52} H^{37} NO^{40}$, but I stated at the same time that neither of these could be considered as the true formula, since the compounds then analysed seemed no longer to contain unchanged indican. However, the compounds, the analyses of which have just been given, even after having been completely dried and decomposed with cold dilute sulphuric acid, yielded solutions which, after having been filtered from the sulphate of lead and boiled, deposited flocks consisting almost entirely of indigo-blue and indirubine, products indicating with certainty the presence of unchanged indican. Since indican exhibits a tendency, as I have before observed, to take up successively a number of equivalents of water, it is probable that of the two formulæ given above, the first, viz. $C^{52} H^{31} NO^{34}$, approaches nearest to, if it is not a correct representation of, its true composition. The formula $C^{52} H^{33} NO^{36}$ may then represent a mixture of indican with a small quantity of what may be called its hydrate, or it may show the composition of indican in the first stage of its hydration before it has lost the property of yielding indigo-blue by decomposition. As far as regards the explanation of the different processes of decomposition which the substance undergoes, it is of course immaterial which formula is adopted.

Action of Acids on Indican.

In the first part of this memoir I have given a general description of the process of decomposition which indican undergoes by the action of acids, and of the products thereby formed. I shall now proceed to give an account of the results obtained in a more minute investigation of this process, performed with larger quantities of material than had previously been at my disposal.

Sulphuric and muriatic acids are not the only acids capable of effecting the decomposition of indican. If to a watery solution of the latter, a small quantity of nitric acid be added, the quantity of the acid not being large enough to enable it to exert any oxidizing action on the indican, the solution immediately becomes green and turbid, and on standing it deposits flocks of a dark colour, while the surface becomes covered with a blue pellicle. The deposit is found to consist principally of indigo-blue with a little indirubine, and a trace of other products of decomposition. The filtered liquid, on being boiled, becomes muddy and deposits some brown flocks, which contain no indigo-blue. The quantity of indigo-blue formed by the action of nitric acid seems indeed to be comparatively larger than when sulphuric or muriatic acid is employed. It is hardly necessary to add, that if this be really the case, it cannot be ascribed to any oxidizing effect produced by the acid. A watery solution of indican, on being mixed with oxalic or tartaric acid and left to stand, yields a dark blue or purple deposit, consisting of indigo-blue and indirubine, which, when oxalic acid is employed, are remarkably free from other products of decomposition. The liquid filtered from this deposit yields in either case, when boiled, a few more flocks, and after being filtered, mixed with sulphuric acid and boiled again, it gives an additional quantity. These flocks contain indirubine and indiretine, but no indigo-blue. Even acetic acid produces a slight effect on indican. On adding that acid to a watery solution of the latter, the mixture deposits, on standing, some dark flocks, consisting of indigo-blue and indirubine, but their quantity is trifling.

A more minute examination of this process of decomposition showed that it was more complicated, and that the products formed by it were more numerous than I had at first imagined. The products of decomposition which I have observed are of three kinds. The first are insoluble in water, and are deposited in the shape of powder or flocks from the acid liquid; the second remain dissolved in the latter; the third are volatile, and are obtained by distilling the liquid either whilst the action of the acid is proceeding or after it has ceased. For the purpose of preparing these various products, I found it to be necessary to obtain indican in a state of absolute purity by successive solution in alcohol, water and æther; for though some indican is always decomposed when its watery solution is evaporated, the substances into which it is thereby converted afford, by decomposition with acids, products which do not differ in their nature, but only in their relative proportions from those which are formed when perfectly pure indican is employed. I therefore contented myself with extracting the dry leaves of the woad plant with cold alcohol, evaporating the extract in the apparatus above described, adding water to the residue and filtering. The solution of indican thus obtained was mixed with a considerable quantity of sulphuric acid, and the green fatty matter precipitated by the acid was separated by filtration. The action of the acid passed, as I invariably observed, through two distinct stages, and I found it convenient to collect and treat the products formed at each stage of the action separately. The filtered liquid, though clear at first, soon became opalescent and muddy, and deposited dark flocks, while the surface became covered with a blue pellicle. After the liquid had stood in the cold for about twenty-four hours, this deposit usually ceased to be formed, and the action then entered on its second stage, which was manifested by the separation from the filtered liquid of a brown powder, the quantity of which was much increased by heating. This powder contained little or no indigo-blue, but some indirubine, and a large quantity of other products of decomposition. I think it probable that the first deposit owed its origin to the pure indican contained in the solution, while the second was formed from indican that had undergone a change by the action of water. The matter insoluble in water formed by the action of acid having been collected on a filter, the acid liquid was employed for the preparation of the other products of decomposition in a manner to be hereafter described. The portion of the products insoluble in water was also obtained by another method, still more expeditious than the one just described. The leaves of the plant having been finely chopped, boiling water was poured over them, and the mixture having been well stirred, the liquid was strained through calico and mixed with sugar of lead. This produced a pale green precipitate which was separated by filtration, and the liquid having been mixed with an excess of sulphuric acid, was filtered from the sulphate of lead and heated for some time, when it produced a deposit containing the same products as before. Instead of sulphuric acid, I sometimes employed nitric acid, avoiding however, in this case, the use of heat. More indigo-blue and less of the other products of decomposition seemed to be formed when nitric acid was used.

In whatever manner the products insoluble in water were obtained, I always adopted the same method of treatment for the purpose of separating them from one another, a method which is indeed essentially the same as that employed by Berzelius for the separation of the constituents of crude indigo. The whole of the acid used in the process having been carefully removed by means of cold water, the mass left on the filter was treated with dilute caustic soda. This dissolved a great portion, forming a dark brown opake liquid, which was filtered from the insoluble matter. The latter was treated again with caustic soda, the action being now assisted by heat, and the process was repeated until nothing more was dissolved. The liquid, on being mixed with an excess of muriatic acid, let fall a voluminous flocculent precipitate of a brown colour, which, after being collected on a filter and washed with water, was treated with a boiling mixture of alcohol and ammonia. This sometimes dissolved the whole of it, sometimes only a part. The insoluble portion, when there was any present, had the appearance of a dark brown powder, and consisted of the body to which I have given the name of *Indihumine.* After having been treated repeatedly with alcohol and ammonia until nothing more was dissolved, and then with muriatic acid, and lastly washed with water, it was considered pure. The alcoholic liquid filtered from it was dark brown. On adding to it an excess of acetic acid, an abundant dark brown deposit was formed, consisting of a substance which I had not previously observed, and to which I propose to apply the term *Indifuscine.* It was collected on a filter, washed first with alcohol, then with hot water until all the acetate of ammonia and acetic acid were removed, and lastly agitated with a little cold alcohol, filtered off and dried, when it had the appearance of a dark brown or reddish-brown powder. By repeating the process of solution in alcohol and ammonia and precipitation with acid, its further purification was effected. The alcoholic liquid filtered from the indifuscine was mixed with an alcoholic solution of acetate of lead, when an additional quantity of the same substance was precipitated in combination with oxide of lead in brown flocks. The filtered liquid, containing an excess of sugar of lead, was mixed with ammonia, which gave a brownish-yellow precipitate, consisting chiefly of indiretine in combination with oxide of lead. This precipitate, after being filtered off, was treated with dilute acetic acid, which removed a considerable quantity of oxide of lead, and after being again filtered off and washed, it was completely decomposed with boiling dilute muriatic acid. The indiretine which was separated collected in the boiling liquid in the form of brown, half-fused masses, which were separated by filtration while the liquid boiled, washed with boiling water, and then treated with a small quantity of cold alcohol. The alcohol acquired a dark brown colour, and after being filtered from a little undissolved indifuscine, was evaporated to dryness, when it left the indiretine in the shape of a brittle resinous residue, which was purified by being again dissolved in cold alcohol.

That part of the product of the action of acids insoluble in caustic soda was usually of a dark bluish-purple colour. It was treated with boiling alcohol until nothing more dissolved. The alcoholic liquid, which had a dark brownish-purple colour, was filtered boiling hot from the insoluble portion, consisting chiefly of indigo-blue, and then mixed with ammonia and an alcoholic solution of acetate of lead, which gave a brown precipitate consisting of oxide of lead in combination with indifuscine, and such other products as had not been completely extracted by the caustic soda. After being filtered from this precipitate, the liquid appeared of a beautiful purple colour. It was mixed with an excess of acetic acid and distilled or evaporated to about one quarter of its original volume, and then mixed with a large quantity of water, which precipitated the whole of the matter dissolved in it in the shape of dirty purple flocks. These flocks were collected on a filter, well washed with water, and then treated with dilute caustic soda, which generally, however, only dissolved a minute portion of them. After being again filtered off and well washed, they were dried and treated with a small quantity of cold alcohol. The alcohol dissolved a portion, forming a solution of a deep reddish-yellow colour, which was filtered and evaporated, when it left a shining resinous substance of the same colour, which, as it possesses characteristic properties and a peculiar composition, I shall call *Indifulvine.* By dissolving it in weak spirits of wine it was separated from a little impurity, which remained undissolved in the shape of a brown powder. The matter left undissolved by the cold alcohol consisted chiefly of indirubine. For the purpose of purifying this body, I availed myself of the property which it possesses, in common with indigo-blue, of dissolving in caustic alkalies in the presence of bodies which easily take up oxygen. On treating the mixture containing indirubine with a solution of protoxide of tin in caustic soda and boiling, I obtained a solution which, after being rapidly filtered, deposited indirubine on exposure to the air in the shape of a reddish-purple pellicle covering its surface. This pellicle, on being broken, fell to the bottom in thick flakes and was succeeded by another. As soon as the whole of the indirubine contained in it had been again oxidized and deposited, it was filtered off, well washed with water and dissolved in boiling alcohol. The alcoholic solution, which had a beautiful purple colour, generally left on evaporation a dark brown amorphous residue, consisting of indirubine in as high a state of purity as I have been able to obtain it when formed by the decomposition of indican. A brown powder was left undissolved by the alkaline solution of protoxide of tin, which, after being again treated with a fresh quantity of the same solution in order to dissolve all the indirubine which might be contained in it, was washed with water, then with acid, washed again with water, dried and treated with cold alcohol. The latter dissolved a second portion of indifulvine, which seemed to have escaped the solvent action of the alcohol in the first instance, in consequence of its having been so intimately mixed with and enveloped by particles of indirubine as not to be reached by the alcohol. The alcohol still left undissolved a quantity of brown powder, which did not seem to be any peculiar substance, but an intimate mixture of indifulvine and indirubine. The indigo-blue left undissolved by the boiling alcohol was purified by treating it, according to Fritzsche's method, with a warm solution of grape-sugar in alcohol to which caustic soda was added, and allowing the mixture to stand in a warm place until the indigo-blue was dissolved. The yellow solution having been drawn off with a siphon and allowed to stand exposed to the air, became first red, then purple, and then deposited the indigo-blue in the shape of small crystalline scales, which were collected on a filter and washed first with alcohol, afterwards with boiling water, then digested with muriatic acid, well washed with water and dried.

The bodies insoluble in water formed by the action of acids on indican are therefore six in number. I shall now give an account of their properties and composition.

Indigo-blue.

The indigo-blue obtained by this process has all the properties usually ascribed to that substance. It is insoluble in alkaline liquids, but dissolves easily when a deoxidizing substance, such as a salt of protoxide of tin or protoxide of iron, or grape-sugar, is added at the same time, the solution exhibiting the usual appearance of an indigo vat, such as the yellow colour, and the blue pellicle on the surface. It is only slightly soluble in boiling alcohol, to which it communicates a blue tinge, but easily and completely soluble in concentrated sulphuric acid, forming a blue solution from which nothing is precipitated on the addition of water. By the action of boiling nitric acid it yields indigotic acid, and when treated with a strong boiling solution of caustic soda, it is converted into an acid having the properties of anthranilic acid. Its identity with indigo-blue is, however, placed beyond doubt by its analysis, which yielded the following results:—

I. 0·3365 grm. dried at 100° C. and burnt with oxide of copper and chlorate of potash, gave 0·8955 grm. carbonic acid and 0·1305 grm. water.

0·5175 grm. burnt with soda-lime gave 0·3775 grm. platinum*.

II. 0·3605 grm. gave 0·9605 grm. carbonic acid and 0·1350 water.

0·5230 grm. gave 46 cubic. centims. of moist nitrogen at a

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* The double chloride which yielded this amount of platinum was washed according to Hofmann's directions, with æther, to which a little alcohol was added, instead of with the usual mixture of alcohol and æther. It weighed 0·9095 grm., which if it had consisted of the double chloride of platinum and ammonium alone, would have corresponded to 0·0571 grm. of nitrogen, or 11·03 per cent. The apparent excess arose without doubt from the presence of aniline.

temperature of 12°C. and a pressure of 736·8 millims., equivalent to 42·7 cubic centims. of dry nitrogen at 0° C. and a pressure of 760 millims. or 0·0534 grm.

Hence was deduced the following composition:—

	Eqs.		Calculated.	I.	II.
Carbon . .	16	96	73·28	72·57	72·66
Hydrogen .	5	5	3·81	4·30	4·16
Nitrogen .	1	14	10·68	10·36	10·22
Oxygen . .	2	16	12·23	12·77	12·96
		131	100·00	100·00	100·00*

Indirubine.

This substance, when obtained by the process above described, usually appears in the form of a dark brown amorphous mass. On one occasion, when very pure indican had been employed in its preparation, it was deposited from the boiling alcoholic solu-

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* The analyses above given lead to a composition more nearly approaching the theoretical one than any previously on record, with the exception of those given by Laurent (*Ann. de Chim. et de Phys.* ser 3, vol. iii. p. 372), which were made with sublimed indigo-blue. To the use of the latter for this purpose, however, objections may be raised on account of the difficulty of separating it from particles of carbon and traces of oily and resinous matters formed by the sublimation. Dumas in his last memoir on the composition of indigo-blue (*Ann. de Chim. et de Phys.* ser. 3, vol. ii. p. 207), proved that the excess of carbon in the previous analyses of the substance was only apparent, being caused by the admixture of a little sulphur derived from the sulphate of iron which is generally used for its purification. Having carefully removed this sulphur, he obtained in three analyses, 72·90, 72·84, and 72·97 per cent. of carbon, which correspond apparently with the theoretical composition. These amounts are, however, calculated according to the old atomic weight of carbon. If corrected in accordance with the new atomic weight of carbon, which was established by Dumas a short time previously, they become respectively 71·89, 75·77, and 71·92, the great excess in the second determination being probably due to some misprint. On analysing some specimens of the indigo-blue remaining from his previous investigation, which he himself had proved to be impure, and calculating the results according to the new atomic weight of carbon, Dumas obtained in four analyses, 73·3, 73·5, 72·7, and 73·3 per cent. of carbon. The coincidence between these and the previous analyses is of course only apparent. I have myself always found a deficiency in the amount of carbon, unless care was taken to wash the precipitated indigo-blue for a considerable time. I ascribe this circumstance to the indigo-blue, like all porous bodies, combining with certain substances and removing them from their solutions in consequence of an attraction of surface exerted by it. If, for instance, grape-sugar is employed in its purification, a certain quantity of it is carried down by the indigo-blue, and can only be removed by continuous washing with hot water, followed by treatment with muriatic acid and renewed washing with water. Each portion of water is found to leave on evaporation a small quantity of syrup, and this does not cease until the washing has been continued for several days.

tion on cooling in long crystalline needles, which were red by transmitted light. The alcoholic solution has a fine purple colour. It is perfectly insoluble in alkaline liquids; but if it be treated with a boiling solution of caustic soda, to which some deoxidizing substance, such as protochloride of tin or grape-sugar, is added, it dissolves with ease, just as indigo-blue does under the same circumstances, forming a solution from which it is again deposited in purple flakes by the action of the atmospheric oxygen. It dissolves in concentrated sulphuric acid in the cold, forming a purple solution, which on the addition of water gives a dark precipitate, the supernatant liquid remaining of a fine purple colour. It is decomposed by boiling nitric acid. On being heated between two watch-glasses, it produces on the upper glass a sublimate consisting of beautiful purple needles which dissolve in boiling alcohol, forming a fine purple solution, which on cooling deposits crystalline needles. This sublimate seems to consist, not of any product of decomposition formed by heat, but of the substance itself, which when freed from all impurities possesses the property of crystallizing.

The quantity of indirubine which I obtained, even when operating on large quantities of indican, was so exceedingly small, that I was unable to apply any means for effecting its further purification.

I was, however, enabled by chance to procure from another source a sufficient quantity of the substance for an examination of its properties and composition. Some time before commencing my investigation of the woad plant, I had obtained from India a quantity of the dried leaves of the *Indigofera tinctoria*, for the purpose of ascertaining the state in which the colouring matter is contained in them. Though the leaves reached me as soon as possible after having been gathered and dried, their examination led to no definite results, the process of fermentation by which the colouring matter is formed having probably been already completed, and I therefore laid them aside. Their peculiar greenish-purple colour and the glaucous appearance of their surface, which resembled that of glazed green tea, showed however that they must contain, ready formed, some peculiar species of colouring matter. I was therefore induced to examine them again, and this examination led to the conclusion that their colour was caused by a thin coating of a substance which was, there could be little doubt, identical with indirubine. This substance was isolated by the following process.

Having prepared a liquid containing protochloride of tin dissolved in an excess of caustic soda, the leaves were immersed in it while boiling. The boiling was continued until the leaves had lost their purple tinge and become pale green. The green muddy liquid was then strained as quickly as possible through canvas, and left to stand exposed to the air in a shallow vessel. The surface of the liquid soon became covered with a purple pellicle, which was carefully skimmed off and was succeeded by another, which was in its turn removed, the process being repeated as long as anything formed on the surface. The purple matter was then dissolved a second time in an alkaline solution of protoxide of tin, and the solution was again left exposed to the atmosphere. The pellicle which was formed by the action of the oxygen was removed this time by means of blotting-paper, to which it adhered without much of the liquid underneath being removed with it. The substance was separated from the paper by agitation in water, collected on a filter, treated with boiling caustic soda to dissolve a little adhering fatty matter, filtered off again, washed with acid, then with water, and lastly dissolved in boiling alcohol. The alcohol acquired a splendid purple colour, and on cooling deposited a quantity of crystalline needles, consisting, as I believe, of indirubine in a state of purity. When thus prepared it is found to have the following properties.

It crystallizes from its alcoholic solution in small needles, forming when dry a silky mass of a colour between purple and chocolate, which, on being rubbed with a hard body, shows a slight metallic lustre resembling that of bronze. When heated on platinum foil it emits red vapours, then melts and burns with a yellow smoky flame, leaving some charcoal. When carefully heated between two watch-glasses it gives a yellowish-red vapour, resembling that of bromine, which condenses on the upper glass in the form of beautiful long crystalline needles. These needles are plum- or garnet-coloured; they possess a somewhat metallic lustre, which is however much inferior to that of sublimed indigo-blue, and seem to consist simply of the original substance, which has been volatilized without change. When the process of sublimation is carefully conducted, only a trace of carbonaceous residue is left. It dissolves completely in concentrated sulphuric acid in the cold, forming a solution of a beautiful purple colour. This solution when heated does not become black, but on the contrary rather paler, and evolves only a trace of sulphurous acid. When mixed with water it gives no precipitate and retains its fine purple colour, which does not disappear or become weakened when the acid is neutralized with carbonate of soda, but soon vanishes entirely when an excess of caustic soda or ammonia is added. The solution in sulphuric acid after dilution with water imparts a fine purple colour to cotton, wool, and silk. When treated with nitric acid of ordinary strength, indirubine begins to dissolve even in the cold, and to a greater extent on the application of heat, forming a purple solution, which on being further heated becomes red, and on boiling, yellow. The whole of the substance is dissolved without leaving any resinous residue, such as is always left when indigo-blue is treated with nitric acid, forming a clear yellow solution. This solution leaves on evaporation a residue which dissolves only partially in hot water. A brown resinous substance is left undissolved by the latter, and the liquid filtered from this is bright yellow and very bitter, and yields, when mixed with carbonate of potash and evaporated, crystals, apparently of picrate of potash, which detonate when heated. Very dilute nitric acid also decomposes and dissolves it on boiling, but its decomposition is effected with far more difficulty than that of indigo-blue by the same means. In like manner a boiling solution of bichromate of potash, mixed with sulphuric acid, which easily decomposes indigo-blue, seems to have very little effect on it even when the boiling is continued for a considerable time. When suspended in water and exposed to the action of a stream of chlorine gas, it loses its colour very slowly, and is changed into a brown resinous substance containing chlorine, which melts in boiling water and is easily soluble in alcohol, but does not crystallize when the solution is evaporated. Like the indirubine from indican, it is quite insoluble in alkaline liquids, but dissolves easily when a deoxidizing agent, such as grape-sugar or a protosalt of iron or tin, is added at the same time. If it be treated for instance with a solution of protoxide of tin in an excess of caustic soda it dissolves rapidly, forming a yellow solution, the surface of which on exposure to the air instantly becomes covered with a film of regenerated indirubine, the appearance being exactly like that of an indigo vat, except that the film floating on the surface is purple instead of blue. If a piece of calico be dipped into the solution and then exposed to the air it acquires a purple colour, which is not removed either by acids or soap. This colour has no great intensity, but by working on a larger scale, it is probable that shades of purple equal in depth to those produced by indigo-blue might be obtained. When the solution is mixed with an excess of muriatic acid, it gives a dirty yellow precipitate, which after filtration and exposure to the air slowly becomes purple. By long-continued exposure of the solution to the atmosphere the whole of the indirubine dissolved in it is again deposited as a purple mass, which is sometimes found to consist of small crystalline needles. When heated in a tube with soda-lime, the substance emits fumes having a smell like that of benzole and an alkaline reaction, which condense on the colder parts of the tube to a sublimate, consisting partly of oil and partly of crystalline needles. It is not precipitated from its alcoholic solution by acetate of lead, even when ammonia is added at the same time.

These reactions seem to me to prove the identity of this body with the indirubine from indican, which, if it could be entirely freed from all impurities, would no doubt exhibit the same property of crystallizing and of volatilizing without residue*.

The behaviour of indirubine towards concentrated sulphuric acid and towards alkaline solutions of deoxidizing substances so much resembles that of indigo-blue towards the same reagents, as to lead one to expect a great similarity in the composition of the two bodies, even if the fact of their being formed from the same parent substance by the same process of decomposition were unknown. The quantity of pure indirubine which I obtained from the leaves of the *Indigofera* was only sufficient for a general examination of its properties, and for one analysis; which showed, however, if it be permitted to draw a positive conclusion from one determination, that it has exactly the same elementary composition as indigo-blue,—that the two substances are isomeric. The following are the numbers yielded by the analysis:—

0·3185 grm., dried at 100° C. and burnt with oxide of copper and chlorate of potash, gave 0·8500 grm. carbonic acid and 0·1195 water.

0·2021 grm. gave 49·3 cubic centims. of nitrogen at a temperature of 10°·5 C. and a pressure of 269·5 millims., equivalent to 16·81 cubic. centims. at 0° C. and a pressure of 760 millims. or 0·2122 grm†.

In 100 parts it contained, therefore,—

Carbon	72·78
Hydrogen	4·16
Nitrogen	10·50
Oxygen	12·56
	100·00

[To be continued.]

VI. *On the Electrical Silure.* By M. du Bois-Reymond‡.

As long ago as the year 1855, spirit-specimens of the electrical Silure were sent to Edinburgh amongst other remarkable natural objects, by the Scotch missionaries from Creek Town

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* When dry woad-leaves are extracted with cold alcohol, the sides of the glass vessel containing the extract generally become covered with patches of small red crystals, which seem to consist of pure indirubine. They are insoluble in caustic alkalies, but soluble in boiling alcohol, the solution depositing, on cooling, crystals exactly like those above described.

† I owe this determination to Professor Frankland, who had the kindness to perform it according to his own method of analysis.

‡ From the *Bericht der Akad. du Wissenschaften zu Berlin,* August 13, 1857, p. 424.

in Western Africa, on the Old Calabar river, which discharges itself into the Bay of Benin in latitude $5\frac{1}{2}°$ N., and longitude 8° W. The fish was described by Mr. Andrew Murray of Edinburgh as a new species under the name of *Malapterurus Beninensis*[*]. A lady, the wife of a missionary, notwithstanding her having been shipwrecked, has now succeeded in bringing three living specimens of the same species to Edinburgh. Professor Goodsir of Edinburgh, who is now travelling through Germany, has had the extraordinary kindness to bring one of these three fishes to Berlin and to place it at the disposal of Prof. J. Müller, who has handed it over to me for experiment.

The fish was the smallest, but the most lively of the three; it measures about 15 centimetres (6 inches) in length. It travelled here by Leith and Rotterdam, with a few aquatic plants in an ordinary gold-fish globe, which was placed in a suitable covered basket. Since Saturday last it has been kept at the Anatomical Museum in a larger vessel with aquatic plants and water from the Spree, and appears to be very comfortable. It seeks the dark, and is very quiet. They have not succeeded with certainty in feeding it. The spirit-specimens examined in Edinburgh had the remains of freshwater crustacea in their intestines; and the living fishes immediately chased similar pelagic forms which were offered to them, and appeared, although they allowed them to escape again at first, to have finally eaten them. Accordingly, we are endeavouring to feed our Silure with the small crustacea of this country, such as *Gammarus, Asellus, Daphnia,* &c.

For the present it appears advisable to call forth the electrical power of the fish as little as possible. I have therefore hitherto confined myself to the examination of the most important point now feasible, namely to ascertain the distribution of the electric tensions, which is still unknown.

According to the concurrent statements of many observers, at the moment of the shock in the *Torpedo,* the dorsal surface of the organ is positive, and the ventral surface negative; that is to say, the current passes in the organ from the belly to the back, and in the surrounding water, or a curved conductor applied to the two surfaces, from the back to the belly[†].

Of the *Malapterurus* of the Nile, we have recently had a description elaborated with all the modern aids, by M. Bilharz, a German naturalist living in Cairo[‡], so that in a morphological

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[*] Edinburgh New Philosophical Journal, New Series, vol. ii. pp. 49, 379; and vol. iii. p. 188. British Association Report, 1855, p. 114.

[†] Experimental Researches in Electricity, Ser. XV. Nov. 1838. Nos. 1761, 1764.

[‡] *Das electrische Organ des Zitterwelses anatomisch beschrieben,* &c. Leipzig, 1857, folio.

point of view this is perhaps the best known of all electromotive fish. By the introduction of the idea of the *electrical plate,* M. Bilharz has in all probability secured the credit of having first attained to a clear view of the essential structure of an electrical organ. In a physiological point of view, on the other hand, nothing more has hitherto been known of the *Malapterurus* than what was known to Adanson no less than 106 years ago, namely that it gives an electrical shock*. The procuring of living specimens in Cairo appears to be attended with almost insurmountable difficulties, which have been feelingly described by M. Markusen in a communication to the Academy of St. Petersburg†. M. Diamanti of Cairo, a pupil of M. Matteucci, is the only person who, some years ago, was permitted for a time to investigate living electrical Silures, and this by the special favour of the Viceroy of Egypt‡. His results, however, have not been published, and were even unknown to M. Bilharz, so that the latter in his memoir has endeavoured to come to a conclusion as to the distribution of the tensions occurring in the organ of the *Malapterurus* from anatomical grounds.

Thus Pacini's *prolungamenti spiniformi* on the hinder negative surface of the electrical plates of the organ of *Gymnotus* §, are regarded by M. Bilharz with great probability as nerve-tubes, which immerse themselves in the plates. In the *Torpedo,* the nerves would also pass to the lower negative surface of that structure which is indicated by M. Bilharz as the electrical plate. Now as in the organ of the *Malapterurus* the nerve-tubes pass to the hinder surface of the electrical plates, Bilharz concludes that in this fish, as in the *Gymnotus,* the head-end is positive, and the tail-end negative, so that the current in the organ will be directed from the tail to the head.

It will be easily seen what great interest now attached to the exact examination of the shock of the *Malapterurus,* in consequence of this opinion of M. Bilharz. The experiments were made this morning in the presence of MM. Goodsir, J. Müller, G. Wagener, and Paul du Bois-Reymond. The fish was placed in a shallow, cylindrical glass jar of 15 centims. (about 6 inches) in diameter, which was filled with water to a depth of about 45 millims. (nearly 2 inches). For conducting the current I made use of two metallic saddles placed upon the fish,—the same pro-

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* *Reise nach Senegall, übersetzt von Martini.* Brandenburg, 1773, p. 201.

† *Bull. Phys. Math. de l'Acad. de St. Pétersbourg,* vol. xii. p. 203, 1854.

‡ Markusen, *op. cit. supra,* p. 208; and Bilharz, *op. cit. supra,* Preface, p. vi.

§ *Sulla Struttura intima dell' Organo elettrico del Gimnoto e di altri Pesci elettrici,* &c. Firenze, 1852, pp. 16, 21.

cess as that adopted by Faraday with the *Gymnotus*. They consisted of strips of strong platinum-foil, 5 millims. in breadth, bent into the form of the transverse section of the fish, and were 55 millims. in length for the thick anterior part, and 45 millims. in length for the thinner caudal extremity. To these strips copper wires coated with gutta-percha were soldered, serving as insulating handles; the soldered parts were carefully covered with varnish. The wires were connected with the ends of a multiplier of 550 convolutions, with heavy double needles. The platinum saddles in Spree water produced no effect upon this multiplier. Lastly, in accordance with Galvani's arrangement for the *Torpedo*[*], a prepared frog was so placed upon the edges of the glass jar, that it could not but betray by its convulsions every discharge of the fish.

When the saddles were placed upon the head and tail of the fish, the frog was convulsed, and the needle flew round the dial in a direction which indicated *the head to be negative and the tail positive, or a current in the organ from the head to the tail, and in the surrounding water and the wire of the multiplier from the tail to the head.* The experiment was repeated again with the same result. I ascertained that the magnetism of the needles had undergone no perceptible alteration.

M. Bilharz's prediction consequently has not proved true. The current in the *Malapterurus* has a direction opposite to that in the *Gymnotus*. If a pile of the organ of the *Torpedo*, in order to become one of the organ of the *Gymnotus*, must bend forwards with its upper end, in order to become a column of the organ of the *Malapterurus*, it must lean the same end backwards.

VII. *On the Reciprocal Action of Metals and the Constituents of Well- and River-waters.* By Henry Medlock, *Esq.*[†]

II. *The Action of Iron.*

In a paper published in the September Number of the Philosophical Magazine, I pointed out the peculiar action of certain constituents of water upon lead. When nitrogenous organic matter is contained in the water of a stream, a portion of the nitrogen is in time converted into ammonia. Part of this ammonia in the ordinary course of oxidation is converted into nitrous acid, which combines with an equivalent of ammonia and forms nitrite of ammonia. The nitrite thus produced may be, and probably is, converted by time into nitrate of ammonia.

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[*] *Memorie sulla Elettricità animale .... al celebre Abate Lazzaro Spallanzani,* &c. Bologna, 1797, iv. p. 74.

[†] Communicated by the Author.

Nitrate of ammonia in contact with the lead of cisterns forms quadribasic nitrite of lead, with evolution of binoxide of nitrogen. The binoxide of nitrogen is converted into nitrous acid, which combines with the ammonia, forming again nitrite of ammonia. The quadribasic nitrite of lead is decomposed by the carbonic acid of the atmosphere. Three equivalents of the oxide are precipitated as carbonate of lead, while the monobasic nitrite of lead remains in solution. This monobasic nitrite in contact with the metal forms again quadribasic nitrite, and by the decomposition of water, ammonia.

Assuming, as we justly may, that decaying organic matter and its products of decomposition are present in the water supplied to our houses, that the active products of decomposition consist chiefly of ammonia and nitrous acid, and remembering the energetic and continuous action of nitrous acid upon lead, we have a ready and easy explanation of the rapidity with which water so circumstanced acts as a powerful solvent of lead*.

The mutual reactions of the organic matter of water and lead appear, as far as my experiments allow me to offer an opinion, to occur in the following order. By the action of the metal, and partly by other influences, the nitrogen of the organic matter is converted into ammonia.

The ammonia in contact with the metal is partially converted into nitrous acid, which combines with another atom of ammonia and forms nitrite of ammonia.

The nitrite of ammonia by further oxidation may be converted into nitrate of ammonia.

Nitrates in contact with metallic lead form quadribasic *nitrite* of lead, with evolution of binoxide of nitrogen.

The binoxide of nitrogen is instantly converted into nitrous acid.

Nitrous acid attacks the lead and forms again quadribasic nitrite of lead. The quadribasic nitrite is decomposed by the carbonic acid of the atmosphere, three equivalents of the oxide are precipitated as carbonate, while monobasic nitrite of lead remains in solution.

Monobasic nitrite of lead in contact with metallic lead decom-

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* The accidental omission of a sentence in my last paper renders my meaning liable to misconstruction. The sentence as printed stands as follows:—"Thus, although the quantity of nitrogenous organic matter in water may be very small, and the nitrous acid *directly* resulting from the oxidation of ammonia may be in very minute quantity, yet it will be evident from the foregoing remarks that the quantity will rapidly increase from a process of self-manufacture which is constantly going on." After the words "foregoing remarks that," ought to have been inserted the words "with the daily accession of more putrescible organic matter to our cisterns" the quantity, &c.

poses water, and forms again quadribasic nitrite of lead and ammonia.

In the decomposition of the quadribasic nitrite of lead, much of the monobasic nitrite is carried down with the precipitated carbonate of lead*. As there is a constant daily accession of new organic matter in a state of decomposition to our cisterns, and as similar changes to those above described are constantly going on, and may under special circumstances be suddenly increased, it will be evident that the use of leaden cisterns exposes us to the danger of having lead dissolved in our water in doses which may sometimes rise to poisonous proportions.

Having established, as I believe by most conclusive experiments, the remarkable reaction which takes place between lead and the organic matters contained in water, I was induced to study more fully some singular phænomena I had previously noticed, which become apparent when water remains for a few hours in contact with a large surface of *iron*.

In September 1856, I was requested by the Directors of the Amsterdam Water Company to analyse several samples of water from the city of Amsterdam. Complaints of the water had been made by many of the inhabitants. The water possessed a peculiar *fish-like* odour; and although perfectly transparent and colourless, and free from the slightest chalybeate taste, it was found, after standing, to deposit a reddish-brown sediment, which coated every vessel in which the water was allowed to remain for a few hours.

The service-pipes being of iron, the deposit was assumed to consist of the sesquioxide, resulting either from the decomposition of a protosalt of iron in solution, or from the attrition of the iron pipes through which the water is conveyed.

I may here observe that Amsterdam is supplied with water from the "Duins" (sand-hills) in the neighbourhood of Haarlem. Trenches five miles in length have been dug on the sides of the hills to intercept the rain-fall. The water is conveyed to a large reservoir, whence it flows to the filter-beds at the works, and there filtered through 4 feet of filtering medium, consisting of pebbles, shells, and fine sand. After filtration it is pumped up an iron stand-pipe, 150 feet high, and conveyed thence by iron mains to Amsterdam, a distance of eighteen miles.

Five samples of the water were sent to me for analysis; one from the works before the water came in contact with iron, the other four from various stand-pipes in the city.

In the water from the works, *before* it came into contact with

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* In investigating the nitrites and hyponitrates of lead, I found the carbonate of lead precipitated by carbonic acid from any of the basic salts, to require long-continued washing to remove every trace of the soluble neutral salt.

iron, the quantity of iron-oxide, alumina, and phosphates (precipitated by ammonia) amounted to 0·95 gr. per gallon. In the four other analyses of the water, after it had passed through twenty miles of iron pipes, the quantities were *reduced* in the several determinations to 0·09 gr., 0·07 gr., 0·07 gr., and in the fourth sample to an unweighable trace. Thus, instead of taking up an additional quantity of iron-oxide from the mains, the water was found actually to have lost nearly the whole of that which it previously held in solution. But notwithstanding the almost entire precipitation of the iron actually in solution from the water which had passed through iron pipes, this water formed the objectionable red deposit on standing; while the water from the works holding in solution nearly half a grain of iron-oxide per gallon *formed no such deposit*. Therefore it was *à priori* to be assumed that the red deposit could not consist of oxide of iron, and consequently it appeared necessary to examine this deposit very carefully. The precipitate from ten gallons of water was collected in a porcelain capsule, evaporated to dryness in a water-bath, and heated subsequently in an air-bath to 248° F. (120° C.) until it ceased to lose weight. On ignition, the precipitate *charred* and was almost entirely consumed, leaving only a minute quantity of ash, consisting of silica, alumina, iron-oxide, and carbonates of lime and magnesia. The small quantity of this deposit at my command did not permit me to make a quantitative analysis thereof; but having found that the deposit consisted almost entirely of organic matter, a portion of it separated from one gallon of water was submitted to a microscopic examination, and found to consist of the filaments of dead and decaying algæ, confervæ, and other microscopic plants, in various stages of decay, and varying in colour from green through pale yellow, orange, red, brown, dark brown to black. Many of the fibres retained a perfectly organized structure, and in some the spores were distinctly visible. Amongst these plant-remains were numerous siliceous tubes, which had probably invested the delicate filaments of the algæ.

It appeared important to determine the *cause* of the precipitation of the organic matter from the water which had been in contact with iron; and in the hope of elucidating this interesting question, the Directors of the Company requested me to proceed to Amsterdam to inspect their works. As anticipated, the Duin canals were found to be choked up with aquatic plants; and at the sides, and wherever the current was sluggish, were large masses of dead algæ floating on the surface. The sand at the bottom, originally very white and pure, was blackened to some depth by decaying vegetable matter, and when disturbed, numerous bubbles of fetid marsh-gas escaped.

The water had the peculiar fish-like smell of the masses of "flab" (*Ulva intestinalis*) floating on the surface. The stones on the sides of the canals, the hurdles placed to keep up the sand, floating pieces of stick, and indeed every *solid* body with which the water came in contact, was coated with a reddish-brown deposit similar to that which was found to separate from the filtered water in Amsterdam. *This deposit was particularly abundant near the iron sluice-gates.* A pipe from one of the mains in Amsterdam was examined at the same time, and found to be coated on the inside with a thick slimy deposit of vegetable remains, which presented under the microscope an appearance similar to that of the deposits above alluded to.

Remembering the important fact established by Schönbein, that copper and platinum in a finely-divided state have the property of converting ammonia into nitrous acid, and having myself demonstrated that lead, in any form*, possesses a similar power, it occurred to me that if the same property were possessed by iron, it would throw a light upon the fact of the separation of organic matters from waters which remain in contact with that metal. Schönbein states that finely-divided copper and platinum convert ammonia instantaneously, at the common temperature, into nitrous acid, while "iron requires to be heated to a dull redness" before it will effect a similar change. I have found, however, that on placing several coils of iron wire in a bottle and adding a few drops of strong ammonia, a slight elevation of temperature takes place, and after an hour nitrous acid may be detected by the acidulated starch-paste and iodide of potassium test. I tried also the action of iron which had been reduced from the oxide by hydrogen, and found that it *instantly* converted a portion of the ammonia into nitrous acid.

In order to study the action of iron upon the Amsterdam water, I submitted to analysis a quantity of this water which had never been in contact with iron, and at the same time I immersed several coils of iron wire in two gallons of the same water. The water in which the iron wire was placed was perfectly bright and free from colour, and had nothing visible in mechanical suspension except a few of the siliceous tubes above alluded to. The experiment was made in four open glass bottles.

A series of remarkable phænomena were observed. In five minutes a faint white cloud was seen radiating from the coil of iron wire and descending in the form of a parachute. In half an hour the cloud increased in density, and the water became opalescent. In six hours the cloud assumed a brownish colour, and there was a brown deposit at the bottom of each of the

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* Lead pyrophorus dropped into a bottle containing the vapour of ammonia mixed with air, instantly produces nitrite of ammonia.

bottles. The cloud continuing to increase during forty-eight hours, assumed more and more a flocculent character and a reddish colour, exactly like the deposit from the Amsterdam water which had flowed through iron pipes. The water was now filtered through fine Swedish filter-paper, submitted to analysis, and a portion of the deposit examined under the microscope. The latter consisted of the remains of algæ, each separate flock having a siliceous tube as a nucleus. The remainder of the deposit was burned, and the ash found to consist of iron-oxide, silica, and the carbonates of lime and magnesia.

The following are the analyses of the water before and after being in contact with iron:—

	I. Water from the Duin canals.	II. The same water which had been in contact with iron for forty-eight hours.	
Sulphate of lime	3·70	3·68	grs. per gal.
Chloride of sodium . . .	4·10	4·01	”
Carbonate of lime	5·60	3·20	”
Carbonate of magnesia . .	3·97	1·30	”
Silica	0·16	trace	
Iron, alumina and phosphates.	0·95	trace	
Matter volatile on ignition .	2·10	trace	
	20·58	12·19	

By these analyses, it is seen that the 2·10 grs. of organic matter originally contained in the water *was decomposed or thrown down by contact with iron.* The water thus purified no longer possessed the unpleasant fish-like taste and odour which had marked its previous contamination.

A portion of the water which had been in contact with iron wire was rendered slightly alkaline by pure alcoholic potassa, evaporated to dryness, and the residue tested for nitrous acid. The paper became instantly *blue.* In the residue of the water which had not been in contact with iron, and still containing its organic matter, no indications of nitrous acid could be obtained. Thus it appears that iron possesses the property of producing nitrous acid from the nitrogen of organic matter, or from the ammonia existing in the water. This nitrous acid, as appears from experiments subsequently to be detailed, seems to act the part of a *scavenger* in the water, oxidizing every particle of putrescent or putrescible matter which may be contained therein.

The water of the Thames, and indeed that of nearly all other rivers, contains a large quantity of decaying organic matter derived from the plants and animals abounding in the streams, from the surface-drainage of richly manured and highly cultivated lands, and, more objectionable still, from the sewage and refuse of towns. The complex organic matters derived from these several sources are by certain influences, and in course of time, resolved into more simple compounds which are comparatively innocuous. Thames water, as is well known, if allowed to stand in an open vessel for a few days in warm weather, acquires a very offensive odour, arising from the decomposition of the animal and vegetable matter held in solution. This water, notwithstanding, is considered by maritime authorities to be the very best for the use of ships' companies. On board-ship the water is stored in wooden casks, which are stowed low down in the hold. During the first week or fortnight, according to the temperature of the atmosphere, the water undergoes a kind of fermentation, evolves a quantity of gas possessing a most offensive odour, and deposits a copious brown sediment. The water gradually ceases to smell offensively, becomes bright and sparkling, and will then keep fresh and sweet for an indefinite length of time. In other words, it loses by a natural process of decay, *the whole of its putrescent impurities*. In our vessels of war and most of our large steamers, tanks of *iron* have been substituted for casks of wood for storing the water necessary for the use of the passengers and ships' company. I am informed that in iron tanks Thames water *evolves no offensive gases*, but becomes pure much quicker than when stored in wood, and deposits a more copious brown sediment which turns red on exposure to the air. The gases produced in the tightly-bunged wooden casks are said to be slightly luminous in the dark, and to explode when a lighted candle is incautiously placed near the bung-hole when the bung is removed.

The gases arising from the decomposition of the organic matter contained in Thames water, possess an odour *far more offensive* than even that of sulphuretted hydrogen, of phosphuretted hydrogen, or of sulphide of ammonium.

The well-known influence of the gaseous exhalations from stagnant waters in producing malarious diseases, renders it highly probable that the gaseous products of the decomposition of organic matter possess *poisonous* qualities, and the true character and composition of which have hitherto eluded the most delicate chemical tests.

Having discovered that when the Amsterdam water was allowed to remain in contact with a large surface of iron for forty-eight hours, the *whole* of its organic matter was precipitated, together with part of the mineral constituents, I thought it desirable to study the action of iron upon the organic matters contained in Thames water, since it promised to furnish a means of removing the one great objection to this water for domestic purposes, namely its *organic impurities.*

In my previous paper, already referred to, I pointed out, that, from the united influence of heat and contact with the copper of the still, *nitrite of ammonia* was produced during the distillation of Thames water. From analogy, it was assumed that iron would possess the same power of converting the nitrogen of the organic matter, or the ammonia resulting therefrom, into nitrous acid. With the view of deciding this important question, I made comparative experiments on the Thames water, as supplied by the Grand Junction Company to my laboratory, before and after being exposed to the action of a large surface of iron. The method adopted of testing for nitrous acid was as follows:—The residue of each gallon of water was introduced into a Florence-flask furnished with a cork and funnel-tube. In the neck of the flask was placed a slip of bibulous paper moistened with starch-paste and iodide of potassium. Ten cubic centimetres of water were added to each flask to cover the bottom of each of the funnel-tubes. To each flask were now added 10 cubic centims. of dilute sulphuric acid (made by mixing 10 cubic centims. of pure HO,SO^3 with 90 cubic centims. of water). No indications of nitrous acid were in either experiment obtained, since the paper in each flask remained perfectly white*. Six other gallons of water, drawn at the same time, were placed in open glass bottles, and to each gallon was added *two ounces* of coiled iron wire. Each bottle was left during forty-eight hours. Phænomena were observed similar to those noticed in the case of the Amsterdam water, but the deposit was of a *browner* colour. After filtration the water was found to be perfectly bright, and as colourless as distilled water. Each gallon was separately evaporated to dryness, and the residues tested, as above described, for nitrous acid:—

Exp. I. Paper became blue in ten minutes, and intensely blue in one hour.
Exp. II. Paper blued in five minutes.
Exp. III. Paper blued in four minutes.

As it appeared probable that the nitrous acid liberated by the sulphuric acid might remain in solution, and in consequence not instantly affect the test-paper, in experiments with the residues of the other three gallons of the water which had been in contact with iron, I used *hot* water and an acid of double strength. In each experiment the test-paper became blue in a few seconds.

These experiments prove, I believe, beyond a doubt, the pro-

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* These experiments were made in very cold weather.

duction of *nitrous acid* by the action of iron upon the nitrogenous compounds contained in the water.

The brown deposit from a gallon of Thames water was collected in a porcelain capsule, dried at 248° F. and weighed. It amounted to 4·4352 grs., and consisted of—

Organic matter	22·92	per cent.
Sesquioxide of iron	65·36	,,
Carbonate of lime	11·11	,,
Carbonate of magnesia	traces	
Silica	0·59	,,
	100·08	

A second deposit produced by iron in four gallons of water was collected in a similar manner, and dried in an air-bath at 248° F. It weighed 14·2450 grs., and lost on ignition 24·31 per cent. of volatile matter.

To ascertain the actual effect produced by iron, I made a series of determinations of the solid residue and organic matter contained in Thames water before and after being placed in contact with iron. Four separate determinations of solid residue and organic matter in the water which had not been in contact with iron, were made on four consecutive days in January last with the following results:—

	Solid residue from one gallon.	Organic matter in the residues.
	grs.	grs.
I.	20·867	2·248
II.	19·573	1·740
III.	21·159	1·424
IV.	20·127	1·401
Average	20·4315	1·7032

Having found that iron promotes the precipitation of organic matter from Thames water, it appeared desirable to ascertain the *quantity* of iron and the length of time necessary to effect the entire destruction or precipitation of the organic impurities contained in it. To accomplish this I instituted the following experiments:—

1st. In a gallon of water was immersed one coil of fine iron wire weighing half an ounce. It was allowed to stand two hours, and the water then filtered; when filtered and evaporated and the residue dried in an air-bath at 248°, till it ceased to lose weight, it gave,—

Solid residue	17·3712	grs.
Including matter volatile on ignition.	1·2320	,,

2nd. A gallon of water with one ounce of iron wire for two hours gave,—

Solid residue	16·6936	grs.
Including matter volatile on ignition.	1·1704	„

3rd. A gallon of water with eight ounces of iron wire for two hours gave,—

Solid residue	13·1824	grs.
Matter volatile on ignition	0·9548	„

At the time these experiments were made (January 9, 1857), the water was somewhat discoloured; and as it evidently varied daily in composition, it appeared necessary, in order to ascertain the actual effects of the iron, to determine at the same time the solid residue and organic matter existing in the water when the above three experiments were made.

Two separate determinations were made (one gallon for each).

I.	Total solid residue	21·4522	grs.
	Including matter volatile on ignition.	3·2494	„
II.*	Total solid residue	20·3742	„
	Including matter volatile on ignition.	2·7566	„

Giving a mean of—

Total solid residue	20·9132	„
Matter volatile on ignition	3·0020	„

From these determinations it is clearly shown that one gallon Thames water, by contact with half an ounce of iron wire for two hours, lost,—

Mineral matter	3·5420	grs.
Volatile matter	1·7700	„

By contact with one ounce of iron wire for two hours, it lost—

Mineral constituents	4·2196	grs.
Volatile matter	1·8316	„

By contact with eight ounces of iron wire for two hours, it lost—

Mineral matter	7·7308	grs.
Volatile matter	2·0472	„

When the residue which is left on evaporating Thames water is ignited in a platinum dish, I have noticed that the *remains* of the organic matter mixed with the bulk of the residue at the *bottom* of the dish burn off in a few seconds, while at the *sides*

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* When the water for this analysis was drawn, a fresh supply was entering the cisterns from the mains.

there is frequently a black residue which is extremely difficult to burn off, even by very lengthened ignition.

This difficultly combustible carbonaceous matter appears to occur only in waters which are mixed with the sewage of towns, since in spring-waters I have never observed it. When the evaporated residue of urine is burned, it leaves with the mineral constituents a quantity of black, lustrous carbon, which requires an intense heat, long continued, before it is entirely consumed. As the carbonaceous matter left with the ignited residue of Thames water bears a remarkable resemblance to that of urine, it is probably in this common admixture with Thames water we must seek the origin of the almost incombustible carbon which remains with the ignited residue.

The residue of Thames water, when dissolved in a dilute acid, evolves a large quantity of *sulphuretted hydrogen*. The organic matter is commonly supposed to reduce the sulphates to sulphides, and these of course evolve sulphuretted hydrogen when acted upon by a dilute acid. Having frequently noticed the large quantity of sulphuretted hydrogen evolved from the solid residue of Thames water, I tested the residues of six separate gallons of water by placing them in Florence-flasks, and adding to each 10 cubic centims. of water, and then, through funnel-tubes, dilute hydrochloric acid. The slips of paper moistened with acetate of lead previously put into the necks of the flasks, *became instantly black*, and the quantity of sulphuretted hydrogen evolved was sufficient to taint the entire atmosphere of the laboratory. The residues of three other gallons of water which had been in contact with iron wire for twenty-four hours, when similarly treated, *gave no indication of sulphuretted hydrogen.*

The white cloud produced in Thames water, after it has been in contact with iron for from five minutes to an hour (according to the temperature of the day), bears a remarkable resemblance to finely-divided *sulphur*; but the quantity even from ten gallons of water, when tested, was too small to permit me to pronounce it to be sulphur with positive certainty, although a portion of it, when burned, evolved an odour resembling that of sulphurous acid, and another portion, when boiled with strong alcoholic potassa, and tested with lead, gave a dark-brown coloration.

With the view of determining whether sulphides are usually present in Thames water, I evaporated six separate gallons, covering each dish over with white blotting-paper, moistened here and there with a drop of dilute solution of acetate of lead. As soon as the water became warm, every lead spot was slightly discoloured from the evolution of sulphuretted hydrogen or a volatile sulphuret. Three other gallons of water were now placed in contact with iron wire (two ounces of iron wire in each gallon), and allowed to remain for two hours. As in former experiments, the white cloud made its appearance. At the end of two hours, the water was filtered through fine white sand (paper allowing the precipitate to pass through) till it became perfectly bright. These three gallons were afterwards evaporated in a manner similar to the preceding, and gave not the slightest indications of sulphuretted hydrogen or a volatile sulphuret.

To each of several Winchester quarts of distilled water, was added a cubic centimetre of strong hydrosulphuret of sulphide of ammonium. Several coils of iron wire, weighing two ounces, were put into each bottle and allowed to remain. In a few minutes a white cloudy precipitate formed; at the end of twelve hours the water was filtered through fine sand, and in the clear filtrate no indications of the sulphide could be obtained, the sulphide of ammonium having been *entirely decomposed* by contact with iron.

By the kindness of Mr. Quick, engineer of the Southwark and Vauxhall and Grand Junction Water Companies, I have had an opportunity of trying the action of iron upon Thames water on a much larger scale than is possible in a laboratory. A brick and cement tank was constructed capable of holding 1600 gallons. In this was suspended, by means of cross-bars, twelve pounds of iron wire. In an hour, the water, previously very bright, became opake and muddy, and in twenty-four hours there was a dark-coloured flocculent precipitate, consisting of organic matter, oxide of iron, silica, and the carbonates of lime and magnesia. The water was now filtered through three feet of the usual filtering medium (coarse and fine sand). It came through perfectly bright and colourless. Eight gallons of this water were reserved for analysis, and at the same time eight gallons of the filtered water from the stand-pipe were taken for comparison. An analysis of each was made under precisely similar circumstances, with the following results. It may be observed, that the residues were dried for the same length of time, and at the same temperature (248° F.).

No I. is the water from the works filtered through sand, and as supplied to the mains.

No. II. The same water, before filtration, in contact with iron for twenty-four hours (twelve pounds of iron wire to 1600 gallons of water).

	No. I. Not treated with iron.	No. II. Treated with iron.	
Lime	7·280	6·173	grs. per gal.
Magnesia	0·624	0·543	„
Iron oxide	0·027	trace	
Chlorine	1·052	1·047	„
Sulphuric acid	2·343	5·302	„
Silica	0·624	0·277	„
Matter volatile on ignition	2·126	1·108	„
Phosphoric acid	traces	traces	
Sodium	0·684	0·678	„
Potassium	0·321	not determined	
Ammonia	traces	none	
Sulphuretted hydrogen	distinct traces	none	
Nitrous acid	none	traces	
Nitric acid	traces	traces	
Total solid residue obtained by direct evaporation	21·8753	18·295	

These analyses exhibit, at first sight, comparatively little difference in the quantities of mineral constituents, but on examining them carefully, we find that in the ordinary water there are, in addition to a large quantity of organic residue, distinct traces of *sulphuretted hydrogen*, while in the water which has been in contact with iron, *no traces of this poisonous gas exist*. Moreover, in the ordinary Thames water, there are marked traces of ammonia, while in the water treated with iron, no trace of this base remained, the ammonia having been converted into nitrous acid. The great increase in the quantity of sulphuric acid, from 2·343 grs. to 5·302 grs. per gallon, is very remarkable, and probably is owing to the oxidation of sulphuretted hydrogen, or sulphurets pre-existing in the water. This, however, requires further investigation, and a series of experiments I am now engaged in, will, I hope, determine this question satisfactorily. In the above analyses, for obvious reasons, I have substituted for the indefinite term "organic matter," the expression "matter volatile on ignition."

From the residue of one gallon of the water, 2·126 grs. were expelled by ignition, while from the residue of one gallon of the water which had been in contact with iron, only 1·108 gr. was volatilized. Moreover, the residue of the first water became nearly *black* when heated, while the residue of the second was scarcely altered in colour, except at the edges, notwithstanding that it lost a little more than a grain by the ignition. This loss *was not organic matter*, as no charring was observed, but was due in all probability to the volatilization of the chlorides of magnesium and sodium* and decomposition of nitrates.

As the spontaneous development of vegetable and animal life in Thames water is one of the surest indications of the presence of decaying organic matter, I allowed two separate gallons of the water which had been treated with iron, to remain exposed to the air and light during two months in the summer. At the end of that time, *not the slightest signs of either vegetable or animal life were observable*; but the bottles containing two gallons of the ordinary water, similarly exposed, were filled with a green vegetation, which afforded a refuge for innumerable animalculæ. This fact appears to offer, perhaps, the best proof of the partial or entire destruction, by iron, of the putrescible organic matter contained in Thames water. As the influence of iron upon the organic matter and the sulphurets contained in water appears to vary very considerably according to the temperature of the atmosphere and the season of the year, I have made some additional experiments on the action of a much larger surface of iron upon a given quantity of water. Although iron in the form of wire exposes a *less surface* to the water than it would in many other forms, I have nevertheless found it more convenient to use the wire.

Two analyses of the Grand Junction Company's water were made on the 20th and 21st of October, and the results of the first analysis were,—

Solid residue (dried at 248° F.) . .	20·1247	grs. per gal.
Including matter volatile on ignition .	1·3456	„

In the second analysis,—

Solid residue (dried at 248° F.) . .	20·2530	grs. per gal.
Including matter volatile on ignition .	1·4871	„

Half a gallon of the water taken on the 20th of October was put into a bottle containing 2½ ounces of iron wire and allowed to remain for forty-eight hours. A copious brown deposit was formed. The water was filtered and evaporated down in an open porcelain dish to about four fluid ounces. The deposit formed on boiling was *perfectly white*. It was transferred to a platinum dish, evaporated and dried in the usual manner, and then ignited. The analysis gave,—

Total solid residue	11·4391	grs. per gal.
Including matter volatile on ignition.	0·2063	„

Hardness of the water by Clarke's scale, 6°·2

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* The intense yellow colour imparted to the gas-flame during the ignition of the residue of Thames water, is a proof that the chloride of sodium is partly volatilized.

A second quantity of water was placed in bottles with the same quantity of iron and allowed to remain for forty-eight hours. When filtered and evaporated, the residue, like the preceding, was perfectly colourless, and when weighed gave,—

Total solid residue	10·1384 grs. per gal.
Including matter volatile on ignition.	0·1321 „
Hardness	6°

From these experiments, it appears that by submitting water to a *large surface* of iron, and allowing it to remain in contact for a longer period, nearly every particle of organic matter, whatever may have been its origin, is *entirely removed from the water*. The trifling loss on ignition (0·2 gr. per gallon) is evidently due to the volatilization of those mineral constituents which have been proved to volatilize at a red heat.

Keeping in view the facts above stated, we shall be enabled to perceive that the method of analysis hitherto adopted for determining the fitness of a water for domestic purposes, is in several respects *defective*. According to the latest and most approved method of analysing a water to ascertain its dietetic value, a given quantity is evaporated in an open porcelain dish, over a gas-flame, nearly to dryness. The residue is transferred to a small dish of platinum, and evaporated to perfect dryness on a water-bath. It is now heated in an air-bath at a temperature of 248° F. (120° C.), to expel the last traces of moisture and the water of hydration belonging to sulphate of lime. The residue is weighed, and the result is recorded as the weight of the "total solid residue." The dish containing the residue is now ignited over a gas-flame, to burn off the "organic matter." The residue is moistened with a few drops of solution of carbonate of ammonia to replace any carbonic acid which may have been expelled from the lime by the ignition. It is dried again in an air-bath at the same temperature as before, and weighed several times till the weight remains constant. The loss of weight occasioned by the ignition, is assumed to represent the quantity of "organic matter" originally present in the water. In the residue are determined the proportions of silica, iron, alumina and phosphates, lime, and magnesia.

A second portion of water is evaporated in a similar manner, and in this are determined the chlorine and sulphuric acid. A third portion is evaporated, and in this are determined the alkalies. Finally, the acids and bases are recorded as associated together in the order of their respective affinities, and the analysis is considered complete.

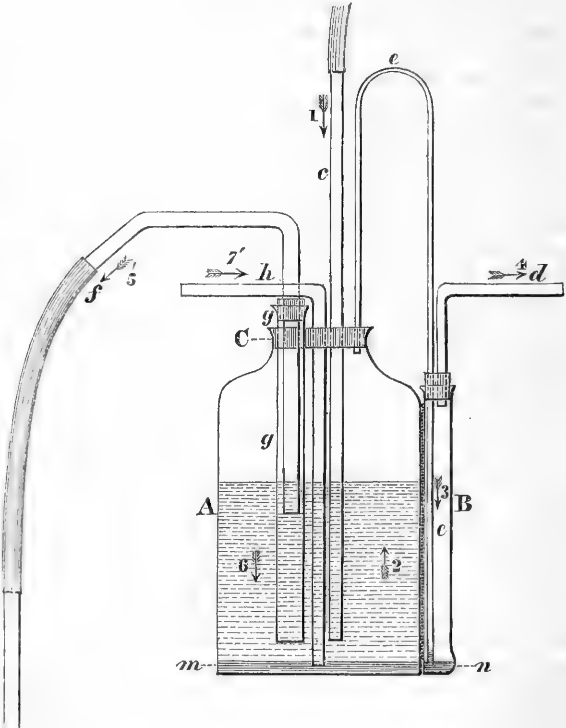
It is obvious that this method of analysis does not determine with certainty the amount of putrescible organic matter originally present. The constituents of Thames water which are supposed to be a fertile source of disease, *escape from the water even at the common temperature,* and it follows as a positive certainty, that during the evaporation of the water in order to obtain the solid residue, the greater part, and probably the whole of those constituents which are inducive of disease, are *expelled with the steam.* The organic matter *remaining* with the mineral constituents of the water, is small in quantity and of an *inert* character. I therefore regard it as unimportant, except so far as it may serve in some measure as an index of the quantity of malarious matter originally present in the water; but the loss of weight experienced on igniting the residue of a gallon of Thames water is not even a *reliable* index of the quantity of organic matter actually *in the residue,* since other constituents present are volatile at a red heat, and in consequence untrustworthy results are obtained.

In selecting a water for the supply of a town, providing the water be not what is called a decidedly *hard* water, the relative quantities of mineral salts are of little consequence; but the question *whether it contain putrescible organic matter, and if so, in what quantity, and of what nature, is the first* to be determined, and its paramount importance is universally recognized. A water containing such matters is supposed very justly—indeed we have abundant evidence—to *promote,* if it does not *originate,* malarious diseases in populations condemned to drink it; such populations, as is well known, being much more prone to certain zymotic maladies, than those who enjoy a supply of water which is free from putrescible organic contaminations.

By allowing Thames water to remain in contact with a large surface of iron for twenty-four or forty-eight hours, the *whole* of the putrescent impurities appear to be removed. This remarkable action of iron appears to be due in a great measure to the formation of nitrous acid, which oxidizes and destroys every particle of destructible matter which may be present in the water. The experiments I have made, and of which the preceding pages afford but an imperfect outline, have occupied me more than twelve months. I am now engaged in making an extensive series of experiments on *large* quantities of Thames water, in the hope of contributing to the elucidation of the malarious gases which escape spontaneously, and which are, in consequence of their volatile character, expelled with the steam in the ordinary methods of analysis. The results of these experiments, which are approaching completion, I hope shortly to communicate.

VIII. *On a new Form of Spirator.* By Frederick Guthrie*.

THE following form of spirator will be found useful in establishing a current of air for an indefinite length of time, by means of a continuous current of water. It may be employed either as aspirator in drawing, or as expirator in forcing air through an apparatus. Its action will be readily understood by referring to the accompanying diagram.



A is a wide-mouthed 12-ounce bottle, whose bottom is covered with a few millimetres of mercury, *m*. Through its well-fitting cork C, four holes are bored. Into the first of these, the wide

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* Communicated by the Author.

tube open at both ends, g is fitted, so that its lower end is a little above the mercury, m. The upper end of g is provided with a cork, through which the siphon-tube f passes. In the second hole of C the bent tube h is fixed, in such a manner that its lower end dips beneath the surface of the mercury, m. Through the third hole of the cork C a straight tube c is passed, whose office is to convey water into the bottle A: the lower end of c is a little above the mercury, m. Through the fourth hole in C a narrow tube e is fastened, whose longer exterior limb dips under the surface of the mercury n in the tube B. B is a small test-tube, widened at the bottom in three directions, and containing a few millimetres in depth of mercury, n. It is fastened with sealing-wax to A, and is provided with a cork, the one orifice of which admits the tube e; through the other passes the bent tube d.

The lower extremity of the tube f being placed far down a sink and a gradual stream of water being allowed to enter by c, as indicated by (1), the water in A rises (2), the air is driven out of A (3), bubbles through the mercury n, and passes by d (4) through the apparatus employed. During this filling of A with water, the mercury m prevents the retrograde flux of water through h. After A is filled, the water rises in the tubes f and e until it reaches the top of f, when this tube acts as a siphon (5′), and being wider than the ingress-tube c, gradually empties the bottle A (6). To supply the place of this water the air must enter by h (7′), and may thus be drawn through the apparatus in use, while the mercury n in B prevents the regression of the air through e.

The instrument has of course a simpler form when required to act only as an aspirator, for then the tube B and its appendages may be dispensed with.

When an increased resistance has to be overcome (the instrument being used either as aspirator or as expirator), the tube f is drawn further out of the tube g.

This form of spirator I have found to be certain in its action; for with a continuous stream of water, the volume of air which passes through is in definite proportion to the volume of water employed; and, indeed, the slower the stream of water the more nearly equal are these volumes, for in this case the amount of water passing through c during the action of the siphon f is inconsiderable.

IX. *Notices respecting New Books.*

A Handbook of Chemical Manipulation. By C. GREVILLE WILLIAMS. London: John Van Voorst. Post 8vo. Pp. 580.

THE importance of skilful manipulation in several of the branches of natural science, and especially in chemistry, can perhaps scarcely be overrated; yet we have sometimes noticed in students a tendency to cultivate it to the neglect of those mental powers, without a due exercise of which mere manipulatory skill becomes little better than a pleasing accomplishment. Like mathematics to science, manipulation should be to chemistry a skilful handmaid, whose mechanical aid, although of great value, must not be allowed to supersede or degrade those higher powers of induction which stand in need of her assistance. It is well known that Dalton was one of the clumsiest of manipulators; his operations were performed in the crudest manner, and his experimental results were, for the most part, far from falling within that narrow circle around perfect accuracy which we usually assign as the boundary of the errors of experiment. But although the great results achieved by the almost unaided intellect of Dalton may serve to point out the true place of manipulation, yet it cannot be doubted that those results would have been still more brilliant, had that intellect been associated with manipulatory skill.

We welcome, therefore, Mr. Williams's book as a valuable contribution to a department of chemical literature which has long been neglected; Faraday's admirable work on chemical manipulation, the only one that ever appeared in our language, having been long out of print. It is singular, but perhaps not wholly inexplicable, that whilst the study of other departments of this science has of late years been much facilitated by numerous treatises, this important branch should have received so little aid from the same quarter. The truth is, that the clear and successful description of the manipulatory portion of any science is much more difficult than is commonly imagined. To write a really useful book on chemical manipulation, an author must unite with lucidity of style, an extensive personal experience in a wide range of chemical operations, qualifications which are not often met with in the same individual.

Mr. Williams's book is written in a terse and intelligible manner, and his descriptions, even of complicated apparatus and operations, are for the most part remarkably clear and comprehensible; consequently the student, who has not the advantage of a teacher's guidance in his manipulations, will especially find this 'Handbook' an invaluable companion; whilst even those who enjoy regular instruction may frequently consult it with advantage in devising the arrangement of apparatus for particular processes. The illustrations inserted in the text are numerous and excellent; some of them are from photographs, an application of photography which we are glad to see gradually coming into use. Illustrations thus made have many of the advantages of drawings executed to scale, and can be at once placed in the hands of an intelligent workman, when the construction of any complicated apparatus is required.

The book opens with a chapter on the arrangement of an experimental laboratory, full of information, most valuable not only to those who have to improvise their work-room in a dwelling-house or other previously-erected building, but also to the more fortunate chemist who can build a laboratory specially adapted to the purposes of chemical research. Then follow chapters on furnaces, lamps, blowpipe apparatus, baths, heat-measurers, the balance, specific gravity, solution, precipitation, &c.,—all containing in a condensed form much useful matter which could only otherwise be found scattered over the pages of numerous bulky volumes.

A very important feature in the 'Handbook' is the devotion of considerable space to the description of certain classes of operations employed in chemical research. The prosecution of investigations, especially in the organic department of the science, has of late years developed certain experimental methods, which from their convenience and wide application have become of paramount importance to the investigator. In this branch of his subject Mr. Williams has given excellent chapters on "Volumetric manipulation," "Gas manipulation," and "Pressure-tube operations." Under the first-named title, the manipulatory methods necessary to the performance of the volumetrical operations devised by Mohr, Bunsen, Liebig and others are described; whilst the chapter on gas manipulation contains descriptions of the apparatus employed in gaseous research by Bunsen, Regnault and Frankland. This chapter is full of most useful information respecting a department of chemical inquiry which is daily becoming more important: we could have wished, however, that the author had devoted more space to the description of the methods of the first-named chemist, to whom this branch of manipulation is so deeply indebted, and whose processes, from the simplicity of the apparatus required, and the ease with which the experimenter may construct it for himself, are more likely to be employed by the student than the more expensive instruments of Regnault and Frankland, which, although affording considerable advantages to the experienced operator, are not so well adapted for the beginner in gas manipulations.

In the chapter on pressure-tube operations we conceive that the author has slightly erred in the opposite direction. He has clearly and fully described all the simpler forms of apparatus used in these important operations, but he has altogether omitted to mention the more convenient though expensive contrivances, which have recently come into use among those chemists who have extensively employed these processes in their researches.

With these slight defects, to which we only direct attention in order that they may be remedied in a future edition, we most cordially recommend the book as an invaluable companion in the laboratory.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

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

May 7, 1857.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—

"On the part which the Silicates of the Alkalies may play in the Metamorphism of Rocks." By T. Sterry Hunt, Esq., of the Geological Survey of Canada.

In my last communication to the Royal Society on the Metamorphic Silurian Strata of Canada, I endeavoured to show, from the results of analyses of the altered and unaltered rocks, that it is the reaction between the siliceous matters and the carbonates of lime, magnesia, and iron of the sedimentary deposits, which has given rise to the serpentines, talcs, pyroxenites, chlorites, and garnet rocks of the formation. I then cited the observation of Bischof that silica, even in the form of pulverized quartz, slowly decomposes these carbonates at a temperature of 212° F., with evolution of carbonic acid; the same author mentions that a solution of carbonate of soda has the power of dissolving quartz under similar conditions*. Desiring to verify these observations, I have since made the following experiments.

Colourless crystalline quartz was ignited, finely pulverized, and then boiled for an hour with a solution of its weight of perfectly pure carbonate of soda; the amount of silica thus dissolved was 1·5 per cent. of the quartz, but on repeating the treatment of the same quartz with a second portion of the carbonate, only ·35 per cent. was dissolved. The object of this process was to remove any soluble silica, and the quartz thus purified was employed for the following experiments, which were performed in a vessel of platinum.

I. 1000 parts of quartz and 200 of carbonate of soda were boiled with water for ten hours, and the mixture was several times evaporated to dryness, and exposed for a few minutes to a temperature of about 300° F. The amount of silica taken into solution was 12 parts.

II. A hydrocarbonate of magnesia was prepared by mingling boiling solutions of sulphate of magnesia and carbonate of potash, the latter in excess; the precipitate was washed by boiling with successive portions of water. 1000 parts of quartz were mixed with about as much of this magnesian carbonate and boiled as above for ten hours. An excess of hydrochloric acid was then added, the whole evaporated to dryness, and the magnesian salt washed out with dilute acid. The residue was then boiled for a few minutes with carbonate of soda, and gave 33 parts of soluble silica.

III. A mixture of 1000 parts of quartz, 200 of carbonate of soda with water, and an excess of carbonate of magnesia was boiled for ten hours, and the residue, treated as in the last experiment, gave 148 parts of soluble silica. The alkaline liquid contained a little mag-

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* Bischof's Chem. and Phys. Geology, Eng. Edition, vol. i. p. 7.

nesia but no silica in solution. That the soluble silica was really combined with magnesia was shown by boiling the insoluble mixture with sal-ammoniac, which, dissolving the carbonate, left a large amount of magnesia with the silica. This silicate was readily decomposed by hydrochloric acid, the greater part of the silica separating in a pulverulent form.

The third experiment was suggested by some observations on the reactions of silicate of soda with earthy carbonates. Kuhlmann has remarked the power of carbonate of lime to abstract the silica from a boiling solution of soluble glass*, and it is known that alumina exerts a similar action. I have found that when artificial carbonate of magnesia in excess is boiled with a solution of silicate of soda, the latter is completely decomposed with the formation of carbonate of soda, and a silicate of magnesia which gelatinizes with acids; and I have long since described this reaction in the evaporation of alkaline mineral waters†. This mutual decomposition of carbonate of magnesia and silicate of soda, conjoined with the power of carbonate of soda to dissolve silica, leads to a curious result. If we boil for some hours a mixture of ignited silica, obtained from the decomposition of a silicate by an acid (and consequently readily soluble in an alkaline carbonate), with a small portion of carbonate of soda and an excess of hydrocarbonate of magnesia, we obtain a dense powder which contains all the silica united with magnesia, and may be boiled with carbonate of soda and sal-ammoniac without decomposition. It is obvious from the above experiments that similar results may be obtained with quartz, although the process is much slower; it would doubtless be accelerated under pressure at a somewhat elevated temperature, which would enhance the solvent power of the alkaline carbonate.

Silicates of potash and soda are everywhere present in sedimentary rocks, where decomposing felspathic materials are seldom wanting, and these salts in the presence of a mixture of quartz and earthy carbonates, aided by a gentle heat, will serve to effect a union of the quartz with the earthy bases, eliminating carbonic acid. A small amount of alkali may thus, like a leaven, continue its operation indefinitely and change the character of a great mass of sedimentary rock. Such a process is not only a possible but a necessary result under the circumstances supposed, and we cannot, I think, doubt that alkaline silicates play a very important part in the metamorphism of sedimentary rocks, which are composed for the most part of earthy carbonates, with siliceous, aluminous, and felspathic materials‡.

The direct action between the carbonates and silica must necessarily be limited by their mutual insolubility, and by the protecting influence of the first-formed portions of earthy silicate; but with the solvent action of a small portion of alkali which is changed from

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* Comptes Rendus de l'Acad. des Sciences, Dec. 3rd and Dec. 10th, 1855, where will be found many important observations on the alkaline silicates.

† Reports of the Geol. Survey of Canada, 1851–53–54.

‡ It is well known that small portions of alkalies are seldom or never wanting in the earthy silicates, such as serpentine, talc, pyroxene, asbestus, epidote, idocrase, and even beryl and corundum. See the memoir of Kuhlmann already cited.

silicate to carbonate, and then back again to silicate, the only limit to the process would be the satisfying of the mutual affinities of the silica and the basic oxides present.

May 14.—General Sabine, R.A., Treas. and V.P., in the Chair.

The following communications were read:—

"On the Organization of the Brachiopoda." By Albany Hancock, Esq.

"On the Placenta of the Elephant." By Professor Richard Owen, F.R.S. &c.

May 28.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Cause of the Rhythmic Motion of the Heart," being the Croonian Lecture. By James Paget, Esq., F.R.S.

The author draws the following conclusions as to the most probable explanation of the rhythmic action of the heart:—

1. In the Vertebrata it is due to the time-regulated discharges of nerve-force in certain of the ganglia in and near the substance of the heart, by which discharges the muscular walls are excited to contraction.

2. In Invertebrata, the corresponding pulsatile movements of hearts or vessels are probably independent of nerve-force.

3. The time-regulated rhythmic action, whether of the nervous centres or of the independent contractile walls, is due to their nutrition being rhythmic, *i. e.* to their being, in certain periods, by nutritive changes of composition, raised, with regulated progress, to a state of instability of composition, in their decline from which they discharge nerve-force, or change their shape, contracting.

4. The muscular substance of the heart in the Vertebrata, governed in its rhythmic action by appropriate nervous centres, has a rhythmic nutrition of its own, corresponding and coordinate with theirs; the impairments of its structure during action being repaired in repose.

5. Rhythmic nutrition is a process in accordance with the general laws of organic life, very many organic processes being composed of timely-regulated alternate action and inaction, or alternate opposite actions, *i. e.* being rhythmical, with larger or shorter units of time; and all organic processes being chronometric, *i. e.* ordered according to laws of time as exact, and only as much influenced by external conditions, as are those relating to weight, size, shape, and composition.

June 18.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On a new Mode of forming Triethylamine." By A. W. Hofmann, LL.D., F.R.S.

Researches on the constitution of the nitrogenous organic bases, which I zealously prosecuted some years ago, and the result of which were communicated to the Royal Society, have been, by circumstances, interrupted for some time. Nearly all the observations recorded in my communications refer to the primary or amidogen-bases, *i. e.* the compound ammonias, in which 1 equivalent of hydrogen is replaced by an organic molecule. Of the secondary and tertiary bases (imidogen- and nitrile-bases), *i. e.* the ammonias in which 2 or 3 equivalents of hydrogen are replaced, and of the fixed ammonium-bases, little more is known at present than their mode of formation and their composition.

On again taking up this subject lately, it appeared of paramount importance to search for new and, if possible, simpler methods of forming the secondary and tertiary bases.

Of the several experiments made for this purpose, on which I intend to report at another opportunity, I may be allowed even now to quote a result which appears to be capable of a more general application.

One of the common methods of preparing the amidogen-bases, consists in treating cyanic ether with hydrate of potassa, a reaction which was first observed by Wurtz. To take a special case, cyanate of ethyle, when treated with hydrate of potassa, assimilates 2 equivalents of water and splits into ethylamine, which is set free, and carbonic acid, which is fixed by the alkali. An appropriate modification of this reaction appeared to afford a means of passing directly from cyanate of ethyle to *triethylamine*. For this purpose it was only necessary to offer to the cyanate of ethyle 2 equivalents of oxide of ethyle instead of 2 equivalents of water. The action of cyanate of ethyle upon ethylic potassa appeared to involve the conditions necessary for accomplishing this result.

Experiment has fully confirmed this anticipation.

On digesting for several hours at a moderate temperature a mixture of pure cyanate of ethyle with pure ethylic potassa or soda (that is, absolute alcohol as far as possible, saturated with potassium or sodium), and distilling subsequently upon a sand-bath, a strongly alkaline liquid containing alcohol is obtained. Saturated with hydrochloric acid, this liquid furnishes on evaporation to dryness a residue from which a considerable quantity of *triethylamine* is expelled on addition of potassa.

The analogy of the two processes, of the formation of ethylamine and triethylamine, is obvious on glancing at the following equations.

$$\underset{\text{Cyanate of ethyle.}}{C_2ENO_2} \quad + \quad \underset{\text{Hydrate of potassa.}}{2(KO, HO)} \quad = \quad \underset{\text{Ethylamine.}}{N\left\{\begin{array}{l}E\\H\\H\end{array}\right.} \quad + \quad \underset{\text{Carbonate of potassium.}}{2KCO_3}$$

$$\underset{\text{Cyanate of ethyle.}}{C_2ENO_2} \quad + \quad \underset{\text{Ethylic potassa.}}{2(KO, EO)} \quad = \quad \underset{\text{Triethylamine.}}{N\left\{\begin{array}{l}E\\E\\E\end{array}\right.} \quad + \quad \underset{\text{Carbonate of potassium.}}{2KCO_3}$$

There could be no doubt regarding the identity of the basic compounds thus formed with triethylamine. The base floating on the surface of the saturated aqueous solution, possessed in a marked manner the characteristic odour of triethylamine. In order to exclude the possibility of a mistake, the hydrochloric solution of the compound was mixed with a solution of bichloride of platinum. Only after protracted standing of the highly concentrated solution, deep orange-coloured, well-formed crystals of the platinum-salt were deposited, the physical characters of which were still fresh in my memory.

The determination of platinum furnished results closely agreeing with the formula

$$N\left\{\begin{matrix}E\\E\\E\end{matrix}\right. HCl,\ PtCl_2$$

I have already applied this reaction in various directions, and shall communicate the results which I have obtained at a future period. On considering how frequently, in the action of hydrated potassa upon organic substances, the hydrogen of the water of hydration is assimilated by the products of decomposition, the substitution of ethylic potassa for the hydrate appears in many cases to promise valuable results from this mode of ethylation. The facility, however, with which ethylic potassa decomposes at a comparatively low temperature, must always be a serious obstacle to an extensive use of this reaction.

GEOLOGICAL SOCIETY.

[Continued from vol. xiv. p. 156.]

November 4, 1857.—Col. Portlock, R.E., President, in the Chair.

The following communications were read:—

1. "On the Correlation of the Triassic and Permian Rocks of the Odenwald in the Vicinity of Heidelberg, and those of Central England." By E. Hull, Esq., B.A., F.G.S.

The author first referred to the memoir by Murchison and Morris, in the eleventh volume of the Society's Journal, on the Palæozoic Rocks of the Thüringerwald and the Harz, for the special description of the Permian formation in that part of Germany; and, as these deposits appear to be comparable, almost stratum for stratum, with those of England, Mr. Hull sought for a similar correlation at Heidelberg when on a visit there this autumn. He referred to Dr. Leonhard's 'Geognostische Skizze von Baden,' 1846, for descriptive details of the geology of this locality.

Immediately overlying the granite at Heidelberg is a red breccia of porphyry and granite, upwards of 200 feet thick, which is the "Rothe-todt-Liegende" of the Odenwald, being similar to that rock in Thuringia and other parts of Germany, and comparable with the trappoid breccia of Worcestershire. It is of local origin; the parent rock-masses being still seen in the neighbourhood. Beneath the Worcestershire breccia, however, is a series of red marls and sandstones, not occurring in the Odenwald, but probably present in the Thüringerwald. The origin of this breccia is in Germany associated with volcanic phænomena; and, according to Prof. Ramsay, it is of glacial origin in England. See Quart. Journ. Geol. Soc. vol. xi. p. 187.

Over the Heidelberg breccia lies a great mass of Bunter sandstone, upwards of 1400 feet in thickness. It is throughout of a nearly uniform composition; and, except that it contains but comparatively few pebbles, it is much like the conglomerate-beds of Lancashire and Cheshire. The author considers, that, from its uniformity of composition, and consequent indivisibility into subdivisions, and from its resemblance to the above-mentioned member of the New Red Sandstone of England, this red sandstone of Heidelberg belongs wholly to the Upper Bunter (Bunter-Sandstein), of Triassic age; and that none of it is referable to the Lower Bunter (Bunter-Schiefer), which Murchison and Morris have shown to belong to the Permian series.

In England the Bunter is divisible into three groups, viz. 1. Upper variegated sandstone; 2. Conglomerate-beds; 3. Lower variegated sandstone. Whilst the Bunter of the Odenwald would seem to be in this respect imperfect, as compared with that of England, yet the author thinks that the two districts in question present but one contemporaneous formation.

Mr. Hull treated of the Muschelkalk, next overlying the Bunter, in a few words, it being absent in England. Of the succeeding formation, or Keuper, the author stated that a true parallelism may be traced in the order of succession of the beds of this formation in England and around the flanks of the Odenwald. First, or lowest, are grits and shales, about 100 feet thick, with a calcareous breccia, of local origin, in which a fragment of Bunter-sandstone has been met with. These beds are parallel with the "Waterstones" and Keuper-breccia of Worcestershire, Staffordshire, and Cheshire, and form the "Lower Keuper-sandstone." Evidences of unconformity between the Waterstones and the Bunter in England are not wanting, though not so strong as those of the unconformity of the Lower Keuper and the Bunter of the Odenwald.

A series of gypseous shales and marls, about 150 feet thick, separate the Lower from the Upper Keuper-sandstone. The last, about 10 feet thick, contain plant-remains, and are similar to the so-called "Keuper-sandstone," with *Posidonomya minuta*, of England. A considerable thickness of variegated shales, with gypsum and hæmatite, covers the Upper-sandstone.

In conclusion, the author dwelt upon the quasi-triassic aspect of the Permian formation, with its two argillo-arenaceous groups and intermediate calcareous band, as pointed out already by Sir Roderick Murchison, and as shown in a synoptical table by which the paper was illustrated.

2. "On the Extinct Volcanos of Victoria, Australia." By R. Brough Smyth, Esq., C.E., F.G.S.

The district in Southern Australia in which lavas, basalts, and other evidences of recent igneous action are found extends from the River Plenty (a tributary of the Yarra), on the east, to Mount Gambier on the west. Its most northern point is Macneil's Creek (a tributary of the Loddon), in 37° S. Lat., and its most southern point is Belfast, in 38° 21′ S. Lat. Its extreme length is 250 miles, and its extreme breadth about 90 miles.

The following were enumerated and described as the most distinctly marked crateriform volcanic hills:—

1. A hill near the source of the Merri Creek, on the Dividing Range, about 25 miles north of Melbourne, and already described by Mr. Selwyn, the Government Geologist. 2. Mount Atkin, about 1500 feet above the sea-level. 3. Mount Boninyong, adjacent to the Ballarat Gold-fields. 4. Larnebaramul or Mount Franklyn. 5. Mount Rouse. 6. Several crateriform hills around Lake Koraugamite, and the often conical hills known as the Stony Rises. 7. Tower Hill, between the towns of Warnambool and Belfast, and close to the coast. In the last-mentioned instance the scoriæ have been found by well-sinkers to overlie, at the depth of 63 feet, the original surface of the ground, covered with coarse grass, such as that now found growing, and amongst this dry, but not scorched, grass the workmen are said to have found some living frogs.

Over nearly the whole extent of Victoria there are masses of intrusive basalt, in some places columnar, breaking through both the granite and the palæozoic strata, and occasionally through the overlying Tertiary (Miocene) beds also. Extensive denudation has destroyed the probably overlying portions of these old basaltic outbursts, both before and after the tertiary period. A newer series of eruptive trap-rocks, sometimes as dense and hard as the older basalts, but more frequently vesicular and amygdaloidal, pierce the old tertiary and also the post-tertiary beds, or the later quartzose and auriferous drifts. These newer basalts and lavas were probably erupted at a period when considerable areas, both north and south of the main coast-range, were submerged; and the lavas cooled rapidly and not under very great pressure. These eruptions do not appear to have disturbed the Tertiary beds, which are usually found nearly horizontal. After these newer basaltic lavas were erupted and denuded, and after the deposition of the overlying pleistocene drift, some of the volcanos were still acting, though not so energetic as previously, emitting porous lavas and pumice; and at a still later period volcanic ash and scoriæ, such as that which rests on the ancient humus at Tower Hill, and that of Mount Leura and the Koraugamite district, were thrown out when the igneous force was almost exhausted. Mr. R. B. Smyth pointed out the interest attached to the extinct volcanos of Victoria as connected with the great volcanic chain extending from the Aleutian Islands to New Zealand; and concluded with some observations on the recent occurrence of earthquake-movements in Southern Australia, and on the evident uprise of the coast-line, as having reference to the probably not yet exhausted force of the volcanic foci of that region.

November 18, 1857.—Col. Portlock, R.E., President, in the Chair.

The following communications were read:—

1. "On Estuary Strata in Shotover Hill, near Oxford." By John Phillips, LL.D., F.R.S., F.G.S., Reader in Geology in the University of Oxford.

The author presented, in the first place, an historical notice of the facts and opinions published in regard to the sandy strata which in this detached hill rest upon the Portland series. From the time of Holloway (1722), to that of Smith (1800–1815), these deposits, with their ochres and variously-coloured sands, were always classified with the ferruginous sands, with ochre and fuller's earth, of Bedfordshire and Buckinghamshire, and received the general title of "Iron Sand." Conybeare (1822) uses the same title, traces them along the vale of Aylesbury, and refers them to the Hastings Sands. At this time, however, the Lower Greensand was not perfectly discriminated from the Hastings group.

In 1827 Dr. Fitton appears to have ascertained the occurrence of Purbeck deposits at Whitchurch in Buckinghamshire. About 1832 the Rev. H. Jelly discovered Paludiniform shells in the sands of Shotover; and in 1833, and again in 1836, Mr. H. E. Strickland communicated notices of this discovery to the Geological Society and to Dr. Fitton, in whose great Memoir "On the Strata below the Chalk" the information was published. From that time to the present no further notice has appeared; but, previous to 1854, Mr. Strickland, by his own researches, added a distinct *Unio* to the shells already collected; and since 1854 the author of the present notice has been enabled to augment the list of fossils, and to ascertain precisely the main facts regarding their geological position.

The Portland rocks (70 feet thick), consisting of green sand, enclosing one band of clay and two or three layers of large subcalcareous concretions, are rich in fossils, and some of these (*Pecten*, *Perna*, &c.) can be traced through the whole deposits to near the very top. This group is suddenly covered by sands, white, yellow, brown, reddish, or black, but not green; banded somewhat regularly by white or cream-coloured clays, and marked by imperfect ramifying layers of deep-brown peroxide of iron; chert-masses also occur. The stratification, where most regular, as in the white sands and clays, is often undulated, but there is very little trace of oblique or drift-bedding. Ochre occurs in several parts of the section, and has been much worked in the upper part. The whole series is about 80 feet thick.

In all the lower half of the series, but only in the hard ferruginous layers and geodic masses, and there not abundantly, we find coniferous wood, small spiral shells, and bivalve shells. The bivalves are of the genus *Unio*—one comparable in size to *Unio Valdensis*, but different in general figure, and in the characters about the fulcrum, ligament, and posterior slope. The other is of smaller and more delicate type, like *U. subtruncatus*. A *Cyclas* or *Cyrena* occurs in the specimens collected by Mr. Jelly. The spiral shells are partly *Paludina*, of two or perhaps three species,—one approaching in size to ordinary examples of *P. fluviorum*, the others smaller,—and partly of other genera. One is striated parallel to the volutions, like a *Littorina*. Another resembles somewhat the cast of a *Natica*. No *Cyprides* have yet been recognized with certainty. Regarding two of the species of molluscs above alluded to, as possibly belonging to salt-water, but all the others as due to the influence of river-action, the author concludes that through a considerable part of the thickness of these sands there is evidence now obtained of the estuarine, but not lacustrine, origin of the sediments. The upper parts of the series have been carefully explored to the summit, especially about the ochre-bands, but to this date they have yielded no fossils, and the author desires to leave it for further inquiry whether these crowning sandstones are of the same estuary character, or belong to a detached part of the Lower Greensand strata, which undoubtedly exist to the south-west and north-east.

Founding his inferences on Shotover, but confirming them by reference to points in the neighbourhood, where Purbeck strata are seen below these "Iron Sands," the author expresses his opinion that in these sands we have a northern equivalent of the Hastings Sands, —that the river to which they may be ascribed was probably not that of the typical Wealden, but a different stream, nourishing different *Unionidæ*,—and that its effects will be traced much farther to the north-eastward, along the country now under survey by the Government Geologists, to whom is committed the very important task of tracing the difficult boundaries of the Lower Cretaceous and Upper Oolitic deposits.

2. "On the Mineralogical and Palæontological characters of the Palæozoic Strata of the State of New York." By J. J. Bigsby, M.D., F.G.S.

In this communication the author presented a *resumé* of the chief geological points characteristic of the palæozoic basin of New York, which may thereby be compared with other districts of palæozoic rocks. These points of character and comparison were arranged under the following heads:—"mineral character," "mode of transition from group to group of strata," "place," "position or dip," "thickness," "fossils in general," "typical fossils," "fossils occurrent in Europe," and "fossils recurrent in New York."

The evidences used in this synopsis are based upon a series of elaborate tables constructed by Dr. Bigsby from the writings of James Hall, Vanuxem, Conrad, De Verneuil, Murchison, Sharpe, Sowerby, Portlock, Salter, M'Coy, Morris, and others. The author commenced with a sketch of the geographical features of the region under consideration, illustrated by the great geological map of middle North-east America prepared by himself.

The characters of the several members of the palæozoic basin of New York were then concisely described in succession, under the heads above referred to, the following being the order of classification used by the author.

A. Lower Silurian: 1. Potsdam sandstone; 2. Calciferous sandstone; 3. Chazy limestone; 4. Bird's-eye limestone; 5. Trenton limestone; 6. Utica slate; 7. Hudson's River rocks. B. Middle Silurian, or Transitional group: 8. Oneida conglomerate; 9. Medina sandstone; 10. Clinton rocks. C. Upper Silurian: 11. Niagara shales and limestone; 12. Coralline limestone; 13. Onondaga salt rocks; 14. Waterme rocks; 15. Lower Pentamerus limestone; 16. Delthyris shaly limestone; 17. Upper Pentamerus limestone. D. Lower Devonian: 1. Oriskany sandstone; 2. Cauda-galli grit; 3. Schoharie grit; 3. Onondaga limestone; 4. Corniferous limestone. E. Middle Devonian: 5. Marcellus shale; 6. Hamilton rocks; 7. Tully limestone; 8. Genessee slate; 9. Portage sandstone; 10. Chemung rocks. F. Upper Devonian: 11. Catskill or Old Red Sandstone.

From the consideration of the many members of this great palæozoic series of Central North America, severally and collectively, the author drew numerous inferences, and offered them to the notice of the Society, not as all being new or unassailable, but as affording many points of interest both for geologists in general, and the student of the American rocks and fossils in particular.

The following are some of the principal inferences:—That the Silurian and Devonian Systems of New York belong to one connected period; being the products of successive and varying Neptunian agencies, operating in waters which deepened westward from the Atlantic side, and southward from the Laurentine chain on the north. These palæozoic groups pass one into the other by gradual mineral and zoological changes; there being a nearly perfect conformability and a considerable community of fossils. The chief break is at the Oriskany sandstone, there being no break of like importance at the period of the local Oneida conglomerate. A division of the Silurian and Devonian systems, each into three stages, is based on change of sediment and the fossil contents. The Middle Silurian stage is a period of especial transition; from the coarseness of some of its sediments, from their alternations, and from the organic poverty prevailing. The New York Basin exhibits few uplifts; it consists of a number of comparatively thin undulating sheets of sediments (about 13,300 feet thick altogether) dipping slightly to the south-west, here and there pierced by a peak of crystalline rock, and along three lines raised into broad low domes of great length. The strata have experienced two kinds of disturbance, from plutonic agency: 1. secular or slow oscillation during deposition; 2. paroxysmal uplifting subsequent to their formation, and probably of post-carboniferous date. The form and direction of the five great Canadian Lakes follow the outcrops of the local sedimentary rocks; and the contour of the valley of the St. Lawrence is also due to the relative positions of these palæozoic strata. Some of the groups of strata, during and after deposition, were subaërial, presenting the conditions of dry land and shallow waters, for long and varying periods. In New York, as elsewhere, there is intimate connexion between the fossils and their sediment or habitat. The iron-ore frequently accompanying invertebrate remains had access to them after their death and sepulture. Every serial group of strata marks a distinct centre of life; a separate realm or community of animated beings, which may be called epochal, so marked are the differences. The majority of these existences always perished at the end of the group, because (among other reasons) the new sediment with its new flora was only able to nourish a few, if any, of the old molluscs. All the individual existences were perfect at once from the earliest dawn of life, and all bear evidence of being created on one and the same idea or plan. There is no evidence of multiplication of species by transmutation. The geographic life-centres are also well marked and numerous, forming separate provinces, linked together by a few forms in common, as seen also in other zoological groups, whether of palæozoic, secondary, tertiary, or recent date. The mollusc having the greatest vertical range has the greatest horizontal extension, being found in the most distant regions. The principles among fossils of recurrence, succession, increment, and relative abundance are the same in New York, Wales, and elsewhere; modified by local circumstances. Recurrence, or reappearance, in different strata, is, at the same time, the measure of viability in the fossil, and of connexion in the groups. It may partly be due to migration. It is not so common in New York as in Wales;—in other words, the vertical range of fossils is longer in Wales; because in the latter area there has been a more universal dissemination of calcareous matter throughout the strata; and because in New York there are three great separate masses of sediment destitute of lime, each more than 1000 feet thick; which were therefore unfavourable for the passage of marine life. A remarkable feature in the uppermost four groups of New York Siluria (the Lower Helderberg series) is the substitution in them of limestone for the arenaceous mud of the Ludlow rocks, their contemporaries. It has given them a Wenlock character; but it is to be remembered that the Ludlow and Wenlock Groups of Wales are in close zoological connexion; 74 out of 311 species of organic remains being common to both—or nearly one quarter. Many Silurian brachiopods and some other molluscs occur also in the Devonian; and some may even be found in the Carboniferous system.

XI. *Intelligence and Miscellaneous Articles.*

NOTE ON THE DECOMPOSITION OF CERTAIN SALTS, PARTICULARLY LEAD-SALTS, BY THE ACTION OF THE VOLTAIC CURRENT.

BY M. C. DESPRETZ.

IN an experiment made with the view of determining the proportion in which copper and lead are deposited on the negative pole, when a solution of a mixture of acetate of copper and lead is traversed by a voltaic current, the author found that the metals, instead of combining, as he expected, at the negative pole, separated, the lead being deposited in the form of oxide upon the positive, and the copper in a metallic state on the negative plate.

The decomposition was effected by two of Bunsen's elements; the acetates of copper and lead were mixed in atomic proportions and dissolved in seven to eight parts of water. As soon as the circuit is closed, the positive (charcoal) platinum plate is seen to acquire a reddish-brown tint. In eight or ten hours the positive plate is covered with a coat resembling a black varnish, and if the experiment be continued still longer, the deposit becomes detached from certain points and falls to the bottom of the vessel, or rises to the surface by means of the adherent gas.

The deposit on the negative pole is red copper mixed with traces of lead. This copper dissolves rapidly at the ordinary temperature in pure nitric acid of spec. grav. 1·329, or diluted with water. The black deposit of the positive pole only dissolves in small quantity in such nitric acid at a boiling heat, even by several days of contact. On the addition of muriatic acid the action is brisk, and the black substance is changed into white chloride of lead. This chloride, treated with an excess of nitric acid, is converted into nitrate.

The black substance heated by the spirit-lamp in a glass tube, acquires the red colour of bichromate of potash without losing its lamellar form. In this state it gives a brick-red powder, which, treated with nitric acid, is partially dissolved; the residue is black. Some other experiments lead to the belief that the black matter is binoxide of lead.

From some experiments made to determine whether the presence of copper in the mixture is necessary for the formation of this black coat, it appears that nitrate of lead, and the pure or commercial acetate, furnish the same black matter at the positive pole and metallic lead at the negative pole, and that with a mixture of the acetates of lead and cadmium, the same black substance is always found at the positive pole, and lead mixed with cadmium at the negative. Protacetate of manganese gave no deposit on the negative pole, and deposited a black, oxidized compound on the positive pole. The absence of deposit at the negative pole is probably due to the metal being dissolved by the free acid as fast as it is deposited. The black deposit on the positive pole is not manganese, or protoxide of manganese, or manganic acid; it is unalterable in aërated water and insoluble in nitric acid. At the ordinary temperature it dissolves in dilute sulphuric acid. The solution is red.

In tartrate of potash and antimony the negative plate is covered with crystalline metallic antimony, and the positive plate with a yellowish-red coat. In the course of a few days the yellow predominates. After immersion for eight days in distilled water, it is perfectly yellow. It is probably anhydrous antimonic acid. The author proposes making experiments with other metals to see whether a similar separation takes place in them.

In the meantime he inquires how these compounds, which are found at the positive pole, are formed. They did not exist in the solution. It can hardly be supposed that a portion of the metal is carried to the positive pole and then oxidized by absorbing the oxygen disengaged at that pole. It is more probable that the division takes place at the negative pole; that a portion of the oxide gives up its oxygen to another portion, which, thus acquiring acid properties, passes to the positive pole. If this be exactly the case, in the decomposition of the lead salts, one half of the lead would be deposited at the positive pole in the form of binoxide of lead, or plumbic acid, and the other half at the negative pole in the metallic state. In this way the law of chemical decompositions would not be altered by the division in question.

As regards the dependence of this phænomenon upon the intensity of the current, a solution of nitrate or acetate of lead submitted to the action of 1, 2, 3, 20, or 100 Bunsen's elements disposed in a tension series, gives the black substance at the positive pole almost instantaneously, and on the negative pole, lead in narrow, brilliant, crystalline laminæ, with this sole difference, that the black product of 20 or 100 elements is not smooth at the surface like that of two elements; it is dull, not compact, &c. With a single element the experiment has a peculiar character,—no gas is evolved from either pole. This is in favour of the opinion that a portion of the oxide yields its oxygen to the undecomposed portion, which becomes surcharged with oxygen, without the decomposition of water.

Nitrate of bismuth also gives a reddish-brown colour to the positive plate.—*Comptes Rendus*, Oct. 5, 1857, p. 449.

ON THE COERCIVE POWER OF PURE IRON.

BY A. MATTHIESSEN, PH.D.

Having prepared some galvanoplastic iron both out of a solution of pure protochloride and protosulphate of iron, in order to ascertain whether galvanoplastic metals, precipitated from different solutions and under different circumstances from the same solution, take the same place in the thermo-electric series, or in other words, give no thermo-electric currents with each other (which, however, they appear to do), I found that iron thus precipitated possesses great coercive powers, and retains its magnetism undiminished for a length of time. It is very brittle, and its hardness is equal to that of tempered steel.

ON THE FORMATION OF ISATINE BY OZONE.

BY PROF. O. L. ERDMANN.

The author has found that pure indigo-blue, made into a paste with water, is readily converted into isatine by ozonized air.—*Journ. für Prakt. Chemie*, lxx. p. 209.

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[FOURTH SERIES.]

FEBRUARY 1858.

XII. *On Gravitation and the Conservation of Force.* *By* ERNST BRÜCKE, *Member of the Academy of Vienna, and Professor of Physiology in the University*[*].

ON the 27th of February of the present year (1857) Faraday gave a lecture in the Royal Institution[†], in which he sought to prove that our usual conception of the force of gravity is not in harmony with the principle of the conservation of force. In accordance with this conception, he defined gravity as "a simple attractive force exerted between any two or all the particles or masses of matter, at every sensible distance, but with a strength varying inversely as the square of the distance." He draws attention to the circumstance that this definition presupposes an *actio in distans,* a point which Newton himself found a source of difficulty, and on which, in his third letter to Bentley, he expresses himself in the following manner:—

"That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance, through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to the other, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers."

Faraday shows, further, that as two particles are separated from each other, their mutual attraction diminishes. This means nothing else than that force is destroyed. When two particles are brought more closely together, their mutual attraction aug-

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[*] From the *Sitzungs-Berichte der Mathem. Natur. Wiss. Classe für* 1857.

[†] Our continental brethren often confound the Royal Society and the Royal Institution.—ED.

ments, and this can only mean that force is generated. Both results are inconsistent with the law of the Conservation of Force. If we imagine a particle isolated and without gravitating force, and afterwards introduce another, an attractive force is supposed to be set up on both sides, and thus again, according to the usual conception, we should have a creation of force.

Further, if we conceive the particle B to be separated from the particle A for an infinite distance, the attractive force is thereby infinitely diminished; it is, in fact, as if B in respect to A were actually annihilated. The same reasoning may be applied to several particles. When a body approaches another by gravitating motion, in virtue of the *vis inertiæ* a quantity of mechanical force is accumulated, and still the *vis attractionis* has not diminished but augmented. If, however, by the application of an external force the body be removed in the opposite direction, there is no force stored up by the act, but, on the contrary, the *vis attractionis* is, in consequence of the greater distance, diminished. Hence Faraday considers that our present notions as to the cause of gravity cannot be in harmony with the law of the conservation of force, as long as it is not shown whence the force generated is derived, and whither the force lost has disappeared. He does not doubt the general applicability of the principle of conservation, but he believes that in our definition of gravity we only describe one exercise of that power, and that this gives an incomplete idea of the nature of the force as a whole.

It is a long time since such a far-reaching physical question has been touched upon wholly without the aid of mathematical apparatus—without the assistance of those wonder-working symbols whose brief rhetoric speaks more convincingly to the mind than the tongue of Cicero or Demosthenes. When, however, the first natural philosopher in the world—when Faraday opens the discussion thus, it is assuredly a sign that the time has come when the question may be so treated.

If I venture to take part in this discussion, it is not because I feel myself competent to meet that great man upon his own field, but because the subject to which I devote myself, that is, physiology, causes me perhaps to pay attention to things which may have been further from the course of thought of the English philosopher. Habitually compelled to direct my eye upon the inner man, I may perhaps be able to make some remarks on the connexion between natural things and our way of representing them to our minds, which will cause the relation between the law of gravitation and that of the conservation of force to appear in a different light.

What is it in physics that we denote by the name Force? Man stands before the external world as a *camera obscura*, on the screen of which Nature perpetually casts her images; we cannot understand these images in themselves, nor the objects from which they proceed; but we see that under the same circumstances the same images always appear, and this regularity of their appearance permits us to determine them beforehand. We construct formulæ, either verbal or in mathematical symbols, and name the principles from which these are deduced Laws of Nature.

We have motion incessantly before us, partly as the motion of masses, and partly as the motion of the ultimate particles of matter among themselves, in the form of heat; or when the rays of this heat excite our organs of vision, in the form of light. We have so studied the circumstances under which these motions vary and succeed each other, that we can predict their mode of appearance, their intensity and their direction; and we have found that the formulæ which serve for this purpose assume their simplest form, when we ascribe to the ultimate particles of matter attributes which we name forces, the magnitude of which we make dependent on mass and distance. No one can prove that these attributes have a real existence, in the strict sense of the word, that they exist in the things themselves, and beyond the domain of human thought. From the experience of the senses, we know no other real source of motion than other motion, and so on, till the links of the chain disappear from our sight: but we help ourselves by assuming causes which we name forces, with which we deal, and as experience proves, with good effect. They exist actually in the domain of thought, and have there their full justification: whether they would also have an existence supposing no thinking being were to have had an existence, is a question which man cannot decide, because, beyond the sphere of his thought, knowledge has for him no existence. The upshot of all natural investigations is to seize upon that which comes immediately within the sphere of our perceptions, and to unfold that which, on account of its magnitude as regards either time or space, is withdrawn from immediate perception, by instruments, by experiments, or by induction, and finally to discover a connexion between the collective phænomena which shall be in harmony with our laws of thought. From this point of view, and only from this, Newton's doubt as to the force of attraction acting at a distance is, I believe, to be regarded. He has shown by his acts how deeply he felt the necessity of giving to his law the form which he has given to it, because in this way alone could it appear as the immediate expression of the facts; but he went further, and wished to form a conception of the forces which were the results of his own logical abstraction, and here he encountered difficulties. We represent to ourselves with perfect clearness things and motions which lie without the domain of our senses, and we need only the constructive understanding, to form and build up the conception. We figure to ourselves the motion of the particles of air in sound, and of the particles of æther in light, without any essential difficulty: the material for this is at hand. We are acquainted with masses great and small: we know their motions; it is only necessary, by an act of thought, to bring the latter into the proper form, abstracting from their relations of time and space, and all is in order. But it is otherwise with forces. We know forces only as mental abstractions, as abstractions from the changes which our *ego* perpetually suffers from the external world. We possess therefore no sensuous material from which we can form an image of them. All attempts that we make in this direction only end in our substituting in a somewhat offhand manner, things for forces which are utterly different from them. We represent them by weights which pull and springs which push, because in particular cases these produce the same effects as the forces in question; or we picture them as lines, because the changes of the latter in magnitude and direction are suited to represent the corresponding changes of the forces. It is simply because forces cannot be pictured before the mind, that Newton found it impossible to form a conception of an attractive force, inherent in bodies and capable of acting through a vacuum on other bodies; and he to whose glance the heavens were more penetrable than his inner self, confounded this impossibility with absurdity. Nothing, however, is further removed from absurdity than the notion of an attractive force acting at a distance. In the case of the abstractions of natural science, the aphorism is perhaps truer than elsewhere, "By their fruits ye shall know them." And as Faraday himself remarks, Newton's law has been found valid "to an extent that could hardly have been within the conception of Newton himself when he gave utterance to the law."

Its validity has thus been proved without our ever having given up the notion of the *actio in distans*; nay, the very idea of an attractive force includes that of an action at a distance; for the only phænomenon which immediately and necessarily leads to the assumption of an attractive force, is the approximation of two masses to each other without external impulse, and without the action of other masses. As the bodies are supposed to approach each other, they could not of course have been already in contact, and as the action of all other particles is excluded, the force which causes them to approach must act across the vacuum between both masses. We shall also, if I mistake not, find that the law of gravitation, as it has been hitherto expressed, is by no means in contradiction with the principle of the conservation of force. I will here ask permission to proceed from Faraday's own example. Let the mass A 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. Let us look a little more closely into the matter with which we have here to deal. What is our measure of the force of attraction? It is the augmentation of velocity which a body experiences in the unit of time by the action of the force. In virtue of inertia, the impulses of gravity are accumulated in the body, the velocity augments in the same proportion, and the quantity stored up in the unit of time serves as a measure for the magnitude of the attractive force. This is the only true and direct measure of a force; for as the very notion of a force has been derived from the concrete phænomena of motion, the measure of a force must also be derived from the same thing.

It is this augmentation of velocity which follows the law of the inverse square of the distance, and this stands in no contradiction, but in the most complete harmony with the principle of the conservation of force. This principle affirms, that in every system which is abandoned to itself the sum of the tensions added to the sum of the *vires vivæ*, gives at all times the same quantity. In other words, that in every such system the quantity which is obtained when the moving masses are multiplied by half the squares of their velocities has a maximum, which is given once for all, which cannot be overstepped, and of which moreover nothing can be lost. Motion can never be destroyed so as to be incapable of regeneration, because motion disappears only in consequence of a change of place of the masses, which change in due time again appears as a cause of motion, and on the return of the masses to their original position, reproduces the motion which was consumed during the change. It is here of course assumed that the measure of the motion is always the product obtained by the multiplication of the single masses with the half-squares of their velocities. Let us take the simplest system of all, the vibrating pendulum. The maximum above referred to is the *vis viva* obtained when the mass is multiplied by the half-square of the velocity with which the pendulum passes its position of equilibrium; at every other point the *vis viva* is less, and at the turning-points it is zero. But at these points the whole force, as a cause of motion, is stored up; and when the pendulum again attains its position of equilibrium, the original motion will be reproduced. In this latter position no other cause of motion acts upon the pendulum than its own moment of inertia. When the pendulum moves away from its position of equilibrium, and then loses velocity, but at the same time generates a cause of motion, we express it by saying that *vis viva* is transformed into tension[*] (*actual* energy into *potential* energy[†]). When the pendulum again approaches its position of equilibrium, and from the cause of motion, motion itself follows, then we say tension is transformed into *vis viva* (*potential* energy into *actual* energy). An analogous state of things is observed throughout nature, from the motion of the heavenly bodies to the motion of the flame which flickers in our chimney, and in which the atoms are freed from that mysterious state of tension into which they have been forced by years of action of the solar rays.

But let us return to the example of Faraday. Let us first fix our attention on the mass A, which is removed from the mass B. Let us suppose B to be the earth, and A a stone which is cast from the earth upwards. Let the earth be supposed to be at rest, and let the influence of the atmosphere and the heavenly bodies be disregarded. After the velocity of the stone has sunk to zero it will fall back, and on reaching the earth will have attained a final velocity, the half-square of which, multiplied by the mass, gives a quantity of force, which, if exerted upwards, would be exactly sufficient to carry the stone to the height from which it has fallen. This is a known fact, and generally the first example with which the law of the conservation of force is illustrated. If I imagine the stone to be cast higher and higher from the earth, nothing is thereby changed, except that in the more distant portions of its path the velocity is more slowly consumed, and on returning is more slowly generated, than in the nearer portions: the final result is always the same, only a longer time passes before it arrives. Let us regard the stone at the moment when its velocity is zero, that is, when it is at its greatest distance from the earth. Has then the moving force, that is, the cause of motion, diminished at the moment in question in the system formed of the stone and the earth? By no means. It is true that the stone, by an external force, may now be more easily removed from the earth, and that it commences to fall with a slower acceleration of its motion than would be the case if it started from a point nearer to the earth; but there is in the stone a store of force accumulated, in virtue of which it attains a greater final velocity the further it has been removed from the earth.

The terms 'living force,' 'tension force,' 'moving force,' as they

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[*] See Helmholtz "On the Conservation of Force," Scient. Mem. 1853. p. 124.—Ed.

[†] Rankine's "Outlines of the Science of Energetics," Edinb. Phil. Journ. July 1855.—Ed.

are commonly made use of in the nomenclature of science, are calculated to convey the idea that these are three different forms of one and the same thing; but we must not suffer ourselves to fall into this delusion; we must always remember that they are three totally different things. We call the sùm of the motion actually existing *vis viva.* We denote it as force, as cause of motion, because it not only continues to act in accordance with the law of inertia, but because it can be communicated to other bodies, inasmuch as motionless bodies can be set in motion by moving ones.

Tension is the term applied to the cause of motion still to be disposed of, which itself is not motion, wholly regardless of the time in which it can generate or produce motion. *Accelerating force* is the term applied to the increase of velocity attained by a mass, or which it can attain, in an infinitely short time, divided by the length of the length of this infinitely small interval. The accelerating force in a system is therefore dependent at any moment, first on the masses which are, or are to be, set in motion, and the velocities which they may have already attained; and secondly, on the velocity with which tension is, or may be, transformed into *vis viva.* If the accelerating force is to remain constant for every single molecule, it is necessary that the velocity with which the molecule transforms tension into *vis viva,* or *vis viva* into tension, when divided by the product of its mass and velocity, should give a constant quotient.

Keeping this always in view, it will appear plain that the accelerating force may increase or diminish, without at all interfering with the principle that the sum of the tensions, added to the sum of the *vires vivæ,* always gives the same quantity; and we see that by the removal of two molecules from each other no portion of the force is destroyed, the indestructibility of which is affirmed by the principle of the conservation of force.

Let us suppose a portion of the masses which gravitate towards each other to be destroyed; then certainly not only accelerating force, but also, according to circumstances, a portion of the tension or of the *vis viva,* or of both, would be destroyed: but this only confirms us in our way of viewing the subject. The law of the indestructibility of matter has been proved as universally valid as that of the conservation of force. That the destruction of the one should involve that of the other, only shows us that both stand in intimate connexion with each other, and proves that we are right in placing the cause of the notion of gravity in the masses themselves, and not in the space between them.

Thus in all that has been hitherto said, so far as my consciousness reaches, so far as I am capable of distinguishing true from false, and like from unlike, all known facts are brought into complete harmony with our laws of thought when we suppose forces, as the causes of phænomena, to reside in the masses, the spaces between these masses being traversed by the forces. If the forces could be imagined as existing in space, it must also be conceivable that matter may be annihilated without changing the sum of the forces, and this, at least by me, is not conceivable.

One more point remains to be considered, which I believe is essential to the doubts raised by Faraday. We say the attraction diminishes with increasing distance, and moreover that the attracting forces are to each other in the ratio of the inverse squares of the distances. In Faraday's process of thought, this is understood as if we assumed a change of the cause of motion which actually resides in the masses. I do not know whether this is a prevalent notion. It is difficult to obtain information on the point, for an investigator may write memoirs all his life long and never once express his convictions upon the subject. I believe, however, it cannot be doubted that Faraday is right in opposing this notion, and in assuming with Newton the existence of a constantly active cause for gravitation. I believe also it may be shown that this assumption of Faraday's contradicts neither the facts themselves, nor the usual mode of interpreting them.

We say that the attracting forces vary inversely as the squares of the distances; we also say that the intensity of light varies inversely as the squares of the distances, but are thereby far from affirming anything regarding the intensity of the source of light itself. We speak solely of the effects upon surfaces exposed to the radiation, and know very well that the intensity of the luminous source will not be in the least affected by the proximity or the distance of the illuminated objects. When the rays of light were regarded not only as mathematical lines, but as the paths of the light particles, it was said the intensity of the light diminishes inversely as the squares of the distances, because the areas on which an equal number of rays fall, augment directly as the squares of the distances. Now that we deduce the action of light from wave-motions, we know that the intensity must obey the law of inverse squares, because during the propagation the same sum of *vis viva* is incessantly transferred to new masses, which augment directly as the squares of the distances. The change as the inverse square of the distance, appears to us now as a simple application of the law of the conservation of force.

We regard the attractive force residing in a mass as proceeding in straight lines from it in all directions, just as light sends its rays on all sides; and as we perceive the light when the retina, or a body seen by us, falls in the domain of the rays, so in like manner we perceive the action of the attractive forces when a body comes in their way [*]. When, therefore, we say, the

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[*] The rays of light exist independent of the presence of the second body, but is this the case with force? Does not the conception of force carry with it that of two material masses at least? If so, when one mass vanishes, force becomes a nonentity.—Ed.

effect of attraction diminishes inversely as the squares of the distances, we can by no means refer this to the source of attraction; we must rather assume that the latter is constant, and that also the sum of the effects neither diminishes nor increases with the distance. An example will make this clear.

Let a molecule be imagined with the space around it filled with molecules which are scattered at equal distances from each other. For the sake of simplicity, let the latter be regarded as of equal mass, and the mass of each of them as a vanishing quantity in comparison with that of the first molecule, which I will call the central molecule. The central molecule will attract all the others, and the action upon each of them will vary as the inverse squares of the distances. Let us suppose that around the central molecule we have a spherical shell of definite thickness, the sum of the actions proceeding from the central molecule will always be the same within this shell, however great the distance at which it is placed from the central molecule. For the action on the single molecules diminishes as the square of the distance increases, but the number of the molecules which make up such a shell, augments directly as the square of the distance.

When, therefore, we say that the attracting forces are inversely to each other as the squares of the distances, we wish to say nothing else than that the attracting force which belongs to every mass is constant, and spreads its actions, undiminished in their totality, in all directions. They act the more feebly on the separate parts the greater the space over which they are spread. We are not hereby forced to assume a propagation in the ordinary sense of the term, that is, a process which requires time for its transmission. For the law of the change of densities (or intensities) inversely as the squares of the distances, is totally independent of this assumption. It is true of a system of an infinite number of straight lines which issue from a point, as well as for a wave-motion which propagates itself on all sides round the same point.

[The idea of gravity was originally suggested by the attraction of iron by a magnet. A magnetic pole and a mass of soft iron will, if free to move, mutually approach each other, in virtue of some quality resident in each, which quality we call, or may call, the force of magnetism. In the case of the soft iron this is a variable quality, for the induced magnetism of the latter augments as it approaches the steel magnet. It would be no illegitimate use of language to say that in the case of the soft iron the force of magnetism varies, while in the steel it remains constant. Regarding, in the same way, the force of gravity as a quality resident in the gravitating masses, if this quality, as believed by Prof. Brücke, be constant, it would certainly be a contradiction in terms to say that *it* varied inversely as the square of the distance. But this is not asserted; and the difference between Mr. Faraday and his commentators we believe to be mainly a difference of definition, while some influence is to be ascribed to that daring hopefulness in investigation to which the world is so deeply indebted, and which causes its possessor to see possibilities of scientific conquest where others see none.—Ed.]

XIII. *On the Nature of Flame, and on the Condition of the Sun's Surface. By* John W. Draper, *M.D., Professor of Chemistry and Physiology in the University of New York*[*].

Among the recent publications on photo-chemistry, there is one by Professor Dove on the Electric Light (Phil. Mag. Nov. 1857), which will doubtless attract the attention of those interested in that branch of science. Examination by the prism, and by absorbing and reflecting coloured bodies, leads him to the conclusion that it is necessary to consider the luminous appearance as having two distinct sources:—1st, the ignition or incandescence of the material particles bodily passing in the course of the discharge; 2ndly, the proper electrical light itself. As respects the first, he illustrates its method of increase from low to high temperatures, by supposing a screen to be withdrawn from the red end of the spectrum through the coloured spaces successively towards the violet; and that of the latter from the bluish brush to the bright Leyden sparks, by a like screen drawn from the violet towards the red.

The true electric light exhibits properties resembling those observed in actual combustions, as though there was an oxidation of a portion of the translated matter when the spark is taken in air. The order of evolution of rays in this instance happens to be the same as in the second illustration of Professor Dove, that is, from the violet to the red. There are certain facts connected with these appearances of colour which are not generally known, and deserve to be pointed out.

In the Philosophical Magazine (Feb. 1848), I showed experimentally that there is a relation between the colour of a flame and the energy with which the combustion giving rise to it is going on. The more vigorous and complete the combustion, the higher the refrangibility of the light. A flame burning in its most tardy and restricted way, emits rays that are red; but burning in its most complete and effective manner, rays that are violet. In intermediate states of combustion, the intermediate colours are evolved in their proper order of refrangibility.

The flame of a candle or lamp consists of a series of concentric luminous shells, surrounding a central dark core. These shells shine with different colours, the innermost one immediately in contact with the dark core being red, and having a temperature of 977° F. Upon this, in their proper order of

[*] Communicated by the Author.

refrangibility, are shells the light of which is orange, yellow, green, blue, indigo, violet. When we look upon such a flame, the rays issuing from all the coloured strata are received by the eye at once, and impress us with the sensation of white light.

The differently coloured shells of which a flame thus consists, may be easily parted out from one another, and demonstrated by a prism. Their cause is the slower rate at which combustion occurs at points more and more towards the interior. On the outside, which we may say is in contact with the air, the combustion is most vigorous and complete, and hence the light there emitted is violet; but in the most interior portion of the shining shell, resting upon the dark combustible matter, the atmospheric air can hardly penetrate, or rather its oxygen is exhausted and consumed. Between the exterior and interior surface, the burning is going on with an activity constantly declining, because the interpenetration or supply of oxygen is gradually less and less.

But besides this collection of coloured shells, constituting what may be termed the actual flame, there is another region exterior thereto, and to be distinguished both in its chemical nature and in its optical relations. Chemically, it consists of the products of combustion and of the unburnt residues of the air, that is to say, carbonic acid, steam, and nitrogen. These are all the time escaping out of the true flame, and envelope it as an exterior cone or cloak. Optically, this portion differs from the true flame in the circumstance, that it is shining as an incandescent, ignited, but not a burning body. For physiological reasons, into the detail of which it is not necessary here to go, the tint of this exterior cloak seems to be a monochromatic yellow. That, however, is to a considerable degree a deception, prismatic examination proving that all the other colours are present, and that the yellow merely exceeds the rest in force or intensity.

A flame thus far may be considered as offering three regions: —1st, a central nucleus, which is not luminous, and consists of combustible vapour; 2ndly, an intermediate portion, the true flame, arising from the reaction of the air and the combustible vapour, and being composed of a succession of superposed shells, the interior being red, the exterior violet, and the intervening ones coloured in the proper order of refrangibility; the cause of this difference of colour being the declining activity with which the combustion goes on deeper and deeper in the flame. As to temperature, the inner red shell cannot be less than 977° F., and the exterior violet one probably more than 2500° F. 3rdly, an envelope consisting of the products of combustion, exterior to the true flame, shining simply as an incandescent body, and its light for the most part overpowered by the brighter portion within.

By the aid of the facts thus presented, we can easily explain the nature of the other regional divisions distinguishable in such a flame. There must be a blue portion below; blue, because it consists of the most refrangible rays, which issue forth in abundance, for there the exterior air is most copiously and perfectly applied. At the upper end of the flame, particularly if the wick be long and the supply of combustible matter abundant, the light emitted is red, for the products of combustion ascending past that part, make it difficult for the exterior air to get access.

Upon these principles we may also predict what colour a flame will have when we vary the circumstances of its burning. Tallow or wax at temperatures greatly beneath their usually understood point of combustion, oxidize with a pale violet phosphorescent light, quite perceptible nevertheless in a dark room; and here the light is violet, for the supply of combustible matter is small, and that of the air abundant. The oxidation is therefore thorough and prompt. For a like reason, sulphur, as we commonly see, burns blue; but if a piece thereof is thrown into nitrate of potash ignited in a crucible, the light yielded is of intolerable brilliancy, and absolutely white. Its whiteness does not depend upon the physiological fact, that any colour, if it be intensely brilliant, will seem white to the eye; but it is optically white, as is proved by prismatic examination, when all the colours are perceptible. And the reason of this is, that at the high temperature to which the sulphur is exposed, it volatilizes faster than the nitrate of potash and air together can oxidize it, and offers every intermediate rate of combustion, and emits rays of every refrangibility.

In like manner it may be shown that carbonic oxide must burn with a blue flame, and cyanogen with a red. We can also foresee what must be the optical result of resorting to unusual methods of combustion, as when we throw into the interior of a flame a jet of air from a blowpipe. In this case we destroy the red and orange strata, replacing them by bluer colours. Examining such a blowpipe cone by the prism, we have a beautiful demonstration that such has actually taken place.

There is one of these special cases which deserves attentive consideration in connexion with the appearance of the electric light,—it is the production of Fraunhoferian lines when things have been arranged in such a way that an incombustible material is present in the substance to be burnt. This state is perfectly represented in the case of cyanogen, which contains more than half its weight of incombustible nitrogen. When the peach-coloured nucleus of the cyanogen flame is properly examined, it yields a series of dark lines and spaces exceeding in number and strength those of the sunlight itself. These fixed lines are the representatives of dark shells, superposed among the shining ones with definite periodicity. In such a cyanogen flame they bear no relation to the burning of the carbon, but to the disengagement of the nitrogen they must be attributed.

In other cases dark lines are replaced by bright ones, as in the well-known instance of the electric spark between metallic surfaces. The occurrence of lines, whether bright or dark, is hence connected with the chemical nature of the substance producing the flame. For this reason they merit a much more critical examination than has been yet given to them, for by their aid we may be able to ascertain points of great interest in other departments of science. Thus if we are ever able to acquire certain knowledge respecting the physical state of the sun and other stars, it will be by an examination of the light they emit. Even at present, by the aid of the few facts before us, we can see our way pretty clearly to certain conclusions respecting the sun. For since substances which are incandescent, or in the ignited state through the accumulation of heat in them, show no fixed lines, their prismatic spectrum being uninterrupted from end to end, it would appear to follow that the luminous condition of our sun, whose light contains fixed lines, cannot be referred to such incandescence or ignition. At various times those who have studied this subject have offered different hypotheses: one regarding the sun as a solid or perhaps liquid mass in a condition of ignition; another considering the light to be electrical; a third supposing him to be the seat of a fierce combustion. Of such hypotheses we have given reason for declining the first. Prismatic analysis, which demonstrates no resemblance between the light of the sun and that of any form of electric discharges with which we are familiar, enables us in like manner to reject the second; and upon the whole, facts seem most strongly to prepossess us in favour of the third, in artificial combustions similar fixed lines being observed. If such is to be regarded as the physical condition of the sun, we can no longer contemplate him as an immense mass slowly and tranquilly cooling in the lapse of countless centuries by radiation into space, as so many considerations drawn from other branches of science have hitherto led us to suppose, but he must be regarded as the seat of chemical changes going on upon a prodigious scale, and with inconceivable energy.

If the law designated above, that the more energetical the chemical action in combustion the more refrangible the emitted light, be translated into the conceptions of the undulatory theory, it not only puts us in possession of a distinct idea of the manner in which the combustive union of bodies is accomplished, the quickness of vibration increasing with the chemical energy, but it also enables us to transfer for the use of chemistry some of the most interesting numerical determinations of optics.

University, New York, Dec. 10, 1857.

XIV. *On the Conduction of Electricity in Electrolytes.*
By R. Clausius*.

1. In a former memoir† I considered the effects of a galvanic current within a conductor of the first class (*i. e.* one which conducts without electrolysis), without taking into account the manner in which the current was produced. It was there found that the laws regulating the production of heat in such conductors follow immediately from Ohm's law, and from the principle of the equivalence of heat and work. In a similar manner, if we consider a conductor of the second class, that is to say, one which conducts by electrolysis, without any reference to the other parts of the circuit, we may deduce conclusions, some strictly accurate, and others at least probable, which appear to possess some interest. In the following pages I will endeavour to develope some of these deductions.

2. With respect to the laws of the production of heat, if we admit the applicability of Ohm's law to conductors of the second class, the conclusions of my former memoir may also be extended, without any modification, to the case under consideration; as before, however, we shall assume that the current is stationary, and that it neither exerts nor suffers any inducing actions of an electro-magnetic or electro-dynamic kind.

In order to maintain the electricity in motion, notwithstanding the resistance offered by the conductor, a force must be present in every point of the latter, tending to move the electricity concentrated in the same in a definite direction; or, assuming two electricities, to drive the positive electricity in one direction, and the negative with equal energy in the opposite direction. This force proceeds from free electricity, which, as Kirchhoff has proved, may exist on the surface of the conductor or on the surface separating two different conductors, but not in the interior of the same. With respect to the interior of a homogeneous conductor, no matter whether the same belong to the first or to the second class, if we admit the hypothesis of two electricities moving in opposite directions, we must assume that equal quantities of both kinds of electricity are always present in every measurable space within the conductor; and if we admit only one kind of electricity, we must suppose the normal quantity of electricity to be always present within a given space, in consequence of which that space appears unelectric.

In our present case the *work* done by the moving force can be

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* From Poggendorff's *Annalen*, vol. ci. p. 338.

† "On the Work performed and Heat generated in the Conductor of a Stationary Current," Poggendorff's *Annalen*, vol. lxxxvii. p. 415; and Scientific Memoirs, New Series, p. 200.

determined in exactly the same manner as in my former memoir, so that it will suffice to give the final result without here entering into particulars. Conceive any portion, bounded by a closed surface, of the interior of a conductor to be selected for consideration; let $d\omega$ represent any element of this enclosing surface, V the value, at this element, of the potential function of the free electricity, N the normal to the element, considered positive outside the enclosed portion, and lastly, k the conductibility of the mass; the work W performed in this portion of the conductor during the unit of time will then be determined by the equation

$$W=k\int V\frac{dV}{dN}\cdot d\omega,$$

wherein the integration is to be extended over the whole surface enclosing the portion of the conductor under consideration.

3. At first sight it might perhaps appear that, with respect to the determination of the *heat* generated by the current, a difference should exist between conductors of the first and those of the second class. In conductors of the first class the molecular masses remain unchanged in position, and the electricity alone moves; whereas in conductors of the second class the constituents of these molecules are also set in motion, and decompositions and recombinations take place, during which, without doubt, a considerable activity is developed by the molecular forces with which the constituents act upon each other. On closer examination, however, it is easily seen that in determining the generated heat, it is not necessary to take into consideration the quantities of work done by these molecular forces, however great the same may be individually, inasmuch as they mutually and completely cancel each other.

If, during the passage of a stationary current through the conductor, we examine, at the commencement and at the close of a unit of time, the portion before separated and enclosed by a surface, we shall find that during the interval the condition of this portion has suffered no material change. Of course the electro-positive constituents of many molecules will have separated themselves from the electro-negative ones with which they were hitherto united; but to compensate this, they have united themselves again with other perfectly equal constituents, and the work performed by the molecular forces during such a combination is undoubtedly just as great as that which they suffer or perform negatively during such a separation. Similarly, the particles leaving the enclosed space on the one side are replaced by an equal number entering on the other, so that at the end of the time under consideration the whole mass in this space has the same density, the same constitution, and the same arrangement of molecules as it had at the commencement. Hence, although ignorant of the quantities of work done in the several processes by the molecular forces, we may safely draw the conclusion that the algebraical sum of all such quantities of work is zero. Consequently there remains only the work performed by the moving electric force when overcoming resistance to conduction, which work, having produced no permanent change in the conductor, must have been converted into *vis viva*, and the latter, inasmuch as no other *vis viva* is present, into heat.

Accordingly, if H represent the heat generated in the above space during the unit of time, and A be the equivalent of heat for the unit of work, we may deduce from the foregoing equation the following:

$$\mathrm{H}=\mathrm{A}k\int \mathrm{V}\frac{d\mathrm{V}}{d\mathrm{N}}\,d\omega.$$

I have already shown in my former memoir that this equation includes, as a particular case, the empirical laws established for a linear conductor, according to which the heat generated is proportional to the resistance to conduction and to the square of the intensity of the current.

4. We will now enter more into the details of the conception to be formed of the manner in which the conduction of electricity takes place within an electrolyte.

The molecules of the electrolyte are resolved by the current into two constituents, which may either be simple atoms, or themselves consist of several atoms combined into molecules. For example, in sulphate of copper the one constituent, Cu, is simple, the other, SO^4, compound. These constituents, whether simple or compound, I will call *partial molecules*, should it be necessary to distinguish them from a *complete molecule* of the electrolyte.

From the connexion between the decomposition of an electrolyte and the conduction of electricity, we are led to the conclusion that the two partial molecules forming a complete molecule are in opposite electric conditions, which conditions remain even after separation. On the hypothesis of two electric fluids, therefore, we must assume that the one partial molecule has an excess of positive, and the other an equal excess of negative electricity; under the hypothesis of one electric fluid, we must assume, however, that the one partial molecule possesses more, whilst the other possesses less electricity than is necessary for the neutral condition.

It is readily conceivable that two molecules of different natures may, during their contact, assume such opposite electric conditions. Neither is it difficult to conceive that these conditions may be retained, even after separation, provided we also assume that no accumulation of positive or negative partial molecules alone takes place in any part of the conductor—in other words, that both kinds of partial molecules are everywhere so uniformly distributed, that in any given space there are an equal number of each kind. For in this case, owing to the opposite actions of positive and negative partial molecules, the forces exerted by the electricities of the surrounding partial molecules upon the electricity adhering to any one cannot occasion any strong resultant having a tendency to move the latter electricity in any definite direction, and thus to separate it from its molecule should the motion of that molecule be impeded.

On the other hand, however, if a large number of molecules charged with the same kind of electricity were congregated in any space, the electricity of any one of them would be repelled by the electricities of all the rest; and unless the molecule in question happened to be in the middle of the mass, these repelling forces, by combining, might occasion a considerable resultant force acting from within outwards. Moreover, inasmuch as the electricities clinging to all other molecules would be subjected to exactly similar actions, each being forced outwards by the total action of all the others, a certain tension would necessarily prevail in the electric condition of the whole mass, which tension could only sustain itself unchanged in the case of this mass being absolutely non-conducting. In other cases the free electricity of all molecules would, according to the conductibility of the mass, flow more or less quickly, first towards its surface, and thence, unless this mass was perfectly insulated, into surrounding space.

5. Further, if we consider the actual process of decomposition which occurs in the liquid serving as electrolyte or containing the electrolytes in solution, we must, at any rate, grant in the first place, that the partial molecules set free at one electrode do not move through the liquid to the other electrode, but that decompositions and recombinations occur everywhere throughout the whole mass of liquid between the two electrodes, so that the positive partial molecules which in the unit of time reach the cathode correspond *in number* to, but are not *identical* with, those which leave the anode; and similarly with respect to the negative partial molecules which arrive at the anode.

The mutual dependence between the decompositions occurring in the several liquid strata, however, requires to be established more accurately, and there is one opinion, particularly, which, being decidedly erroneous although apparently plausible, must once for all be rejected.

In fact, one might possibly conceive that the decomposition started from one electrode, *e. g.* the anode, that the negative partial molecules of the decomposed complete molecules were here retained, whilst the positive ones proceeded to the next stratum of liquid, and there determined a new decomposition by combining with the negative partial molecules of that stratum, and liberating the positive ones; that the latter again withdrew to the following stratum, and there once more occasioned the same effects, and so on. According to this, the decomposition of one stratum would cause that of the next following, and the action of the moving force in the conductor would be limited, first, to moving the liberated partial molecules of the first stratum to the following one, and secondly, to facilitating the decomposition by also pressing forwards the positive partial molecules of this latter stratum.

The falsity of this view is at once proved by the fact that, according to it, an excess of positive partial molecules, and consequently of positive electricity, would always exist within the liquid during the current; as already mentioned, this hypothesis —according to the laws of the distribution of electricity—is just as inadmissible in the case of a stationary current as in that of a state of equilibrium. In the same manner, if we were to assume that the above-described method of propagation of the decomposition took place in a contrary direction, from the cathode to the anode, we should obtain an excess of negative partial molecules within the liquid, which would of course be equally untenable.

As a basis for all further considerations, we must adhere to the principle, *that every measurable portion of the liquid contains an equal number of positive and negative partial molecules*, no matter whether the same are combined in pairs to form complete molecules, or whether some are distributed in an uncombined state, amongst these complete molecules.

From this it follows, that in an electrolytic liquid in its natural state, wherein neither kind of partial molecules preponderates, such decompositions and recombinations as are necessary to the conduction of electricity may take place under the sole influence of the force which serves to overcome the resistance to conduction.

The explanation of this fact presents a peculiar difficulty, which, it appears to me, can only be surmounted by assuming a deportment of liquids altogether different from that ordinarily accepted. In the following paragraphs I will endeavour to elucidate this.

6. Let us conceive a given liquid consisting either entirely or partially of electrolytic molecules, and let us in the first place assume that during the natural state of the liquid these molecules have arranged themselves in some definite order, in which

they will remain as long as no foreign force acts on them, so that the several molecules, although capable of oscillating about their positions of equilibrium, are not able to quit the same entirely; further, in accordance with the assumption which such an arrangement necessitates, let the attraction between two partial molecules combined into one complete molecule (and consequently in close proximity to each other) be greater than the attraction between the positive partial molecule of one complete molecule and the negative one of another. If within this mass an electric force, whose tendency is to drive the positively electrified partial molecules in one direction, and the negatively electrified ones in the opposite direction, begins to act, the question arises, what influence will the same exert upon the deportment of the molecules.

The molecules being supposed capable of turning round a point, the first effect would clearly be to give them all a like direction, so that the two oppositely electric constituents of each complete molecule would turn towards the sides to which they are solicited by the acting force.

The force would further strive to separate the constituents of each complete molecule and cause them to move in opposite directions, so that the positive constituent of each complete molecule would encounter and combine with the negative constituent of the next following molecule. But in order to separate the already combined partial molecules, the attraction which they exert upon each other must be overcome, for which a force of a certain intensity is necessary; hence we are led to the conclusion, *that so long as the force acting within the conductor does not possess this requisite intensity, no decomposition whatever can take place; but that, on the other hand, as soon as the force has attained this intensity, a great many molecules must be simultaneously decomposed, inasmuch as all are exposed to the influence of the same force, and have almost the same relative positions to each other.* If we assume that the conductor conducts only in virtue of electrolysis, then with reference to the electric current, this conclusion may be thus expressed:—*So long as the moving force acting within the conductor is below a certain limit, it causes no current whatever; so soon as it attains this limit, however, a very strong current is suddenly produced.*

This conclusion, however, is in direct contradiction to experiment. The smallest possible force* gives rise to a current

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* I must once more expressly state, that here and elsewhere, throughout the whole of this memoir, I do not refer to the forces which act at the electrodes, where the results of decomposition are separated and polarization must be overcome, but solely to the force which acts within the electrolyte itself, where each partial molecule, separated from the one previ-

accompanied by alternate decompositions and recombinations, and the intensity of this current increases in proportion to the force, according to Ohm's law.

The above hypothesis, therefore, according to which the partial molecules of an electrolyte are combined in a definite manner to form complete molecules, which latter also possess a certain regular arrangement, must be incorrect. This result may be still more generally expressed thus:—Every hypothesis is contradictory to Ohm's law which involves the assumptions that the natural state of an electrolytic liquid is one of equilibrium, wherein each positive partial molecule is combined in a fixed manner with a negative one; and, further, that a force of determinate intensity acting on these molecules is necessary in order to transform this state of equilibrium into another essentially similar to the former,—differing, in fact, from the same only in so far that a number of positive partial molecules, instead of being combined with the same negative ones as before, are combined with others.

I am consequently of opinion that the following hypothesis, which does not involve this contradiction, and which, it appears to me, is also in harmony with other known facts, deserves some consideration.

7. In my memoir "On the Nature of the Motion which we call Heat"[*], I have expressed the opinion that in liquids the molecules have not determinate positions of equilibrium around which they merely oscillate, but that their movements are so active that they are thereby translated into quite different, and even new positions towards each other, and that they move irregularly amongst each other.

Conceive now, in the first place, a single, *e.g.* an electro-positive, partial molecule of an electrolytic liquid, and let us assume that its electric state is as yet exactly the same as at the moment of its separation from a complete molecule. I can imagine that in moving about between the complete molecules, amongst the many positions which this partial molecule may assume, there exist some wherein it will attract the negative partial molecule of some complete molecule with a force greater than that which the partial molecules—whose relative position, too, is not quite unchangeable—of this complete molecule exert upon one another. As soon as it attains such a position it will combine with this negative partial molecule, and the positive one before combined with the same will be set free. The latter will now also wander

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ously combined with it, recombines immediately with another partial molecule of the same kind as the latter, so that essentially the mass remains unchanged, and the resistance to conduction has alone to be overcome.

[*] Poggendorff's *Annalen*, vol. c. p. 353; Phil. Mag. vol. xiv. p. 113.

about alone, and after some time decompose another complete molecule in the same manner, and so on; and all these motions and decompositions will take place just as irregularly as do the motions of heat, to which they owe their origin.

Further, with respect to the deportment of the complete molecules towards one another, I am inclined to think that here, too, it may sometimes happen that the positive partial molecule of some complete molecule may assume towards the negative one of some other a position more favourable than that enjoyed at the moment by either of these two partial molecules with reference to the other partial molecule of its own complete molecule. In such a case the two first partial molecules, which were before estranged, will combine to form a complete molecule, and the two others thereby set free—*i. e.* the negative constituent of the first, and the positive of the second complete molecule—will at the same time either combine with each other, or, should the motion of heat prevent their so doing, mix with the other complete molecules, and there produce decompositions such as we before described with reference to a single partial molecule.

The frequency of such mutual decompositions in a given liquid will depend, in the first place, upon the nature of that liquid, with respect to the greater or less intimate connexion between the constituents of the several complete molecules; and secondly, upon the activity of the molecular motion, or, in other words, upon the temperature.

8. If now, upon a liquid whose molecules already move about amongst each other in such a manner as to occasion an irregular exchange amongst their partial molecules, an electric force acts so as to cause a tendency in all positive partial molecules to move in one direction, and in all negative ones to move in the opposite direction, the difference which must thereby be originated in the nature of the molecular motion is easily conceivable.

A free partial molecule will no longer follow altogether the irregular and variable directions in which it was solicited by the motions of heat, but it will change its course in obedience to the acting force; so that amongst the still very irregular directions followed by the free, positive, partial molecules, some determinate one will predominate, and at the same time the negative partial molecules will, for the most part, move in a direction opposite to the last. Further, in the action of a partial molecule upon a complete one, as well as in the mutual action of two complete molecules, all decompositions which at the same time permit the partial molecules to move in obedience to the electric force will be facilitated, and consequently will occur more frequently than they could do in the absence of such a force; for even in cases where the position of the molecules is not yet sufficiently favourable to cause spontaneous decomposition, the cooperation of the electric force may hasten the same. On the contrary, all decompositions which necessitate a motion of the partial molecules opposed to the electric force will be retarded by the latter, and thereby rendered less frequent.

Let us consider a superficial element within this liquid perpendicular to the direction of the electric force acting at the moment; *in the unit of time there are more positive partial molecules passing through this element of surface in the positive than in the negative direction, and more negative partial molecules in the negative than in the positive direction.* But as for each kind of partial molecules, two oppositely-directed passages mutually destroy each other's action, and as it is only the residual excess of passages in one direction which demands consideration, the foregoing may be more simply expressed thus:—*a certain number of positive partial molecules traverse the element of surface in the positive direction, and a certain number of negative partial molecules traverse it in the negative direction.* The magnitudes of these two numbers need not be equal, since they depend not only upon the moving force, which is the same for both, but also upon the degree of mobility, which, for many reasons, may be different for different kinds of partial molecules.

These opposite motions of the two kinds of partial molecules constitute the galvanic current within the liquid. In order to determine the intensity of this current, it is not necessary to know how many positive partial molecules traverse the element of surface in the positive, and how many negative ones in the negative direction; it will suffice if merely the sum of these two numbers is known. For, no matter whether we start from the hypothesis of two electricities, and assume that a negative partial molecule possesses a certain quantity of free negative electricity, or from the hypothesis of one electric fluid, and conceive a negative partial molecule to possess less electricity than is necessary for the neutral condition, in both cases we must assume that a galvanic current is intensified just as much by the motion of an electro-positive partial molecule in the direction of the current, as it is by that of an equally charged electro-negative partial molecule in the opposite direction. Consequently if, in the case of a molecular motion where an excess in one direction exists for positive partial molecules alone, we represent by $C.n$ the intensity of the current corresponding to the passage of n positive partial molecules in the positive direction through the element of surface in the unit of time, then, in a similar manner, $C(n+n')$ will represent the strength of the current when simultaneously n positive partial molecules and n' negative ones respectively traverse the element of surface in the positive, and in the negative direction.

9. In this conception of the condition of the liquid, the abovementioned difficulty disappears. It is at once seen that the influence exerted by the electric force upon the already existing, though irregular molecular decompositions and motions commences at once in the manner above described, and increases with the intensity of the force instead of remaining ineffective until the force has reached a certain limit of intensity. The whole process, therefore, agrees very well with Ohm's law.

Why the electric conductibility, which depends upon the facility with which the molecular decompositions take place within the liquid, should be so different in different liquids; why, for example, these decompositions take place so much more readily in the molecules of hydrated sulphuric acid than in those of water; and why the dilution of sulphuric acid should have so remarkable an effect on its conductibility,—are questions which certainly have not yet received satisfactory answers. Nevertheless, I see nothing therein which can be regarded as contradictory to the foregoing theory.

On the other hand, the increased conductibility in conductors of the second class attendant upon an increase of temperature, is explained very naturally by this theory; for greater activity in molecular motion will evidently tend to facilitate the mutual decomposition of these molecules.

By comparing the present theory with the former one of Grotthuss, the principal difference will be found to consist in the circumstance, that in the latter the molecular motions are assumed to be originated by the electric force, and to take place in two definite directions only, the decompositions progressing regularly from molecule to molecule; whereas in the present theory the motions which already exist are merely modified, not so as to become perfectly regular, but merely to such an extent that in the great variety of motions which still exist the two definite directions predominate.

10. After I had committed to paper the foregoing view of the deportment of electrolytic liquids, I learned from a conversation with a chemist, that similar views respecting the deportment of compound liquid and gaseous bodies had been already expressed by Williamson in a memoir on the Theory of the Formation of Æther[*]. In this memoir we read[†], "We are thus forced to admit, that, in an aggregate of molecules of any compound, there is an exchange constantly going on between the elements which are contained in it. For instance, a drop of hydrochloric acid being supposed to be made up of a great number of mole-

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[*] Phil. Mag. November 1850, p. 350.

[†] Ibid. p. 355.

cules of the composition Cl H, the proposition at which we have just arrived would lead us to believe that each atom of hydrogen does not remain quietly in juxtaposition with the atom of chlorine with which it first united, but, on the contrary, is constantly changing places with other atoms of hydrogen, or, what is the same thing, changing chlorine."

According to this, Williamson appears to assume even a greater mutability in the grouping of the partial molecules than is necessary for the explanation of the conduction of electricity. He speaks of constant changes of place between one atom of hydrogen and other hydrogen atoms, whereas, in order to explain electric conduction, it is sufficient if the impact between complete molecules is occasionally, and perhaps, comparatively speaking, rarely accompanied by an interchange of partial molecules.

In confirmation of his views, Williamson adduces the deportment of a liquid in which two compounds consisting of different electro-positive and different electro-negative constituents are dissolved: in such a case, the two original compounds do not simply continue to exist, neither does another arrangement ensue in which an electro-positive constituent is exclusively combined with one of the two electro-negative constituents, and *vice versâ*; on the contrary, all the four possible combinations occur in certain proportions, whence it happens that when one of the four combinations is insoluble, the same becomes precipitated. I also think that this deportment receives a very natural explanation from the assumption, that the combinations of each pair of partial molecules, instead of being fixed, are mutable, and that a positive partial molecule may not only displace another of the same kind, but also a positive partial molecule of a different kind; in the exposition of the foregoing theory, this deportment has been kept in view. Nevertheless, here also I regard it as unnecessary that all molecules should be subject to incessant change; occasional mutual interchanges amongst the same appear to me to be sufficient, for even if the number of interchanges be small in comparison to the number of impacts, still, considered alone, this number may be very great, and consequently it may cause, in a short time, a considerable change in the original mode of combination.

As I arrived at the conclusions concerning the interchanges which take place amongst the partial molecules of a liquid in a manner totally different from, and independent of that of Williamson, I thought it advisable, even after becoming acquainted with his memoir, to communicate my own views of the subject unchanged, since, by so doing, I shall best render manifest how far these two methods of viewing the subject serve mutually to corroborate each other.

11. The question has lately been several times discussed, whether, in conductors of the second class, besides the conduction by means of electrolysis, another kind similar to that in conductors of the first class also takes place.

From a theoretical point of view, the hypothesis of the coexistence of both kinds of conduction in one and the same body appears to me admissible. The determination, however, in particular cases of the quantitative relation between the two kinds of conduction, must, for the present at least, continue to be referred to experimental research, in consequence of the absence of accurately established facts whereon to base theoretical conclusions.

With respect to the bodies hitherto examined in this respect, and which, on account of their frequent application, are most important, it has been found that conduction without electrolysis, if it at all exists, is at any rate very small; hence it will not here be necessary to consider this kind of conduction, which, indeed, would offer little essentially new in a theoretical point of view.

12. Lastly, there remains a phænomenon to be mentioned, which, in many cases, accompanies electric conduction: I refer to that of the translation of the liquid in the direction of the positive current which accompanies the passage of the latter through a porous partition.

Wiedemann* especially has carefully examined this phænomenon, and by so doing, has arrived at a series of laws of importance in the theory of the phænomenon. Towards the end of his first memoir, many of these laws are comprised in the following theorem:—"An electric tension which exists on both sides of a porous partition immersed in any liquid, transports the latter from the positive to the negative side with a force equal to a hydrostatic pressure directly proportional to that tension."

What Wiedemann here calls the electric tension, is, in mathematical language, the difference between the values of the potential function of the free electricity at the two limiting surfaces of the porous partition; hence if we bear in mind in what manner the electric force acting within the porous partition, and occasioning the current, depends upon that difference, we shall find that the above theorem may be more simply expressed thus:—*The force which drives the liquid through the porous partition is proportional to the force which drives the electricity through the same.*

This principle is so simple, that one might, perhaps, be thereby

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* Poggendorff's *Annalen*, vol. lxxxvii. p. 321, and vol. xcix. p. 177. Also Phil. Mag. vol. iv. p. 546.

induced to ascribe to it a general validity, and thus to assume that in all electric conduction through electrolytes, a certain relation exists between the force which moves the electricity within the liquid, and a second force which drives forward the liquid itself, and hence that every explanation of electric conduction wherein this latter force is not taken into consideration, must necessarily be incomplete. This opinion, however, would be incorrect. The principle can only be applied to cases such as those for which it was discovered by Wiedemann. The liquid must be in a porous partition, and when, for comparison, many porous partitions are employed, they must all be of the same nature. There are many facts which would lead us to conclude, indeed recent experiments by Breda and Logeman* have proved, that in a continuous liquid mass the above second force is either altogether absent or vanishingly small. Hence the force under consideration is not generally connected with electric conduction, but acts solely within a porous partition; in other words, this force only acts when, instead of forming a continuous mass, the liquid is divided into numerous very small canals.

As yet we possess no successful explanation of this phænomenon; the notion I have formed for myself concerning the same, is still too hypothetical to warrant its communication. As far as the present memoir is concerned, it will suffice if it be admitted that, in the foregoing considerations concerning the manner in which electric conduction takes place within the continuous mass of an electrolytic liquid, it was not necessary to take the above phænomenon into consideration.

13. In one respect only do I feel induced to make the above phænomenon the subject of a few remarks; that is to say, with respect to the simultaneous production of work and generation of heat.

In the first place, it can scarcely be doubted that the motion of the liquid through the partition is caused by the same free electricity which gives rise to the current of electricity within the partition. Further, in accordance with all known effects of electricity, we must assume that the force proceeding from free electricity, can, in return, only act upon electricity, and upon molecular masses only in so far as they are electrified. We may consequently affirm that the immediate effect of the force exerted by the free electricity consists solely in the motion of electricity, and the motion of material masses is a secondary effect connected with this motion of electricity.

Hence the whole work performed by the electric force within a porous partition may be determined just as simply as in other conductors. In fact, it will be represented by the increase of

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* *Biblioth. Univ.* vol. xxxiii. p. 1, and Poggendorff's *Annalen*, vol. c. p. 149.

the potential of the free electricity upon the electricity flowing through the partition.

Within a porous partition the effects of this work, as transferred to the material masses, are in some respects different from those in a continuous liquid mass. A portion, and in most cases by far the greater portion of the work, will be employed in overcoming, in the ordinary manner, the resistance to conduction in decompositions and recombinations of molecules, and in opposite motions of the partial molecules. This portion of the work within the partition will be completely converted into heat. Another portion of the work will, on the other hand, be employed in moving the whole liquid, and in reference to this latter portion, a twofold deportment may manifest itself.

We must distinguish between the cases when the liquid, in passing through the partition, has, and has not to overcome a counter-pressure, which latter may be considered as a hydrostatic pressure. If the liquid encounters no counter-pressure, then there is only the friction within the partition to be overcome, and the portion of the work employed in doing so will also be completely converted into heat. When a counter-pressure exists, however, sufficiently great to impede, but not to prevent the translation of the liquid, then the work employed in forcing the liquid forwards is itself smaller because there is less liquid moved; and of this smaller quantity of work, only a part is converted into heat, the other being consumed in overcoming the hydrostatic pressure, or, as we may express it, annulled by the negative work of the hydrostatic counter-pressure. Lastly, when the counter-pressure is so great, that the force driving the liquid through the partition is thereby held in equilibrium, when, consequently, no translation of the liquid occurs, then that portion of the work devoted to the same is also zero.

These different cases may be thus comprised. In the two limiting cases, where no counter-pressure exists, and where the latter is so great that no translation takes place, the whole work done by the electric force is converted into heat; on the contrary, in the intermediate cases a portion of this work is employed in overcoming the hydrostatic counter-pressure.

14. The magnitude of the work employed in forcing the liquid through the partition, and that part of the same spent in overcoming the counter-pressure, may be easily determined. If we represent by weights to the unit of surface the force which drives the liquid through the partition, and which may also be conceived to be replaced by a hydrostatic pressure, we shall obtain the work done by the driving force in a given time, by simply multiplying the above weight by the volume of liquid driven through the partition in that time. In the same manner we

obtain the negative work of the counter-pressure by multiplying the volume of the liquid by this pressure.

In order to obtain an approximate estimate of the values of these quantities of work, we will, as an example, assume that the driving force is sufficiently great to overcome a hydrostatic counter-pressure of a whole atmosphere. This force, expressed in kilogrammes to a square metre, is then 10333, and in order to obtain the work, expressed in kilogramme-metres, we must multiply the above number by the volume of the liquid driven through the partition and expressed in cubic metres. For instance, if, during a given time, a litre or 0·001 cubic metre is driven through the partition, the correspondong work will be

$$10333 \,.\, 0{\cdot}001 = 10{\cdot}333 \text{ kilogramme metres.}$$

This work, if converted into heat, would give

$$\frac{10{\cdot}333}{423{\cdot}55} = \frac{1}{41} \text{ of a unit of heat,}$$

consequently as much heat as would raise the temperature of a litre of water, weighing a kilogramme,

$$\frac{1}{41} \text{ of a degree C.,}$$

or that of a litre of the liquid driven through

$$\frac{1}{41 \,.\, c \,.\, s} \text{ of a degree C.,}$$

where c is the specific heat, and s the specific gravity of the liquid.

This result, in which definite values have been assumed merely for the sake of simplicity, may be immediately generalized. Let p represent the force with which the liquid is driven through the partition, the same being conceived to be replaced by a hydrostatic pressure and expressed by atmospheres; then the work done by this force in driving a given volume of the liquid through the partition, would, if converted into heat, raise the temperature of the volume of liquid in question

$$\frac{p}{41 \,.\, c \,.\, s} \text{ degrees C.}$$

This work is so small, that it can form but a small portion of the whole work done by the electric force within the porous partition. In badly-conducting liquids only can it be, comparatively, more considerable, since with the same strength of current a greater quantity of a bad than of a good conducting liquid passes through the partition.

From the connexion which exists between the motion of electricity and that of a liquid through a porous partition, it appears to me probable, that the difference between the values of the potential function on both sides of the partition remaining the same, the quantity of electricity passing through the partition in the unit of time will be somewhat smaller when the translation of the liquid is obstructed by a hydrostatic counter-pressure, than when this is not the case; in other words, the translation of the liquid being more obstructed, the partition offers a greater resistance to conduction. For the reasons above stated, however, the difference can be but small in liquids which do not conduct very badly.

Zurich, April 25, 1857.

XV. *Chemical Notices from Foreign Journals.*
By E. Atkinson, *Ph.D.*

[Continued from vol. xiv. p. 278.]

FOR the preparation of colourless crystals of alumina, or artificial sapphires, Gaudin* gives the following process:—A mixture of equal parts of alum and sulphate of potash, which have been previously ignited and powdered, is introduced into a crucible lined with lampblack, and the crucible is then heated for a quarter of an hour in a blast-furnace. When the crucible is cold, a mass full of brilliant points is found in the cavity of the charcoal. It consists of sulphide of potassium enclosing crystals of alumina, which are large in proportion to the size of mass operated on. The crystals are obtained in the form of fine sand by treating the mass with dilute aqua regia, and subsequent washing with water. The crystals are harder than native rubies, quite transparent and colourless, for coloured metallic oxides are reduced by the carbon. Gaudin ascribes the formation of these crystals to the fact that sulphide of potassium is formed, which acts as a solvent for alumina at a high temperature, and on cooling alumina crystallizes out. He obtained the crystals also by heating together calcined alumina and sulphide of potassium in a luted crucible.

In the course of some experiments on titanium, made with a view of investigating its properties in the compact form, Wöhler and Deville† have found that a great affinity exists between that metâl and nitrogen;—that titanium, at least at the moment of its formation, can unite directly with the nitrogen of the atmosphere. Nothing is more difficult than the preparation of pure metallic titanium. It can only be obtained so with certainty by

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* *Comptes Rendus*, April 6, 1857.

† Liebig's *Annalen*, August 1857.

the action, at a high temperature, of sodium on the double fluoride of titanium and potassium in an atmosphere of hydrogen. It is a dark gray powder, like iron reduced by hydrogen. The substances hitherto obtained in reduction experiments, and described as titanium, really consist of nitride of titanium. Of the two constituents of the atmosphere, oxygen is that which offers most resistance to the preparation of elements in their pure form; but in the case of titanium, it is nitrogen. A crucible lined with charcoal, and filled with a mixture of titanic acid and charcoal, is impermeable to the oxygen of the furnace, while it is penetrated with ease by the nitrogen.

Wöhler and Deville mixed titanic acid and charcoal in the proportion in which, by assimilation of nitrogen, they would form the cubic titanium of the blast-furnaces, $\mathrm{Ti\,C^2\,N + 3Ti^3\,N}$. A small charcoal tray was filled with this mixture, and heated in a tube of dense carbon*, externally protected from combustion, to a temperature at which platinum melts; at the same time a stream of pure, dry nitrogen was passed through the tube. When the temperature had reached a certain point, and the gas passed through in a rapid stream, the tray became perceptibly more incandescent than the surrounding tube; and the nitrogen was so rapidly absorbed, that nothing but pure carbonic oxide issued from the other end of the apparatus. The apparatus was then allowed to cool, the passage of the gas being continued. The contents of the tray were then found to be changed into a crystalline substance of metallic lustre, which had all the chemical and physical properties of the titanium of the blast-furnaces. When this substance is heated in a lime-crucible to a temperature at which rhodium becomes liquid, it was found to be diminished in volume and covered with small octahedral crystals.

It would seem from this experiment, that nitrogen in its free state has such an affinity for titanium, that it can unite with it; at any rate if it comes in contact with it at a high temperature at the moment of its formation.

On heating a mixture of charcoal and titanic acid in a carbon crucible to a high temperature, a large quantity of cubic titanium was formed. It was also formed by the action of sodium upon fluoride of titanium and potassium in a tube heated to redness, through which nitrogen was passed.

Another instance of this unexpected property of nitrogen has been made known by Wöhler and Deville†, who, in continuing their investigation of boron, have found that this element also possesses the property of directly combining with nitrogen.

Amorphous boron they now prepare by an easy and expeditious

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* Phil. Mag. vol. xiii. p. 124.

† Kosmos, Nov. 27, 1857.

method, which has already furnished them 500 or 600 grammes of this substance. 100 grms. of boracic acid, fused and coarsely powdered, are mixed with 60 grms. of sodium, and the mixture projected into a red-hot iron crucible. The whole is covered with 40 or 50 grms. of fused salt, the crucible closed, and when the action is over, the contents stirred about with an iron rod. The mass, which contains boron swimming in a flux of boracic acid, borate of soda, and chloride of sodium, is poured, while still hot, into water acidulated with hydrochloric acid, then washed on a filter, first with acidulated water until free from boracic acid, and finally with distilled water. The boron remains on the filter and is dried in the air on porous tiles.

When amorphous boron is heated in a current of ammonia, it appears to take fire, incandescence ensues, and the ammonia is decomposed, the nitrogen combining with the boron to form nitride of boron, and the hydrogen escaping. Boron, or a mixture of boracic acid and charcoal strongly heated in a current of pure nitrogen, is entirely converted into nitride of boron. Boron cannot therefore be heated in crucibles in ordinary furnaces without being changed into nitride. Amorphous boron may be readily converted into crystallized boron by lining a crucible with amorphous boron, and placing in the cavity a piece of aluminium. At an elevated temperature the aluminium becomes charged with boron, which crystallizes out on cooling, and may be obtained by dissolving the aluminium either in soda or in hydrochloric acid. To prevent the boron from absorbing nitrogen, the crucible containing it is placed inside another crucible, lined with a mixture of titanic acid and charcoal, which stops both the nitrogen and the oxygen.

At a full red heat boron decomposes the vapour of water, with the production of hydrogen and boracic acid. Part of the boracic acid volatilizes with the vapour of water, and crystallizes at some distance from the point at which the tube in which the experiment was made is heated.

When heated in sulphuretted hydrogen, boron combines with the sulphur, hydrogen being liberated, and the sulphide of boron volatilizes in the excess of sulphuretted hydrogen, just as boracic acid in the vapour of water; and the sulphide of boron is obtained crystallized at some distance from the point at which it is formed.

At a very moderate temperature hydrochloric acid gas is decomposed by boron, with evolution of light and formation of chloride of boron; and bromide of boron is similarly obtained. The chloride and bromide of boron are not gases, as generally stated, but liquids, of which the first boils at 17° and the latter at 90° C.

Amorphous boron possesses properties as a reducing agent which place it near carbon. Its affinity for chlorine is so great, that the chlorides of lead, mercury and silver, are reduced at a high temperature with formation of chloride of boron. Galena is also reduced by boron, lead and sulphide of boron being the products.

Brunner* obtained metallic manganese by the process by which aluminium is obtained—the reduction of its fluoride by means of sodium. He brings two parts of fluoride of manganese and one of sodium in alternate thin layers into a crucible, presses the two well, covers them with a layer of dry chloride of sodium, and then with a layer of small pieces of fluor-spar to prevent spurting. The crucible with a cover is placed in a wind-furnace and gently heated till the first action is over, which is perceived by a hissing in the crucible, and the issuing from it of a yellow flame; the crucible is then raised to a bright red heat and maintained in this state for a quarter of an hour, after which all the openings of the furnace are closed to allow the crucible to cool gradually. The manganese is then found melted at the bottom of the crucible. If it is not fused together, it is again melted beneath a cover of chloride of sodium to which one-tenth of saltpetre has been added.

The manganese thus obtained has the colour of cast iron; it is very brittle, and so hard that it scratches even the best hardened steel, and cuts glass like a diamond. It is capable of a high polish; does not change at the ordinary temperature in moist air: heated on platinum-foil it tarnishes with the same succession of colours as steel, and by continued heating becomes covered with a coating of brown oxide. It is not attracted by the magnet, and does not attract the magnetic needle. It has the specific gravity 7·138 to 7·206. By the action of strong sulphuric acid in the cold a little hydrogen is disengaged: heated with the same acid, it dissolves with evolution of sulphurous acid. Dilute sulphuric, nitric, hydrochloric, and acetic acids dissolve it with facility.

Fremy† prepared metallic chromium and manganese by the action of sodium on their respective chlorides. He caused the vapour of sodium carried in a stream of hydrogen to act upon the metallic chlorides, heated in a porcelain tube to a bright red heat. In both cases the metal separates out in a crystalline form. Fremy only describes the chromium. Freed from chloride of sodium by washing, it has the form of brilliant crystals

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* Liebig's *Annalen*, June 1857. Poggendorff's *Annalen*, June 1857.

† *Comptes Rendus*, March 23, 1857. Liebig's *Annalen*, June 1857.

which belong to the cubic system. They are very hard, and resist the action of aqua regia. Many alloys of chromium with other metals possess the hardness of the former, and, like it, resist the strongest acids. An alloy of chromium and iron was obtained by reducing chrome iron ore with charcoal, and also by the action of iron upon oxide of chromium. This alloy often crystallizes in long needles. It resembles cast iron; it scratches the hardest bodies, even steel. Green oxide of chromium melts in a blast-furnace, and then forms a black crystalline mass; it scratches quartz and steel with ease.

St.-Claire Deville*, adverting to his previous researches† on this subject, points out that by the method of Brunner the presence of impurities is not excluded. Most sodium contains carbon, and being porous, encloses much rock-oil in its pores. Silicon would also infallibly be reduced from the substance of the Hessian crucible by means of sodium. That the manganese prepared by Brunner is more fusible, and decomposes water less readily than that of Deville, may be hence explained.

At a red heat sodium attacks porcelain energetically, setting silicon free; and metals prepared in vessels of porcelain would always be likely to contain silicon. Fremy's chromium may therefore have contained silicon, and this might explain the differences between this chromium and that prepared by Deville by reduction from the oxide in lime-crucibles. The latter, as well as that which Bunsen prepared by electrolysis, dissolve easily in hydrochloric acid, while that of Fremy resists the action of aqua regia.

Deville discusses the conditions of preparing metals free from impurities, and points out the advantages presented by the mode of reducing the oxide mixed with an insufficient quantity of charcoal in a lime-crucible. In this case the lime absorbs the excess of oxide. Debray and himself have found that platinum prepared in small lime-crucibles has quite a different appearance to the ordinary metal, because it has been deprived of the osmium and the silicon which the latter always contains.

The application of sodium in preparing the metals is advantageous in cases in which it is wished to obtain the metals in the crystalline form.

If, according to Pauli‡, pentasulphide of phosphorus intimately mixed with an excess of sal-ammoniac be heated in a retort over the gas-lamp, hydrochloric and hydrosulphuric acids are disengaged, together with sal-ammoniac, and after some time yellow sulphide of ammonium distils over. When the evolution of gas

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* *Comptes Rendus*, March 30, 1857.

† Phil. Mag. vol. xiii. p. 124.

‡ Liebig's *Annalen*, January 1857.

has ceased, the residue is taken out of the retort and again heated, the process being repeated as long as sal-ammoniac distils off.

The residue presents then the form of a powder, light like magnesia. It is insoluble in water and in fuming nitric acid; fused with caustic potash, it disengages ammonia, and at a red heat is completely decomposed by sulphuretted hydrogen into sulphide of phosphorus and ammonia. Fused with zinc, it is decomposed, ammonia being liberated. It therefore contains hydrogen.

The analytical data gave numbers which agree with the theory of its being a mixture of phosphuret of nitrogen, PN^2, and phosphamide, PN^2H. The formation of the latter would be thus expressed:—

$$3NH^4Cl + PS^5 = PN^2H + 4HS + 3HCl + NH^4S.$$

Rose ascribed to phosphuret of nitrogen prepared by the action of ammonia on terchloride of phosphorus, and which has the same properties as Pauli's compound, the formula PN^2. But his analyses gave a small constant quantity of hydrogen, which he however neglected. Gerhardt's analyses led to the formula PN^2H; and a recent qualitative analysis of a specimen of this compound, which had been prepared by Liebig and Wöhler, was found by Schiff[*] to contain hydrogen.

Fresenius[†] has observed the occurrence, in the water of the mineral spring at Weilbach, of traces of formic and propionic acids; and Kraut[‡] found in a spring water, butyric and caproic acids. The water was taken from a brook which arose in a marshy neighbourhood; it contained the usual mineral constituents, and an almost equal quantity of organic substances. A special experiment showed that the fresh water contained merely traces of caproic and butyric acids, and that the greater or perhaps entire part was formed by the decomposition of the organic substances.

According to Wöhler[§], molybdic acid may be at once obtained pure and sublimed by heating native sulphide of molybdenum in pieces in a glass tube through which a stream of atmospheric air is passed until the molybdenum is oxidized.

A new mode of forming silicon has been made known by St.-Claire Deville[||] and Caron in a communication on silicon and its alloys. It consists in taking advantage of the solubility of silicon in zinc.

An earthen crucible is made red-hot, and a carefully prepared mixture of three parts of silico-fluoride of potassium, one part of

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[*] Liebig's *Annalen*, March 1857.

[†] *Journal für Praktische Chemie*, January 1857.

[‡] Liebig's *Annalen*, July 1857. [§] Ibid. December 1856.

[||] *Comptes Rendus*, August 3, 1857.

zinc, and one part of sodium, is projected into it. The reduction of the silicon is accompanied by a very feeble action, which is not enough to produce complete fusion of the substances; and it is necessary to heat the crucible to redness, and to maintain it in this state until the contents are completely fused.

The heat must not be so high that the zinc would volatilize; the crucible is allowed to cool, and when broken is found to contain a regulus of zinc saturated in its whole mass, but especially at its upper surface, with fine long needles of silicon. To extract them, it is merely necessary to dissolve the excess of zinc in hydrochloric acid.

In this manner large crystals of silicon are obtained with ease, and in greater quantity than by any other method. It is possible that zinc, from its many useful qualities, may become a most valuable agent in the preparation of elements by metallic solution.

If the zinc-silicon be heated, the zinc is gradually volatilized, but a very high temperature is necessary for this. Pure silicon may be melted and cast in moulds. Copper and silicon unite in various proportions. A very hard, brittle, and white alloy, containing 12 per cent. silicon, is obtained by melting together three parts silico-fluoride of potassium, one part sodium, and one part of copper, at such a temperature that the fused mass remains covered with a very liquid scoria. The copper takes up the whole of the silicon, and remains as a white substance less fusible than silicon, which may serve as basis for other alloys. An alloy with 5 per cent. silicon has a beautiful bronze colour, and will probably receive important applications.

Silicon and iron combine to form an alloy which is a sort of fusible steel, in which carbon is replaced by silicon. The siliciurets are all of them quite homogeneous, and are not capable of being separated by liquation.

A new determination of the atomic weights of cobalt and of nickel has been made by R. Schneider*. The numbers hitherto adopted, 29·55 for cobalt, and 29·49 for nickel, rest upon the determinations of Rothoff; and Berzelius considered that the small difference arose from an error of observation, and that the two metals have the same atomic weight. Rothoff's method consisted in converting a weighed quantity of pure oxide into the chloride by solution in hydrochloric acid, and then evaporating the excess of hydrochloric acid, and determining the chlorine in the pure chloride as chloride of silver.

But the method cannot be considered very reliable; for although chlorine determinations are accurate, the preparation

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* Poggendorff's *Annalen*, July 1857.

of a normal chloride is very difficult; and thirty-nine years ago, when these determinations were made, the methods of preparing pure cobalt and nickel compounds were very imperfect.

Schneider's determination was made with the oxalates of nickel and of cobalt, which have the advantage that they are very insoluble in water and dilute acids, and also that the determination of the carbon and the metal may be made with great accuracy.

Great care was taken to obtain the oxalates quite pure. The oxalate of nickel was prepared by adding to a feebly acid solution of perfectly pure chloride of nickel, a cold saturated solution of oxalic acid, washing the resulting oxalate with water until it was perfectly neutral and free from all trace of acid, and then continuing the washing some time. When dry, it forms a sandy powder, which under the microscope has a crystalline granular appearance.

The oxalate of cobalt was obtained by digesting pure carbonate of cobalt with oxalic acid, and then washing out the oxalate, at first by decantation, and then on a filter. When dry, the salt is a loose woolly powder, which, like sulphur, becomes electrical by rubbing in a mortar.

For the analysis, the salts were heated to 110° C. in a stream of dry air. The neutral oxalates contain for two equivalents of carbon, one equivalent of metal; and hence, from the quantitative relation between the carbon and the metal, the equivalent of the latter may be determined, and with the greater certainty from the equivalent of carbon being accurately known. Schneider's method consisted in the determination of this relation. It was necessary that the oxalate should be perfectly neutral, and that the salt used in each carbon and metal determination should have the same degree of dryness.

The determination of the carbon was made by an elementary analysis, the usual precautions being observed. The metal was determined by heating the oxalate in a bulb tube, in a stream of atmospheric air, care being taken to prevent loss by spirting &c. After the decomposition was over, the residue was heated in a current of oxygen until every trace of carbon was burnt; and when this was effected, the oxide was reduced by hydrogen at as low a temperature as possible, but the heat at length raised so that the metal was superficially caked together. By this means the metal had completely lost the property of absorbing oxygen. The hydrogen was then expelled by atmospheric air and the tube weighed, the usual precautions being taken. The mean of the determination of nickel gave as its equivalent 29·025; and of cobalt, 30·003. The equivalents are hence simple multiples of the equivalent of hydrogen; the small difference may be ascribed to errors of observation. The equivalents differ also by one, and are not equally great, as has hitherto been generally assumed.

Weber describes* the preparation of iodide of aluminium. One part of aluminium in filings is placed in a sealed tube, and ten or eleven parts of dry iodine added. The tube is then sealed at the other end and gently heated. Combination takes place with strong evolution of light and heat: the granules of aluminium burn with a splendid violet light. On cooling, the iodide appears as a solid mass coloured brown by excess of iodine; by using a slight excess of metal, and resubliming the iodide once or twice, it is obtained pure. It forms then brilliant white crystalline laminæ, which melt to a very mobile liquid; on further application of heat this boils readily, and sublimes in delicate snow-white laminæ in the colder part of the tube. Exposed to the air, it fumes strongly, and readily deliquesces. Heated in the air it decomposes with the liberation of iodine. The formula of the iodide is $Al^2 I^3$, quite analogous to the chloride. Iodide of aluminium forms with water a compound which is probably a hydrate. It also forms with iodide of potassium a double compound. This is a waxy, transparent, crystalline mass, which readily melts, but sublimes with difficulty. It has the formula $Al^2 I^3 KI$.

XVI. *On the Formation of Indigo-blue.*—Part II.

By Edward Schunck, *Ph.D., F.R.S.*

[Continued from p. 45.]

Indifulvine.

THIS substance is obtained, on the evaporation of its alcoholic solution, in the form of a deep reddish-yellow, transparent, amorphous resin, which when dry is brittle, and may easily be reduced to powder. It is perfectly insoluble in caustic alkalies, a property by which it may be at once distinguished from indiretine, which it resembles in its outward appearance. Even when grape-sugar or protochloride of tin is added to the alkaline liquids, not a trace of it dissolves even on boiling, and in this respect it differs widely from indirubine. When it is treated with strong caustic soda-lye, only a trace of ammonia is given off, but on heating the dry substance with soda-lime, there is a very perceptible evolution of ammonia. When heated on platinum it melts and then burns with a bright flame, leaving much charcoal which burns away with difficulty. On being heated in a tube, it melts and gives off fumes having a strong smell, resembling that of crude indigo when heated. These fumes condense on the colder parts of the tube to a brown oil, which on cooling

* Poggendorff's *Annalen*, July 1857.

becomes almost solid without exhibiting a trace of anything crystalline. It dissolves in concentrated sulphuric acid in the cold, forming a solution of a greenish-brown colour, which when heated becomes black and disengages sulphurous acid. It is not much affected by nitric acid of ordinary strength even on boiling, but fuming nitric acid dissolves it readily, even in the cold, giving a dark reddish-yellow solution, which on the addition of water deposits orange-coloured flocks. If the solution in fuming nitric acid be boiled, it gives off nitrous acid, and on evaporation it leaves a reddish-yellow resinous mass, the greatest part of which, on being treated with boiling water, remains undissolved in the shape of a yellowish-red resin, resembling indifulvine itself in appearance, but differing from it in being easily soluble in alkaline liquids, and soluble with difficulty in boiling alcohol. The watery liquid filtered from this resin yields on evaporation white needle-shaped crystals, which are not oxalic acid. A boiling solution of bichromate of potash to which sulphuric acid is added decomposes indifulvine very slowly, the solution becoming green from the reduction of the chromic acid. Chlorine converts indifulvine, when suspended in water, into a body which does not much differ from it in appearance, but is soluble in alkalies. Indifulvine is not precipitated from its alcoholic solution by acetate of lead, even on the addition of ammonia, as indeed might be inferred from its method of preparation.

Notwithstanding that I worked with tolerable large quantities of the mixed products of decomposition of indican, I obtained only on two occasions a sufficient quantity of pure indifulvine for analysis. The composition on these two occasions was not the same; so that if the substance was in each case pure, there are, strictly speaking, two bodies having the general properties of indifulvine. Nevertheless the formulæ of the two bodies stand in a definite relation to one another and to that of indican, so that in either case the formation of the substance may easily be explained.

I. 0·3695 grm., dried at 100° C., gave 0·9945 grm. carbonic acid and 0·1795 water.

0·3605 grm. gave 0·4665 grm. chloride of platinum and ammonium.

These numbers lead to the following composition:—

	Eqs.		Calculated.	Found.
Carbon . .	22	132	73·33	73·40
Hydrogen .	10	10	5·55	5·39
Nitrogen . .	1	14	7·77	8·12
Oxygen . .	3	24	13·35	13·09
		180	100·00	100·00

The second analysis afforded the following data:—

II. 0·3125 grm. gave 0·8975 grm. carbonic acid and 0·1635 water.

0·3400 grm. gave 0·4635 grm. chloride of platinum and ammonium.

Hence may be deduced the following composition:—

	Eqs.		Calculated.	Found.
Carbon . .	44	264	78·80	78·32
Hydrogen .	19	19	5·67	5·81
Nitrogen . .	2	28	8·35	8·56
Oxygen . .	3	24	7·18	7·31
		335	100·00	100·00

If the first formula be doubled, it gives $C^{44} H^{20} N^{2} O^{6}$; and if from this be deducted one equivalent of water and two equivalents of oxygen, it gives the second formula, $C^{44} H^{19} N^{2} O^{3}$.

For the sake of distinction I think it may be of advantage to apply to the first of these modifications of indifulvine the term *a*-indifulvine, and to the second that of *b*-indifulvine. The manner in which these bodies are derived from indican can only be understood after all the products of decomposition have been treated of.

Indihumine.

This substance has the appearance of a sepia-brown powder, which is insoluble in water and alcohol, but soluble in alkaline liquids, forming brown solutions, from which it is reprecipitated by acids in brown flocks. When heated on platinum it burns without melting, leaving some charcoal which easily burns away. Boiling nitric acid decomposes it easily, forming a yellow solution, which on evaporation leaves an orange-coloured residue insoluble in water. Indihumine is not always formed in any great quantity in the decomposition of indican by acids. Sometimes, indeed, it cannot be detected among the products of decomposition, and usually it is present only in minute quantities. What are the circumstances which determine its formation in particular cases I am unable to say. On the only occasion on which I obtained a sufficient quantity for analysis, it was procured from an alcoholic extract of woad by evaporating, adding water to the residue and filtering, then adding sulphuric acid to the watery solution containing indican, filtering again, allowing the solution to stand for twenty-four hours, filtering off the indigo-blue and other products which had separated, boiling the liquid, collecting the brown powder which was deposited during the ebullition on a filter, washing it with water, and then treating it with a boiling mixture of alcohol and ammonia until nothing more dissolved.

The insoluble residue, consisting of indihumine, was analysed, when the following numbers were obtained:—

0·3065 grm., dried at 100° C., gave 0·7065 grm. carbonic acid and 0·1300 water.

0·3285 grm. gave 0·3765 grm. chloride of platinum and ammonium.

From these numbers it may be inferred that the composition is as follows:—

	Eqs.		Calculated.	Found.
Carbon . .	20	120	62·82	62·86
Hydrogen .	9	9	4·71	4·71
Nitrogen . .	1	14	7·33	7·19
Oxygen . .	6	48	25·14	25·24
		191	100·00	100·00

Indifuscine.

This body so much resembles the preceding in its outward appearance and most of its properties, that the two might easily be confounded. Indifuscine is always obtained in the shape of a dark brown powder, exhibiting sometimes a reddish tinge. It is insoluble in boiling water and only slightly soluble in boiling alcohol, the solution being light brown, and depositing a great part of the substance on cooling in brown flocks. It is easily soluble in a mixture of alcohol and ammonia. The solution is dark-brown and opake; and when it is mixed with an excess of muriatic or acetic acid, the greatest part of the indifuscine is deposited in the form of a brown powder, while the supernatant liquid retains a brown colour, which is rather darker than that of the solution of the substance itself in boiling alcohol. It is also soluble in watery solutions of caustic and carbonated alkalies, forming brown solutions, from which it is precipitated again by acids in brown flocks. The ammoniacal solution gives brown precipitates with salts of baryta, lime, magnesia, alumina, iron, zinc, copper, lead, mercury and silver, the whole of the indifuscine being precipitated in combination with the respective bases. When indifuscine is heated in a platinum crucible, the whole mass begins to heave and is kept in a state of agitation for a few moments, in consequence probably of an evolution of gas at the points of contact with the metal, whereupon it burns, but without melting, and leaves a considerable quantity of charcoal, which burns away with difficulty without leaving any ash. When heated in a tube, it gives fumes having a smell like that of burning turf, besides a little oily sublimate, unmixed with anything crystalline. Concentrated sulphuric acid dissolves indifuscine, forming a brown solution, which on being heated evolves sulphurous acid. A boiling solution of bichromate of potash, to which sulphuric acid is added, dissolves and decomposes it rapidly with an evolution of gas, the chromic acid being reduced to oxide of chromium. On being treated with boiling dilute nitric acid, indifuscine is decomposed with a disengagement of nitrous acid, giving a yellow liquid, which on evaporation yields crystals of oxalic acid. The mother-liquor of these crystals, on being neutralized with carbonate of potash and evaporated, gives brownish-yellow crystals, which detonate when heated, and give with acetate of lead, nitrate of silver, and sulphate of iron, reactions showing them to consist of picrate of potash. When finely-powdered indifuscine is suspended in water and subjected to the action of chlorine, it is converted into a yellow flocculent substance containing chlorine, which is insoluble in boiling water, but dissolves easily in boiling alcohol, forming a brown solution, which on spontaneous evaporation leaves a light brown amorphous residue.

When the indican submitted to decomposition with acids has not been purified, the quantity of indifuscine formed far exceeds that of the other products of decomposition, which, with the exception of indigo-blue, are always produced in comparatively small quantities. In this case a great part of the indifuscine owes its origin to the action of the acid on a body formed by the influence of water and oxygen on indican. Nevertheless, even when perfectly pure indican is employed, a certain quantity of indifuscine is always produced, especially if the quantity of material used is considerable.

When submitted to analysis, indifuscine prepared on different occasions is found to exhibit considerable variation in its composition. The analyses, the results of which I am about to give, were made with specimens derived from different sources, which, notwithstanding the difference in their composition, showed no difference in their properties.

I. 0·3135 grm. indifuscine, obtained from the deposit formed on mixing a watery solution of indican with sulphuric acid and allowing the mixture to stand in the cold, dried at 100° C. and burnt with oxide of copper and chlorate of potash, gave 0·6830 grm. carbonic acid and 0·1305 water.

II. 0·3930 grm., obtained by adding sulphuric acid to a watery solution of indican, allowing the mixture to stand for some time in the cold, filtering, and then employing the deposit produced on heating the filtered liquid, gave 0·8720 grm. carbonic acid and 0·1625 water.

0·5675 grm. gave 0·5230 grm. chloride of platinum and ammonium.

III. 0·3435 grm., derived from the deposit formed on mixing a decoction of woad leaves with muriatic acid and boiling, gave 0·7635 grm. carbonic acid and 0·1510 water.

0·4550 grm. gave 0·4365 grm. chloride of platinum and ammonium.

IV. 0·3675 grm., obtained by extracting finely-chopped woad leaves with warm water, adding sugar of lead to the extract, filtering from the green precipitate, removing the excess of lead with sulphuric acid, filtering, adding more acid, and treating the flocculent deposit which was formed in the usual manner, gave 0·8950 grm. carbonic acid and 0·1635 water.

0·5715 grm. gave 0·5855 grm. chloride of platinum and ammonium.

V. 0·3640 grm., prepared in a similar manner to the last, gave 0·9020 grm. carbonic acid and 0·1640 water.

0·4470 grm. gave 0·5070 grm. chloride of platinum and ammonium.

These numbers correspond in 100 parts to—

	I.	II.	III.	IV.	V.
Carbon . .	59·41	60·51	60·61	66·41	67·58
Hydrogen .	4·62	4·59	4·88	4·94	5·00
Nitrogen . .	...	5·78	6·02	6·43	7·12
Oxygen . .	...	29·12	28·49	22·22	20·03
		100·00	100·00	100·00	100·00

In passing the eye along these numbers, it will be seen that the amount of oxygen decreases progressively from II. to V., while that of the other constituents increases. These two analyses represent, in my opinion, the composition of the two extreme members of a series, the intermediate members of which consist of mixtures or compounds of the two. A number of other analyses which I made, gave results which can only be explained on the supposition that there are two bodies having the same general properties which I have ascribed to indifusceine, but a different composition. Of the above analyses, the first two agree tolerably well with the formula $C^{24} H^{10} NO^{9}$, whereas the last leads to the formula $C^{22} H^{10} NO^{5}$, as the following calculation will show:—

	Eqs.		Calculated.	Eqs.		Calculated.
Carbon . .	24	144	60·00	22	132	67·34
Hydrogen .	10	10	4·16	10	10	5·10
Nitrogen .	1	14	5·83	1	14	7·14
Oxygen . .	9	72	30·01	5	40	20·42
		240	100·00		196	100·00

It will be seen that the two formulæ differ from one another by two equivalents of carbonic acid, and hence the name of *indifuscone* might not be inappropriate for the body whose composition is expressed by the second formula, $C^{22}H^{10}NO^5$. Though this is the only way in which I am able to explain these discrepancies, still I failed in all my efforts to separate any specimen of the substance having an intermediate composition into two distinct constituents, as every such specimen behaved towards all reagents as if it were one single substance. By treating, however, a specimen of this kind with a boiling solution of caustic soda for a length of time, the per-centage of carbon was increased by about 2·5, showing that the body whose formula is $C^{24}H^{10}NO^9$, has a tendency to lose carbonic acid and be converted into the one whose composition is expressed by the formula $C^{22}H^{10}NO^5$. The substance used in this experiment was that employed for the analysis No. III. It was dissolved in caustic soda; the solution was boiled for some time, and then mixed with an excess of muriatic acid. The precipitate produced by the acid was dissolved in alcohol and ammonia, and the solution having been mixed with an excess of acid, deposited a brown powder, which after being collected on a filter, washed and dried, was analysed, when it was found to contain 63·22 per cent. of carbon.

Indiretine.

This body, the most striking properties of which have been already mentioned in the first part of this paper, appears on evaporation of its alcoholic solution in the form of a dark brown, shining resin, which is transparent only in very thin layers. It resembles indifulvine in appearance, but is distinguished from the latter by its being easily soluble in all alkaline liquids. When heated on platinum-foil it melts, swells up very much, and burns with a yellow smoky flame, leaving some charcoal which slowly burns away. When heated in a tube, it swells up and gives strong-smelling fumes, together with an oily sublimate resembling that obtained from indifulvine, which when cool becomes half solid. Concentrated sulphuric acid dissolves it in the cold, forming a brown solution, which when boiled becomes black, and disengages sulphurous acid. Boiling nitric acid decomposes it with an evolution of nitrous acid, giving a yellow solution, which on evaporation leaves a brown residue consisting of a resinous substance insoluble in water and a little picric acid. When it is treated with boiling caustic soda-lye only a trace of ammonia is given off, but when heated with soda-lime there is a much stronger evolution of ammonia. A boiling solution of bichromate of potash, to which sulphuric acid has been added, slowly decomposes it with an evolution of gas, while the liquid becomes green. The solution in ammonia is brown, and gives brown precipitates with the chlorides of barium and calcium and with nitrate of silver, while the liquid in each case becomes colourless. The alcoholic solution gives with acetate of lead a brown precipitate, which dissolves entirely on the addition of acetic acid; and with acetate of copper it gives a slight brown precipitate, the filtered liquid being still dark brown.

In the first part of this paper I have given for indiretine the formula $C^{36} H^{20} NO^{13}$. The analyses which I have made with fresh preparations of this body lead to the formula $C^{36} H^{17} NO^{10}$, as will be seen from the following details.

I. 0·3955 grm., dried at 100° C. and burnt with oxide of copper and chlorate of potash, gave 0·9565 grm. carbonic acid and 0·1995 water.

0·5215 grm., burnt with soda-lime, gave 0·1400 grm. platinum.

II. 0·4250 grm. of the same preparation, heated to 190° C. and then kept for several hours at 100° C., gave 1·0300 grm. carbonic acid and 0·2090 water.

0·5065 grm. gave 0·1370 grm. platinum.

III. 0·4210 grm. of a different preparation gave 1·0200 grm. carbonic acid and 0·2140 water.

The theoretical composition, as compared with that derived from these numbers, is as follows:—

	Eqs.		Calculated.	I.	II.	III.
Carbon . .	36	216	66·05	65·96	66·09	66·07
Hydrogen .	17	17	5·19	5·60	5·46	5·64
Nitrogen .	1	14	4·28	3·81	3·84	
Oxygen . .	10	80	24·48	24·63	24·61	
		327	100·00	100·00	100·00	

I think it is improbable that the discrepancy between the two formulæ, which differ from one another merely by 3HO, proceeds from any impurity in either case, or that it is to be attributed to the substance having been more carefully dried at one time than at another. Indiretine appears to furnish one of those instances, of which I have met with several during the investigation of this series, of a body exhibiting when prepared on different occasions, the same properties, but having at one time a composition differing by the elements of water from that which it has at others.

Having described the several products of decomposition formed by the action of acids on indican, which are insoluble in water, I shall now proceed to the consideration of those which are soluble in water. In order to obtain these, I found it advisable to employ sulphuric acid for the decomposition of the indican. After the process was completed, the insoluble matter deposited was separated by filtration; the sulphuric acid was removed by means of carbonate of lead, and the liquid having been filtered, sulphuretted hydrogen was passed through it in order to precipitate a little lead contained in it, and after being again filtered, it was evaporated by means of a current of air in the same apparatus as that employed in the evaporation of solutions of indican. After the evaporation was completed, there was left a light brown syrup, which on being treated with alcohol was usually entirely dissolved. The alcoholic solution was filtered if necessary, and then mixed with about twice its volume of æther, which immediately turned it milky and produced a deposit consisting of a brown syrup. This syrup was allowed to settle, and the whole was left to stand for twenty-four hours. The surface of the syrup and the sides of the glass vessel were then found to be covered with a quantity of small, almost white crystals. These crystals are the same as those referred to above as being obtained in the preparation of indican, when æther is added to the alcoholic solution of the latter. I was at first inclined to suppose that they consisted of a substance which was contained as such in the plant, but I soon discovered that they were a product of decomposition of indican, as they were also obtained from perfectly pure indican, which had been prepared by successive solution in alcohol, water and æther, in the last of which the crystals are insoluble. Indeed no product of decomposition of indican seems to be so easily formed as this. By shaking the liquid from which they were deposited, the crystals were easily detached from the sides of the vessel and the surface of the syrup. They were collected on a filter, washed with æther, and then pressed between folds of blotting-paper, in order to absorb any of the syrup which might be mixed with them. They were then dissolved in boiling water, and the solution having been decolorized with animal charcoal, was filtered and evaporated, when it left a crystalline mass, which was again pressed between blotting-paper, and dissolved in a small quantity of boiling alcohol. The alcoholic solution on cooling deposited a mass of small crystals, which had the properties and composition of

Leucine.

It crystallized from the alcoholic solution in small flat tables having a pearly lustre, which repelled cold water like a fatty acid, but were readily soluble in boiling water. It was insoluble in æther. When heated in a tube, it was completely volatilized without melting, forming a sublimate on the colder parts of the tube in the form of a light mass like cotton. It was easily soluble, even in the cold, in sulphuric, muriatic and nitric acids. The solution in nitric acid gave off no nitrous fumes on being boiled, and left on evaporation a colourless syrup, which on standing was changed into a crystalline mass. The solution in muriatic acid left on evaporation a crystalline residue. It was easily soluble in caustic soda, and the solution evolved no ammonia on being boiled, but when the dry substance was heated with soda-lime, it gave off a strong smell of ammonia accompanied by a peculiar penetrating odour. The watery solution was neutral to test-paper, and had no perceptible taste. When mixed with freshly precipitated oxide of copper and boiled, the watery solution became sky-blue; the filtered liquid gave no precipitate with caustic soda, and on being evaporated left a residue consisting of bright blue crystals. The watery solution gave no precipitate with nitrate of silver, but on the addition of a little ammonia, there was deposited almost immediately a quantity of small crystalline scales, which blackened slightly on exposure to the light, and were not easily soluble in an excess of ammonia. The watery solution gave no precipitate with acetate of lead, and even on adding ammonia there was only a slight precipitate; but on allowing the ammoniacal liquid to stand for some hours, there was formed a crystalline mass of a pearly lustre, consisting of needles arranged in star-shaped masses.

The analysis of the substance gave the following results:—

0·3430 grm., dried at 100°C. and burnt with chromate of lead, gave 0·6820 grm. carbonic acid and 0·3125 water.

0·2550 grm. gave 0·4125 grm. chloride of platinum and ammonium.

The composition in 100 parts agrees tolerably well with that required by the formula of leucine, $C^{12}H^{13}NO^{4}$, as will be seen from the following comparison of the calculated composition with that found by experiment:—

	Eqs.		Calculated.	Found.
Carbon . . .	12	72	54·96	54·22
Hydrogen. . .	13	13	9·92	10·12
Nitrogen . . .	1	14	10·68	10·16
Oxygen . . .	4	32	24·44	25·50
		131	100·00	100·00

However strange the fact of leucine, a substance hitherto supposed to be a product of decomposition peculiar to animal matters, being obtained from the decomposition of a vegetable substance may have appeared at a former period, it will no longer excite surprise at the present time, when so many different bodies have been found to be common to both classes of organisms. It is a fact, however, which seems to imply some connexion, hitherto unsuspected, between leucine and indigo-blue.

The brown syrup precipitated together with leucine by the addition of æther to the alcoholic solution, consisted chiefly of the peculiar kind of sugar produced by the decomposition of indican, and to which, as having a composition differing from that of most other species of sugar, I propose to give the name of

Indiglucine.

In order to purify it, the brown syrup, after the crystals of leucine had been separated by decantation, was dissolved in water, and acetate of lead was added to the solution. A slight precipitate was thereby produced, which was separated by filtration; and on adding ammonia to the liquid, a bulky yellowish precipitate fell, consisting chiefly of the lead compound of indiglucine. This was filtered off, completely washed with water, and decomposed with sulphuretted hydrogen. The filtered liquid was agitated with animal charcoal until it had quite lost the yellowish tinge which it showed at first, and until a portion of it, on being mixed with acetate of lead and ammonia, gave a perfectly white precipitate. It was then filtered again, and evaporated either in the apparatus above described by means of a current of air, or over sulphuric acid. The syrup left after evaporation was dissolved in alcohol, and the solution was mixed with twice its volume of æther, when the indiglucine was precipitated as a pale yellow syrup, having a sweetish taste.

To the description formerly given of this substance I have only a few particulars to add. Baryta-water gives no precipitate in the watery solution, but on adding alcohol a slight flocculent yellow precipitate is produced. The watery solution, after being mixed with milk of lime and filtered, is found to have become strongly alkaline, and on being boiled becomes quite thick in consequence of the separation of a bulky yellow mass of flocks, which on the liquid cooling is completely redissolved, forming a clear yellow solution as before, an experiment which may be repeated any number of times. The solution of the lime compound, when mixed with an excess of alcohol, gives a bulky yellowish precipitate, after which the liquid appears almost colourless. When treated with boiling nitric acid, indiglucine is decomposed and yields oxalic acid. When a watery solution of indiglucine is mixed with yeast and left to stand in a warm place, no disengagement of gas is observed, nor is any sign of fermentation taking place manifested. After some days, however, the solution begins to acquire a strongly acid taste and reaction, showing that it has entered into a state of acetous fermentation without having passed through the intermediate stage of the alcoholic fermentation.

The new analyses which I have made of the lead compound, confirm the conclusion at which I arrived at an early period of the investigation, viz. that when in combination with oxide of lead the composition of indiglucine is expressed by the formula $C^{12}H^{9}O^{11}$, and that hence its formula, when in an uncombined state, is probably $C^{12}H^{10}O^{12}$.

An analysis of the lead compound, prepared by adding acetate of lead and ammonia to a watery solution of indiglucine, filtering, washing and drying *in vacuo*, gave the following results:—

0·5640 grm., burnt with chromate of lead, gave 0·2430 grm. carbonic acid and 0·0845 water.

0·2495 grm. gave 0·2445 grm. sulphate of lead.

These numbers lead, like those of the former analyses, to the formula $C^{12}H^{9}O^{11}+4PbO$, as will be seen by comparing the numbers required by theory with those deduced from the analysis.

	Eqs.		Calculated.	Found.
Carbon . . .	12	72	11·69	11·75
Hydrogen . .	9	9	1·46	1·66
Oxygen . . .	11	88	14·30	14·49
Oxide of lead. .	4	446·8	72·55	72·10
		615·8	100·00	100·00

There still remain some products of the action of acids on indican to be treated of. These products are volatile. In order to ascertain their nature, I took a solution of indican, mixed it with sulphuric acid and boiled it in a retort, the tube of which passed through a cork into a receiver from which a tube led into a bottle with lime-water, the joinings being all air-tight. After the liquid had entered into a state of ebullition and the air had been expelled from the apparatus, bubbles of gas were seen now and then to pass through the lime-water, which became milky and deposited a quantity of carbonate of lime. After a great part of the solution had been distilled, the receiver was removed, and the liquid contained in it, which was yellowish and had an acid reaction, was mixed with an excess of carbonate of soda and evaporated to dryness. The saline residue was mixed with an excess of dilute sulphuric acid and the liquid was distilled. The distillate was now colourless. It contained formic acid, for after being neutralized and mixed with nitrate of silver, metallic silver was soon deposited even in the cold. The whole of it was boiled with carbonate of lead, and the filtered liquid was evaporated, when it yielded some shining crystalline needles surrounded by a thick syrup. By means of a little cold water, the syrup was removed, the needles being left undissolved. The latter had the properties of formiate of lead.

0·3450 grm. of these needles, dried at 100° C. and heated with sulphuric acid, gave 0·3505 grm. sulphate of lead, equivalent to 0·2579 oxide of lead or 74·75 per cent. In 100 parts of formiate of lead there are contained by calculation 75·11 parts of oxide of lead.

The liquid poured off from these crystals was mixed with an excess of sulphuric acid, filtered from the sulphate of lead and distilled. The distilled liquid was boiled with peroxide of mercury, in order to decompose any formic acid which it might contain, and filtered, and after sulphuretted hydrogen had been passed through it, it was again filtered from the precipitated sulphuret of mercury. The excess of sulphuretted hydrogen was removed by agitation with carbonate of lead, and the filtered liquid was mixed with an excess of sulphuric acid, filtered again from the sulphate of lead and distilled. The distillation was repeated, and the distillate was then boiled with carbonate of silver, filtered and evaporated *in vacuo*. A residue was left consisting of small white crystalline grains, which repelled water just as if they contained fatty matter. When a portion of this residue was mixed with alcohol and sulphuric acid and the mixture was boiled, a smell like that of butyric æther was given off. The quantity obtained was just sufficient for one analysis, the results of which were as follows:—

0·4420 grm. gave 0·2370 grm. carbonic acid and 0·0925 water.

0·0990 grm. gave 0·0835 grm. chloride of silver.

These numbers correspond in 100 parts to—

Carbon	14·62
Hydrogen	2·32
Oxygen	14·87
Oxide of silver	68·19
	100·00

This composition approximates, as will be seen, to that of acetate of silver, which consists in 100 parts of—

Carbon	14·37
Hydrogen	1·79
Oxygen	14·38
Oxide of silver	69·46
	100·00

The excess in the amount of carbon and hydrogen, and the deficiency in that of the oxide of silver, show, however, that it must have contained a small quantity of the silver salt of another acid belonging to the same series as formic and acetic acids, a series having the general formula $C^n H^n O^4$. This acid was probably propionic acid, an acid the formation of which must indeed be assumed in order to explain how one of the other products of decomposition of indican takes its rise. The quantity of this acid, however, contained in the silver salt, the analysis of which has just been given, was very small, since, as may be inferred from the composition of the salt, it was to that of the acetic acid in the proportion of 1 equivalent of the former to 23 equivalents of the latter.

Having described all the products to which the decomposition of indican with acids gives rise, it will now be possible to give an account of the manner in which these various products are formed, and of the relation in which they stand to one another.

The decomposition of indican, after taking up several equivalents of water, into 1 equivalent of indigo-blue or indirubine and 3 equivalents of indiglucine, will be evident at once from a comparison of the formulæ of these bodies.

The formation of leucine will also be easily understood when it is considered that indigo-blue and 10 equivalents of water contain the elements of 1 equivalent of leucine, 1 equivalent of formic acid, and 2 equivalents of carbonic acid, as the following equation will show:—

$$\left.\begin{array}{ll}\text{1 eq. indigo-blue} & C^{16}H^{5}NO^{2} \\ \text{10 eqs. water} & \phantom{C^{16}}H^{10}O^{10} \\ \cline{2-2} & C^{16}H^{15}NO^{12}\end{array}\right\} = \left\{\begin{array}{ll} C^{12}H^{13}NO^{4} & \text{1 eq. leucine.} \\ C^{2}H^{2}O^{4} & \text{1 eq. formic acid.} \\ C^{2}\phantom{H^{2}N}O^{4} & \text{2 eqs. carb. acid.} \\ \hline C^{16}H^{15}NO^{12} & \end{array}\right.$$

Such a decomposition as this can of course only take place before the elements of indican have arranged themselves in such a manner as to form indigo-blue, which is a body of far too stable a nature to undergo any decomposition by the action of dilute acids.

In order to explain the formation of indifulvine, it is necessary to take into consideration the simultaneous formation of formic acid. Indican may be supposed, after taking up 5 equivalents of water, to split up into 1 equivalent of *a*-indifulvine, 2 equivalents of indiglucine, and 3 equivalents of formic acid, as will be seen from the following equation:—

$$\left.\begin{array}{ll}\text{1 eq. indican} & C^{52}H^{31}NO^{34} \\ \text{5 eqs. water} & \phantom{C^{52}}H^{5}O^{5} \\ \cline{2-2} & C^{52}H^{36}NO^{39}\end{array}\right\} = \left\{\begin{array}{ll} C^{22}H^{10}NO^{3} & \text{1 eq. } a\text{-indifulvine.} \\ C^{24}H^{20}O^{24} & \text{2 eqs. indiglucine.} \\ C^{6}H^{6}O^{12} & \text{3 eqs. formic acid.} \\ \hline C^{52}H^{36}NO^{39} & \end{array}\right.$$

The following equation shows how the other modification of indifulvine may be supposed to take its rise:—

$$\left.\begin{array}{ll}\text{2 eqs. indican} & C^{104}H^{62}N^{2}O^{68} \\ \text{7 eqs. water} & \phantom{C^{104}}H^{7}\phantom{N^{2}}O^{7} \\ \cline{2-2} & C^{104}H^{69}N^{2}O^{75}\end{array}\right\} = \left\{\begin{array}{ll} C^{44}H^{19}N^{2}O^{3} & \text{1 eq. } b\text{-indifulvine.} \\ C^{48}H^{40}\phantom{N^{2}}O^{48} & \text{4 eqs. indiglucine.} \\ C^{10}H^{10}\phantom{N^{2}}O^{20} & \text{5 eqs. formic acid.} \\ C^{2}\phantom{H^{40}N^{2}}O^{4} & \text{2 eqs. carb. acid.} \\ \hline C^{104}H^{69}N^{2}O^{75} & \end{array}\right.$$

It may further be assumed that 1 equivalent of indican, after taking up 4 equivalents of water, is decomposed into 1 equivalent of indihumine, 2 equivalents of indiglucine, 1 equivalent of propionic acid, and 2 equivalents of carbonic acid, as follows:—

$$\left.\begin{array}{ll} \text{1 eq. indican} & C^{52}H^{31}NO^{34} \\ \text{4 eqs. water} & \underline{\qquad H^{4}\quad O^{4}} \\ & C^{52}H^{35}NO^{38} \end{array}\right\} \left\{\begin{array}{ll} C^{20}H^{9}NO^{6} & \text{1 eq. indihumine.} \\ C^{24}H^{20}O^{24} & \text{2 eqs. indiglucine.} \\ C^{6}H^{6}O^{4} & \text{1 eq. propionic acid.} \\ \underline{C^{2}\qquad O^{4}} & \text{2 eqs. carbonic acid.} \\ C^{52}H^{35}NO^{38} & \end{array}\right.$$

The formation of indifuscone is quite analogous to that of indihumine, the propionic acid in the preceding equation being simply replaced by acetic acid, for—

$$\left.\begin{array}{ll} \text{1 eq. indican} & C^{52}H^{31}NO^{34} \\ \text{3 eqs. water} & \underline{\qquad H^{3}\quad O^{3}} \\ & C^{52}H^{34}NO^{37} \end{array}\right\} \left\{\begin{array}{ll} C^{22}H^{10}NO^{5} & \text{1 eq. indifuscone.} \\ C^{24}H^{20}O^{24} & \text{2 eqs. indiglucine.} \\ C^{4}H^{4}O^{4} & \text{1 eq. acetic acid.} \\ \underline{C^{2}\qquad O^{4}} & \text{2 eqs. carbonic acid.} \\ C^{52}H^{34}NO^{37} & \end{array}\right.$$

The manner in which indifuscine takes its rise from indican needs no explanation, since a comparison of its formula $C^{24}H^{10}NO^{9}$ with that of indifuscone shows that its composition differs from that of the latter by containing in addition the elements of 2 equivalents of carbonic acid. In its conversion into indiretine, indican splits up into 1 equivalent of the latter body, 1 equivalent of indiglucine, and 4 equivalents of carbonic acid. Here, however, the anomaly presents itself of a copulated body like indican losing water instead of taking it up during its decomposition into simpler compounds, as will be seen from the following equation:—

$$\text{1 eq. indican } C^{52}H^{31}NO^{34} = \left\{\begin{array}{ll} C^{36}H^{17}NO^{10} & \text{1 eq. indiretine.} \\ C^{12}H^{10}O^{12} & \text{1 eq. indiglucine.} \\ C^{4}O^{8} & \text{4 eqs. carbonic acid.} \\ \underline{\qquad H^{4}\quad O^{4}} & \text{4 eqs. water.} \\ C^{52}H^{31}NO^{34} & \end{array}\right.$$

It will be observed that when indican is converted into indigo-blue or indirubine, it yields at the same time 3 equivalents of indiglucine, whereas the formation of the other products of decomposition is accompanied by the elimination of no more than 1 or 2 equivalents of that substance. Hence it may be inferred that the appearance of these other products is due to a part of the indiglucine undergoing a further decomposition from the action of the acid, its elements, together with the residual portion of the indican, affording the materials out of which the other products are formed. In fact, we may easily suppose indiglucine, or perhaps more strictly speaking the group of atoms contained in indican which goes to form indiglucine, to split up into 1 equivalent of propionic acid, 1 equivalent of acetic acid, and 2 equivalents of carbonic acid, for

$$C^{12} H^{10} O^{12} = C^6 H^6 O^4 + C^4 H^4 O^4 + 2CO^2.$$

Each of these subordinate groups of atoms, or any two of them, may then be supposed to enter into combination with that portion of the indican which goes to form indigo-blue or indirubine, and which may be called its central nucleus. When, for instance, indihumine is formed, 2 equivalents of indiglucine are produced from the indican, whereas the third equivalent splits up into acetic acid, propionic acid, and carbonic acid. The two latter are set at liberty, and may be found among the volatile products of decomposition, but the elements of the acetic acid unite with the indigo-blue group of atoms, yielding by the combination indihumine. In the case of indifuscone, the acetic and carbonic acids derived from the third equivalent of indiglucine are set at liberty, whereas the propionic acid combines with the indigo-blue molecule constituting indifuscone. Indifuscine again may be supposed to consist of indigo-blue, propionic acid and carbonic acid, acetic acid alone being in this case disengaged. In the process of decomposition which leads to the formation of indiretine, only 1 equivalent of indiglucine is eliminated, and 4 equivalents of carbonic acid are disengaged, while the 2 equivalents of propionic acid, as well as the 2 equivalents of acetic acid derived from the other two equivalents of indiglucine, unite with the indigo-blue nucleus to produce indiretine. When *a*-indifulvine is formed, it must be assumed that 1 equivalent of indiglucine, after taking up 2 equivalents of water, splits up into 3 equivalents of formic acid, and a body represented by the formula $C^6 H^6 O^2$, as follows:—

$$C^{12} H^{10} O^{12} + 2HO = 3C^2 H^2 O^4 + C^6 H^6 O^2.$$

Now the last is the formula belonging to the aldehyde of propionic acid, and by adding to this formula that of indigo-blue, the sum will represent the composition of *a*-indifulvine. The more complicated formula given above for *b*-indifulvine, viz. $C^{44} H^{19} N^2 O^3$, represents a compound of 2 equivalents of indigo-blue, with a body whose formula is $C^{12} H^{12} O^2$, and which is therefore homologous with propylic aldehyde, its origin being due to 2 equivalents of indiglucine, being decomposed in such a manner as to give rise to formic acid and carbonic acid in accordance with the following equation:—

$$2C^{12} H^{10} O^{12} + 2HO = 5C^2 H^2 O^4 + 2CO^2 + C^{12} H^{12} O^2.$$

It will be observed in all these cases, with one exception, viz. that of indihumine, that in the assumed combination of the elements of indigo-blue with those of these various acids, &c., one or more equivalents of water must be supposed to be eliminated, as will be seen by a glance at the following equations:—

$$\begin{array}{lclcl}\text{Indihumine.} & & \text{Indigo-blue.} & & \text{Acetic acid.}\\ C^{20}H^{9}NO^{6} & = & C^{16}H^{5}NO^{2} & + & C^{4}H^{4}O^{4}\end{array}$$

$$\begin{array}{lclcl}\text{Indifuscone.} & & \text{Indigo-blue.} & & \text{Propionic acid.}\\ H^{22}H^{10}NO^{5} & = & C^{16}H^{5}NO^{2} & + & C^{6}H^{6}O^{4}-HO\end{array}$$

$$\begin{array}{lclcl}\text{Indifuscine.} & & \text{Indigo-blue.} & & \text{Propionic acid.}\\ C^{24}H^{10}NO^{9} & = & C^{16}H^{5}NO^{2} & + & C^{6}H^{6}O^{4}+2CO^{2}-HO\end{array}$$

$$\begin{array}{lclcl}a\text{-Indifulvine.} & & \text{Indigo-blue.} & & \text{Propylic aldehyde.}\\ C^{22}H^{10}NO^{3} & = & C^{16}H^{5}NO^{2} & + & C^{6}H^{6}O^{2}-HO\end{array}$$

$$\begin{array}{lclcl}b\text{-Indifulvine.} & & \text{Indigo-blue.} & & \\ C^{44}H^{19}N^{2}O^{3} & = & 2C^{16}H^{5}NO^{2} & + & C^{12}H^{12}O^{2}-3HO\end{array}$$

$$\begin{array}{lclcl}\text{Indiretine.} & & \text{Indigo-blue.} & & \text{Acetic acid. Propionic acid.}\\ C^{36}H^{17}NO^{10} & = & C^{16}H^{5}NO^{2} & + & 2C^{4}H^{4}O^{4}+2C^{6}H^{6}O^{4}-8HO\end{array}$$

It must not for a moment be supposed that these bodies really are compounds of indigo-blue, or that the latter is in any shape contained in them or may be obtained by their decomposition. Indeed all my experiments lead to the conclusion that the elements are arranged in a manner very different from what might be inferred from the above equations. If the nitrogenous substances formed from indican, together with indigo-blue, were copulated bodies containing the latter, it would be possible to obtain from them either indigo-blue itself, or its products of decomposition. With the small quantities of these substances which were at my disposal, I was unable to make many experiments to decide this point. A tolerably large quantity of indifuscine was, however, subjected to the action of a strong caustic soda-lye, the liquid being boiled until it left a thick mass, which was heated still further until a small portion of it no longer dissolved in water with a dark brown colour. I was unable, however, to discover among the products of decomposition a trace of anthranilic acid, which would probably have been present if indifuscine contained the elements of indigo-blue.

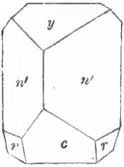
Some advantage may nevertheless arise from looking at these compounds from the point of view just presented, as their relation to one another, to indigo-blue and to indican is thereby more vividly impressed on the memory. This method of considering them may also serve to show that these compounds are all produced at the expense of indigo-blue, that the elements contained in indican which have formed a certain portion of indifulvine, indihumine, &c., might, under certain unknown circumstances, have produced equivalent quantities of indigo-blue, and that the latter cannot therefore be said in any sense to preexist in indican.

[To be continued.]

XVII. *On some new Forms of British Sphenes.*
By Dr. Heddle*.

THE following forms of sphene were found by Mr. Dudgeon and myself in a glen about a mile west of New Abbey in Kirkcudbrightshire:—

ync; yncr; yncx; yncxr; yncxw; yncxwz.



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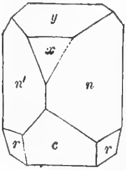

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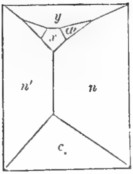

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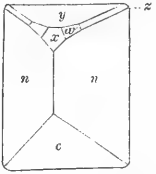

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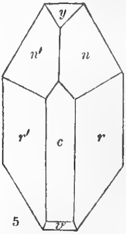

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	°	′		°	′		°	′
y c	120	30	*c r*	146	13	*w w*	164	32
y n	141	36	*c v*	126	26	*x z*	164	49
y r	114	21	*r r*	113	28	*z z*	149	38
y v	66	58	*n n*	136	04	*z w*	172	32
y x	158	55	*c n*	144	53	*z n*	167	23
y z	154	13	*n r*	153	24	*z r*	140	47
c x	140	39	*x w*	172	16			

These crystals, which are imbedded in syenite, are associated with minute crystals of Allanite of the form M T *n*, and small specks of Gadolinite (?).

The largest crystal of sphene found was but the third of an inch in length; at some places, however, they occur in such quantity as to become quite a constituent of the rock, and to communicate to it their characteristic hair-brown colour.

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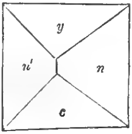
* Communicated by the Author.

The frequent occurrence of crystals in which the planes *n* are short, as in the adjoining figure, may explain statements as to zircon being found at this locality; such crystals having much resemblance to the terminal planes of a prism of zircon: no crystal of that mineral has at least been found either by Mr. Dudgeon, or by the late Mr. Copland of Blackwood, who had previously carefully surveyed all the Criffel district.



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The form *y c r n v* (fig. 5) was found with rutile and chlorite in quartz at Craig Cailleach in Perthshire; the crystals were of a straw-yellow colour.

I have followed Dana in giving to the figures of these crystals a position in which *y* is the base of the fundamental prism, and *r* is the prismatic plane; a consideration of the Perthshire and St. Gothard crystals seems to indicate this to be the natural position: the cleavage of the New Abbey crystals appeared to be parallel to *r* and *l*, but very indistinct.

XVIII. *On the Adjustments of the Needle of a Tangent-galvanometer. By* G. Johnstone Stoney, *A.M., M.R.I.A., Secretary to the Queen's University in Ireland*[*].

M. BRAVAIS has already published[†] the corrections arising from the length of the needle of a tangent-galvanometer, so constructed that the magnetic centre of the needle is at any given point of the axis of a circular current placed in the plane of the magnetic meridian. Herr Bosscha, adopting the ordinary form of galvanometer, in which the centre of the needle coincides with the centre of the current, has added[‡] the correction to be applied for the breadth of the metal band, which is often used in these instruments instead of a wire as the guide to the current. Since, however, the magnetic centre of a needle seldom coincides with its point of suspension, and as the usual mode of supporting it by fibres of silk forces us to depend for the accuracy of the position of its point of suspension on adjustments to be made each time the instrument is used, it was found necessary to inquire also into the effects of the kinds of derangement to be apprehended from either of these causes; and as others may have felt the same want as the author, it may perhaps prove useful to publish the following results.

Conceive the circular current to be placed in the plane of the

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[*] Communicated by the Author.

[†] *Comptes Rendus*, vol. xxxvi. p. 193.

[‡] Poggendorff's *Annalen*, vol. ci. p. 527.

magnetic meridian, and draw through its centre three rectangular coordinates, X perpendicular to its plane through the magnetic east, Y and Z in its plane, Z towards the zenith and Y towards the magnetic north point of the horizon. Imagine now a moveable axis Y′ to be drawn through the centre of the circle and the projection of the north pole of the needle on the plane of the circle, then taking the radius of the circular current as the unit of length, and dividing its circumference into elements $d\phi$, commencing from its intersection with Y′, we find by Ampère's theory for the components of the current's action on the north pole of the needle,

$$X=2\mu i\int_0^\pi d\phi\,.\,\frac{1-y'\cos\phi}{(1+x^2+y'^2-2y'\cos\phi)^{\frac{3}{2}}},$$

$$Y'=2\mu i\int_0^\pi d\phi\,.\,\frac{x\cos\phi}{(1+x^2+y'^2-2y'\cos\phi)^{\frac{3}{2}}},$$

in which i is the intensity of the current, μ a constant, and x, y, z, y' coordinates of the north pole of the needle. We have also for the force northwards,

$$Y=\frac{y}{y'}\,.\,Y';$$

and for the moment to turn the needle round its magnetic centre,

$$\lambda\cos\theta\,.\,X-\lambda\sin\theta\,.\,Y;$$

2λ being the interval between the poles of the needle, and θ its deviation from the magnetic north. Hence this moment, which arises from the action of the current on the north pole,

$$=2\mu i\lambda\cos\theta\,.\int_0^\pi d\phi\,.\,\frac{1-\dfrac{xy\tan\theta+y'^2}{y'}\,.\cos\phi}{(1+x^2+y'^2-2y'\cos\phi)^{\frac{3}{2}}}.\quad\ldots\quad(1)$$

Assuming now that the magnetic centre of the needle is meant to be at the distance a from the origin along the axis of the current, but that through want of adjustment it deviates from this position so that its coordinates are $a+\alpha$, β, γ, we find that

$$\left.\begin{aligned} x&=a+\alpha+\lambda\sin\theta\\ y&=\beta+\lambda\cos\theta\\ y'^2&=y^2+\gamma^2.\\ \text{Hence}\qquad 1+x^2+y'^2&=A^2+2B,\\ \text{writing}\qquad A^2&\text{ for }1+a^2+2a\alpha+\lambda^2+\alpha^2+\beta^2+\gamma^2,\\ \text{and}\qquad B&\text{ for }\lambda a\sin\theta+\lambda(\alpha\sin\theta+\beta\cos\theta).\end{aligned}\right\}\quad .\quad(2)$$

Substituting this value for $1+x^2+y'^2$ in equation (1), and expanding the denominator, we find that the moment from the current's action on the north pole

$$=2\mu i\frac{\lambda\cos\theta}{A^3}\int_0^\pi d\phi\,.\left(1-\frac{xy\,.\tan\theta+y'^2}{y'}\,.\cos\phi\right)$$
$$\times\left\{1-\frac{3}{1}\cdot\frac{1}{A^2}\cdot(B-y'\cos\phi)+\frac{3\,.\,5}{1\,.\,2}\cdot\frac{1}{A^4}\cdot(B-y'\cos\phi)^2-\&c.\right\}.$$

In multiplying this out, all odd powers of $\cos\phi$ may be rejected, since $\int_0^\pi .\,d\phi\,.\cos^{2n-1}\phi=0$. After integrating the other terms and reducing, the expression for the moment becomes

$$2\mu i\lambda\cos\theta\pi\,.\left[\frac{1}{A^3}-\frac{3B}{A^5}+\frac{15}{4}\cdot\frac{1}{A^7}\cdot\left\{2B^2+y'^2-(xy\tan\theta+y'^2)\right.\right.$$
$$\left.\left.\times\left(\frac{2}{5}A^2-2B\right)\right\}+\&c.\right],$$

into which λ enters implicitly through the values (2) of A, B, x, y and y'. Now by simply changing the sign of λ throughout and then the sign of the entire expression, we obtain the moment from the current's action on the south pole; so that as the sum of the moments from the current's action on the north and south poles of the needle is equal to the sum of the moments from the earth's action, we have at once, by putting M for the horizontal intensity of the earth's magnetism, the equation*

$$2\mu M\lambda\sin\theta=4\mu i\lambda\cos\theta\pi.\left[\frac{1}{A^3}+\frac{15}{4}\frac{1}{A^7}\cdot\left\{2B^2+y'^2-(xy\tan\theta+y'^2)\right.\right.$$
$$\left.\left.\left(\frac{2}{5}A^2-2B\right)\right\}+\&c.\right],\quad\ldots\ldots\ldots\ldots\quad(3)$$

on the understanding that all terms containing odd powers of λ are to be omitted in expanding the quantity under the bracket. It only remains to invert the equation thus found in order to have the value of i in terms of θ, which is the ultimate object of our research.

In effecting these operations, if we regard λ as a small quantity of the first order, and λ^2, α, β, γ as small quantities of the second order, and reject higher powers, we shall find†

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[*] From this equation the term $-\frac{3B}{A^5}$ has been left out, since it changes sign with λ.

[†] If the second powers of the displacements α, β, γ had been retained as well as of λ, we should have had under the bracket in equation (4) the

$$i = \tan\theta \cdot \frac{\mathrm{M} \cdot (1+a^2)^{\frac{3}{2}}}{2\pi} \cdot \left\{ 1 - \frac{3}{4} \frac{1-4a^2}{(1+a^2)^2} \cdot \lambda^2 \cdot (1 - 5\sin^2\theta) + \frac{3}{2} \cdot \frac{a}{1+a^2} \cdot (2\alpha + \beta\tan\theta) \right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

which becomes Bravais' formula by erasing the terms containing α and β.

In this formula the error occasioned by a separation between the point of suspension and the point midway between the poles of the needle is mixed up with other errors; it may, however, be distinguished from them by calling this interval of separation δ, and writing $\alpha + \delta\sin\theta$ instead of α, and $\beta + \delta\cos\theta$ instead of β; α and β will thus become the displacements of the point of suspension, and not, as heretofore, those of the magnetic centre of the needle. By this substitution equation (4) becomes

$$i = \tan\theta \cdot \frac{\mathrm{M} \cdot (1+a^2)^{\frac{3}{2}}}{2\pi} \cdot \left\{ 1 - \frac{3}{4} \cdot \frac{1-4a^2}{(1+a^2)^2} \cdot \lambda^2 \cdot (1 - 5\sin^2\theta) + \frac{3}{2} \cdot \frac{a}{1+a^2} \cdot (2\alpha + \beta\tan\theta + 3\delta\sin\theta) \right\}, \quad . \quad . \quad . \quad (5)$$

in which the effect of a faulty position of the needle in its cradle is separated from that arising from a defect in the adjustment of its point of suspension.

Two cases only need to be examined in detail: that in which $a = 0$, which is the ordinary form of tangent-galvanometer, and that in which $a = \frac{1}{2}$, which is Gaugain's arrangement*.

1. In the first case, since $a = 0$, equation (5) becomes

$$i = \tan\theta \cdot \frac{\mathrm{M}}{2\pi} \cdot \left\{ 1 - \frac{3}{4}\lambda^2 \cdot (1 - 5\sin^2\theta) \right\} \quad . \quad . \quad (6)$$

which

$$= \mathrm{K}\tan\theta \left(1 + \frac{15}{4}\lambda^2\sin^2\theta \right),$$

K being a constant so long as the same needle is used. From either of these formulæ a table may be once for all computed of the corrections to be applied in consequence of the length of the

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additional terms

$$+ \frac{3}{4} \frac{1-4a^2}{(1+a^2)^2} \cdot (2\alpha^2 - \beta^2 - \gamma^2 + 2\alpha\beta\tan\theta) + \frac{9}{4} \frac{a^2}{(1+a^2)^2} \cdot (2\alpha + \beta\tan\theta)^2,$$

of which the first part vanishes in Gaugain's arrangement of the galvanometer, and the second in the ordinary form. Neither these, however, nor the other terms of the fourth order containing $\alpha\lambda^2$ and $\beta\lambda^2$, could become sensible with any probable error of adjustment.

* *Comptes Rendus*, vol. xxxvi. p. 191.

needle; and the absence from them of α, β, γ and δ, teaches us that in the ordinary form of tangent-galvanometer, a slight derangement of the position of the needle in its cradle or in the position of its point of suspension will not cause sensible error.

2. In Gaugain's galvanometer $a=\frac{1}{2}$, and therefore from equation (5),

$$i=\tan\theta\cdot\frac{125}{128}\cdot\frac{\mathrm{M}}{\pi}\cdot\left\{1+\frac{3}{5}\cdot(2\alpha+\beta\tan\theta+3\delta\sin\theta)\right\}. \quad . \quad (7)$$

Hence in this description of galvanometer we get rid of the trouble of applying a correction for the length of the needle; but it becomes necessary to attend carefully to the position of the needle in its cradle, and to the horizontal adjustments of the point of suspension, lest errors should creep in of which it would be impossible to make any exact estimate. Accuracy of adjustment in a vertical direction is of less importance, since γ enters only in the second and higher powers.

To convey a clearer idea of the amount of error which may be expected to arise in using Gaugain's galvanometer, let α, β, γ, and δ be supposed each to have attained the magnitude 0·01*. Using this value in equation (7), we find that when the needle deviates 20°, the error of the observation would rise to more than $\frac{1}{50}$th of the whole amount, at 32° to above $\frac{1}{40}$th, and at 51° to above $\frac{1}{30}$th. No doubt these numbers are higher than need be provided against by any careful manipulator; but they suffice to show distinctly that the errors arising in this way cannot be safely disregarded, and that in conducting investigations in which accuracy is a point of much importance, the ordinary form of tangent-galvanometer is to be preferred†.

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* This is equivalent to assuming that a millimetre and a half is an amount which each displacement could be supposed to attain in a galvanometer whose current-circle has a diameter of three decimetres; or in British measure, displacements of about a sixteenth of an inch in galvanometers a foot across.

† In this investigation the magnetism has been conceived as collected into two points, the poles of the needle. In general, however, the magnetic intensity is some function ψ of u, the distance from the centre of the needle; whence using $2l$ for the length of the needle, it is easy to see that the only change we need make is to write in equations (4), (5), and (6), $\dfrac{\int_0^l u^2\psi(u)du}{\int_0^l \psi(u)\,du}$ instead of λ^2. This expression is obviously of the second order, and rejecting higher orders, becomes $\frac{1}{2}l^2$ if we assume the magnetic distribution to be that of a linear magnet, in which $\psi(u)$ is approximately equal to $k(e^{\alpha u}-e^{-\alpha u})$. (See Biot's *Physique*, vol. iii. p. 77.)

XIX. *Notices respecting New Books.*

A Manual of Qualitative Analysis. By Robert Galloway, *Esq., F.C.S.* 2nd Edition. 8vo. Pp. 197. Churchill, London.

BEGINNERS in the study of chemistry are much indebted to Mr. Galloway for the great pains he has taken, in the book before us and in his 'First Step in Chemistry,' to remove from the path of the student the many difficulties and obstacles which hinder his early progress. These difficulties, so puzzling and discouraging to the pupil, are too often forgotten or neglected by the compilers of such text-books; but our author has evidently devoted considerable and especial attention to them, and we think he has been successful in most materially facilitating the acquisition of sound knowledge in the first stages of chemical study.

The book is written in a very clear and intelligible style; in fact the author has perhaps sometimes committed the pardonable fault of repetition, in order to avoid any possible ambiguity. The second edition has also been considerably improved and enlarged, several new and useful chapters having been added. A most important feature of the system pursued consists in contrasting the chief properties of the individual members of each group of substances, by placing a description of these properties in parallel columns; thus the student, after experimentally demonstrating their behaviour with reagents, is enabled to devise for himself methods for the separation and individual detection of the different members of the group before him. We cannot but greatly admire this plan of teaching chemical analysis, which, whilst it calls into exercise the powers of observation, at the same time appeals to the judgement and reasoning faculties of the experimenter. The adoption of methods of study like this, cannot fail still further to increase the value of the natural sciences as instruments of intellectual discipline. We can confidently recommend Mr. Galloway's work as a valuable text-book, both to the regular laboratory student, and to the beginner who has not the advantage of professional instruction.

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 72.]

June 18, 1857.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"Contribution towards the History of Thialdine." By A. W. Hofmann, LL.D., F.R.S.

Researches into the behaviour of several organic bases with bisulphide of carbon, which I intend to lay before the Society at a later period, have led to some experiments with thialdine, the results of which I may be allowed briefly to state, since they are but loosely connected with the principal object of the inquiry.

It appeared of interest to gain some insight into the constitution of thialdine, which is the prototype of an important class of alkaloids containing sulphur. To what group of bases does this body belong? Is it a primary, a secondary, or a tertiary base? The favourite method of questioning bases by means of iodide of methyle promised to furnish some elucidation of this subject.

Thialdine, dissolved in iodide of methyle, to which conveniently its own bulk of ether has been added, furnishes after twelve hours a solid crystalline mass, which may be readily freed from adhering thialdine by washing with ether and recrystallizing from alcohol.

The crystalline compound produced in this manner is the iodide of a methylated base, generated, as proved by analysis, by the union of equal equivalents of the two constituents.

$$\underbrace{C_{12}H_{13}NS_4}_{\text{Thialdine.}} \quad + \quad \underbrace{C_2H_3I}_{\substack{\text{Iodide of}\\\text{methyle.}}} \quad = \quad \underbrace{C_{14}H_{16}NS_4I}_{\substack{\text{Iodide of}\\\text{methylthialdine.}}}$$

The deportment of the new iodide resembles that of iodide of tetramethylammonium, and the analogous iodides, which I have described some years ago. Insoluble in ether, soluble in alcohol, and precipitated from this solution by ether in the crystalline state, soluble in water with an acid reaction, and separated from it again, unchanged by potassa in the cold, this substance exhibits in general all the characters which distinguish the iodides of the so-called ammonium-bases.

The great mobility of the elements in thialdine, and especially the large amount of sulphur which it contains, afforded but little hope of successfully submitting the new iodide to the experiment with oxide of silver, which is so characteristic for the ammonium-bases. On adding oxide of silver to the aqueous solution of this body, which, as has been remarked already, possesses an acid reaction, iodide of silver is formed, and the liquid assumes at once a marked alkaline reaction; but since the almost simultaneous formation of sulphide of silver, and a powerful evolution of aldehyde, sufficiently indicate the perfect destruction of the compound, and since experiment denotes the presence of ammonia in the liquid, it is impossible to decide whether the alkaline reaction observed after decomposition is due to the liberation of an ephemeral ammonium-base, or to its product of decomposition.

My experiments appear nevertheless to establish that thialdine belongs to the tertiary bases,—that it is a nitrile-base.

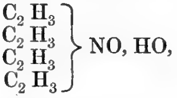
$$\text{Ammonia}\ \ N\left\{\begin{matrix}H\\H\\H\end{matrix}\right.$$

$$\text{Thialdine}\ \ N\left\{\ C_{12}H_{13}S_4.\right.$$

To the complex molecule, $C_{12}H_{13}S_4$, we must ascribe the faculty of replacing the 3 equivalents of hydrogen in the ammonia. In what manner, however, the elements are grouped in this complex molecule, whether we have here to assume a very unstable tribasic radical, or whether special molecules are inserted for each of the hydrogen-equivalents in the ammonia;—to decide these points further experiments are required.

Liebig and Wöhler, in their memoir on thialdine, state that the whole of the nitrogen of this base is eliminated in the form of ammonia, if the compound be decomposed by nitrate of silver. This deportment appeared to furnish an easy mode of controlling the formula of methylthialdine; after the decomposition of this body by nitrate of silver, I expected to find the whole of the nitrogen in the residuary liquid in the form of methylamine. On performing the experiment, it was found that the liquid contained considerable quantities of ammonia, but together with the latter alkali a fixed base. This unexpected deportment reminded me of experiments published several years ago *, on the action of oxide of silver on thialdine, which is said to be thus converted into *leucine*. The question presented itself, were the phenomena observed in the present case of an analogous character? had the above experiment given rise to the formation of methylated *leucine*?

Experiment proved that the decomposition of iodide of methylthialdine by oxide of silver gives rise to the formation of ammonia and of the oxide of tetramethylammonium



$$\left.\begin{array}{l} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ C_2H_3 \end{array}\right\} NO, HO,$$

which was satisfactorily identified by the analysis of the gold- and platinum-compound.

The following equation represents this change:—

$$\underbrace{4C_{14}H_{16}NS_4I}_{\text{Iodide of methylthialdine.}} + 20AgO + 2HO = 4AgI + 16AgS + \underbrace{12C_4H_4O_2}_{\text{Aldehyde.}}$$

$$+ 3H_3N + \underbrace{C_8H_{12}NO,HO}_{\text{Hydrate of the oxide of tetramethylammonium.}}$$

The absence of any leucine-like compound among the products of decomposition of iodide of methylthialdine induced me to repeat the experiment on the action of oxide of silver upon thialdine itself.

The transformation of thialdine into leucine, announced several years ago, and apparently very intelligible by the analogy of the formulæ of the two bodies

Thialdine.... $C_{12}H_{13}NS_4$
Leucine...... $C_{12}H_{13}NO_4$,

has not failed to rivet the attention of chemists. I wonder how such an error could have crept into science; for I have convinced

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* Goessmann, Chem. Gaz. vol. xii. p. 325.

myself, by a series of careful experiments, that this assertion is without the slightest foundation.

Thialdine, when treated with oxide of silver, as might have been expected, undergoes the same decomposition which Liebig and Wöhler had effected by means of nitrate of silver. It furnishes in this process aldehyde, acetic acid, and ammonia, but *no leucine*.

In conclusion, a remark which refers to a question connected with this alleged formation of leucine:—Soon after the statement regarding this subject had been made, Wagner[*] observed that the action of bisulphide of carbon upon amylamine gives rise to the formation of a crystalline compound. This compound was not analysed, but Wagner suggested that possibly it might be thialdine,

$$\underset{\text{Amylamine.}}{C_{10}H_{13}N}+2CS_2=\underset{\text{Thialdine.}}{C_{12}H_{13}NS_4},$$

and that in case this assumption proved to be correct, the action of bisulphide of carbon upon the homologues of ammonia might lead to the artificial formation, not only of leucine, but of glycocine, alanine, &c.

During my experiments on the deportment of bisulphide of carbon with organic bases, I have also had occasion to study the compound of amylamine with bisulphide of carbon. It is only necessary superficially to compare this substance with thialdine, in order to perceive at once that they are two absolutely different compounds; and if there is still a chance of producing leucine from amylamine, it is because the action of bisulphide of carbon does *not* give rise to the formation of thialdine.

"On the Action of Sulphuric Acid upon Anisic and Salicylic Acids." By A. W. Hofmann, LL.D., F.R.S.

In a paper read before the Royal Society[†], we have, Mr. Buckton and myself, directed attention to the general occurrence of the disulpho-acids, of which only few and scattered examples were known at that time.

M. Louis Zervas has since continued the study of this subject in my laboratory, and has arrived at the following results.

Anisic acid, treated with Nordhausen sulphuric acid, at a temperature of 100°, assimilates the elements of 2 equivs. of sulphuric acid, and is converted into a crystalline acid, containing

$$C_{16}H_8O_6+2HSO_4=C_{16}H_8S_2O_{12}+2aq.$$

The formula of this acid, which M. Zervas calls *sulphanisic acid*, was controlled by the analysis of a crystalline lead-salt and a barium compound, which were found to contain respectively

Lead-salt .... $C_{16}(H_6Pb_2)O_{12}+2aq.$
Barium-salt .. $C_{16}(H_6Ba_2)O_{12}+2aq.$

If the mixture of anisic and fuming sulphuric acid be submitted to higher temperatures than that of boiling-water, a lively evolution of carbonic acid takes place, containing, if the temperature be main-

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[*] Journal für Praktische Chemie, lxi. 505.

[†] See Phil. Mag. vol. xiii. p. 276.

tained within the proper limits, scarcely a trace of sulphurous acid. After several hours' digestion, the solution contains no longer anisic or sulphanisic acid, but a new acid, which has the composition

$$C_{14}H_8S_4O_{14}.$$

The formula of this acid, which could not be obtained in a state fit for analysis, was fixed by the analysis of a barium-salt, which was found to contain

$$C_{14}(H_6Ba_2)S_4O_{14}+2aq.$$

This acid may be considered as formed by the association of 1 equivalent of anisol (the product of the action of alkaline earths upon anisic acid at a high temperature) with 4 equivalents of anhydrous sulphuric acid. M. Zervas has, in fact, experimentally proved, that his acid may be obtained directly by treating anisol with fuming sulphuric acid, and he accordingly proposes to designate this compound by the name of *disulphanisolic acid*.

From the preceding experiments it is obvious that the action of sulphuric acid upon organic acids with 6 equivalents of oxygen, is perfectly similar to that which this agent exerts upon acids with 4 equivalents of oxygen.

Acetic acid.

1st Stage . . $C_4H_4O_4+2HSO_4=C_4H_4S_2O_{10}+2HO.$
2nd Stage. . $C_4H_4O_4+4HSO_4=C_2H_4S_4O_{12}+4HO+2CO_2.$

Anisic acid.

1st Stage $C_{16}H_8O_6+2HSO_4=C_{16}H_8S_2O_{12}+2HO.$
2nd Stage $C_{16}H_8O_6+4HSO_4=C_{14}H_8S_4O_{14}+4HO+2CO_2.$

There can be no doubt that salicylic acid, so closely allied to anisic acid, in fact, its homologue, a step lower in the system, must exhibit a similar deportment. Mr. Baldwin Duppa has been occupied with this subject in my laboratory, and has already obtained the first term, the *sulpho-salicylic acid,*

$$C_{14}H_6S_2O_{12},$$

which forms extremely beautiful compounds.

On submitting salicylic acid to the action of an excess of sulphuric acid, at a temperature of 180°, the same phenomena present themselves which are noticed in the case of salicylic acid, a powerful evolution of carbonic acid ensues, and the solution now contains a new sulphur-acid, which is crystalline. As yet Mr. Duppa has not succeeded in obtaining this acid in a state of sufficient purity for analysis, but it may be assumed, without much hesitation, that it will be found to be

Disulphophenic acid. . . . $C_{12}H_6S_4O_{14}.$

"On the Separation of Iodine, Bromine, and Chlorine, and the comparative degree of Affinity of these Elements for Silver; with some Analyses of their Combinations with that Metal occurring in Chili." By Frederic Field, Esq.

Although both bromide and iodide of silver are decomposed by the action of chlorine at an elevated temperature, yet chloride of silver is completely decomposed by bromide of potassium, and both the bromide and chloride of silver by iodide of potassium. Even the action of hot strong hydrochloric acid has but little influence upon the iodide of silver; many days of continuous boiling are necessary for its entire decomposition. I believe that it has been the opinion of chemists that chlorine possesses an affinity for silver superior to all other elementary bodies, and we are told in Gmelin's Handbook that all salts of silver, even the insoluble ones, are converted into chloride by solutions of metallic chlorides. From the following experiments it appears to me that bromine has a greater affinity for silver than chlorine, and iodine a still greater affinity than bromine.

When a mixed solution of bromide of potassium and chloride of sodium is added gradually to a solution of nitrate of silver, not in excess, no trace of chloride of silver is precipitated, as long as any bromide remains in solution.

If to a similar solution, iodide and bromide of potassium and chloride of sodium be added, iodide of silver and nitrate of potassa are formed, the bromide of potassium and chloride of sodium remaining undecomposed.

When bromide of potassium is poured upon chloride of silver, an entire decomposition ensues, bromide of silver and chloride of potassium being produced.

When iodide of potassium is added to chloride of silver, iodide of silver and chloride of potassium are formed; and when iodide of potassium is added to bromide of silver, there is a similar decomposition, the iodine replacing the bromine.

When chloride of silver in excess is agitated with a solution of iodide of potassium and warmed for some hours, no trace of iodine can be detected in the solution: when however chloride of sodium is poured upon iodide of silver, no decomposition occurs, neither is there any action upon bromide of silver with the same salt: and when bromide of potassium is added to iodide of silver, there is no alteration in the union of the elements.

From a number of experiments made in illustration of the preceding statements, I deemed it possible that the separation of chlorine, bromine and iodine could be accomplished by this reaction.

The method which I have devised is simply this: After weighing three equal portions of the salts to be analysed, they are placed in three flasks with ground-glass stoppers, and about an ounce of water is added to each; nitrate of silver being then added, slightly in excess, to the three, the stoppers are replaced, and each flask agitated violently. The precipitates subside in a few minutes, leaving the supernatant liquid perfectly clear. They are then filtered through separate funnels, and washed with hot water. No. 1 is dried and weighed. No. 2 is digested in bromide of potassium, dried and weighed; and No. 3 in iodide of potassium, dried and weighed.

To test the method, a mixture was made of 5 grains of iodide of potassium, 5 grains of bromide of potassium, and 5 grains of chloride of sodium. The following is a comparison of the theoretical and experimental results:—

	EXPERIMENT.	THEORY.
Iodine	3·69	3·81
Bromine	3·51	3·34
Chlorine	2·92	3·02

I have availed myself of this method in analysing several silver ores containing chloride, bromide and iodide of silver found in Chili, the formulæ of which I subjoin:—

Chloride of silver	Ag Cl.
Chlorobromide of silver	2Ag Cl, Ag Br.
Chlorobromide of silver	3Ag Cl, 2Ag Br.
Chlorobromide of silver	Ag Cl, 3Ag Br.
Bromide of silver	Ag Br.
Iodide of silver	Ag I.

"Note on the Density of Ozone." By Thomas Andrews, M.D., F.R.S., Vice-President of Queen's College, Belfast, and P. G. Tait, M.A., Fellow of St. Peter's College, Cambridge, and Professor of Mathematics in Queen's College, Belfast.

It is known that ozone can only be obtained mixed with a large excess of oxygen. In a former communication by one of the authors of this note, it was shown that in the electrolysis of a mixture of 8 parts of water and 1 of sulphuric acid, the mean quantity of ozone does not exceed ·0041 gramme in a litre of oxygen, or $\frac{1}{350}$th part. By using a mixture of equal volumes of acid and water, the relative quantity of ozone may be doubled; but even with the ozone in this more concentrated state, the ordinary methods of determining the density of a gas are plainly inapplicable. The difficulty of the problem was farther increased by the rapid action of ozone on mercury, which rendered it impossible to collect or measure the gas over that metal; and the tension of aqueous vapour, as well as the gradual destruction of ozone by water, prevented the use of the latter. After numerous trials, the method finally employed was to measure the change of volume which occurs in exposing a gaseous mixture containing ozone to a temperature of 230° C., or upwards. The volume of the gas after this treatment was invariably found to have increased; and by eliminating the effects of alteration of temperature and pressure during the course of the experiment, by the aid of a similar vessel to that containing the ozone, the authors succeeded in estimating the change of volume which took place, to an extremely small fraction of the entire amount.

The vessels employed in different experiments varied in capacity from 200 CC. to 600 CC., and terminated in tubes of about 2 millimetres in diameter, bent in a U-form and containing sulphuric acid. The amount of ozone was ascertained by passing a stream of the gas through two other vessels, one placed on each side of the vessel to be heated, and afterwards analysing their contents by the method described in the communication already referred to. It was easy to measure with certainty a change of pressure amounting to $\frac{1}{60,000}$th of the whole; but on account of the ordinary fluctuations of atmospheric pressure between two consecutive observations of the primary and auxiliary vessels, it was rarely possible to work to this degree of accuracy.

The experimental data have not yet been completely reduced, and some slight corrections have yet to be investigated; but the general result of the inquiry, which has been a very protracted one, gives—on the assumption that ozone is oxygen in an allotropic condition—for its density as compared with that of oxygen, nearly the ratio of 4 to 1.

The following approximate formulæ were employed in the reduction of the experiments. They are sufficiently exact for the purpose of calculation on account of the smallness of the quantities observed.

I. To reduce the change of level observed in the auxiliary vessel during the interval of the experiment to the equivalent quantity for the primary vessel—

$$\left(\frac{2}{a}+\frac{1}{\mathrm{H}}\right)\delta x=\left(\frac{2}{a}+\frac{1}{\mathrm{H}_{\prime}}\right)\delta x_{\prime}.$$

II. To deduce from the corrected change of level in the primary vessel the relative density of ozone and oxygen—

$$\frac{e-1}{e}=m\left(\frac{2}{a}+\frac{1}{\mathrm{H}}\right)\delta x_{\prime\prime}.$$

In these formulæ

a is the barometric pressure in terms of the sulphuric acid in the U-tubes.

H, the length of a tube of the same diameter as the U-tube of the primary vessel, and whose capacity is equal to that of the same vessel measured to the mean level of the acid in the U-tube.

$\mathrm{H}_{\prime}$, the same quantity for the auxiliary vessel.

$\delta x_{\prime}$, one-half of the change in the difference of levels in the U-tube of auxiliary vessel.

δx, the corresponding quantity for the primary vessel.

$\delta x_{\prime\prime}$, half the observed change in the primary vessel corrected by the quantity δx.

m, the ratio of the weights of oxygen and ozone in the gaseous mixture.

e, the relative density of ozone and oxygen.

"Contributions towards the History of the Phosphorus-, Arsenic-, and Antimony-Bases." By A. W. Hofmann, Ph.D., LL.D., F.R.S.

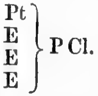
The facility with which the phosphorus-bases can be obtained by the action of zincethyle upon terchloride of phosphorus, has induced me to study the action of this agent upon the terchlorides of arsenic and antimony. I have found that triethylarsine and triethylstibine, hitherto obtainable only with difficulty, by submitting the alloys of potassium with arsenic and antimony to the action of iodide of ethyle, may be procured by this process as readily and as abundantly as the phosphorus-bases.

In attempting to identify the substances obtained by the new process with those prepared by the old one, by the analysis of platinum- and gold-salts, I have been led to the discovery of a series of compounds of remarkable beauty.

Since the study of these bodies, and especially of their derivatives, which are particularly numerous and interesting, will involve considerable time, I beg leave to submit to the Royal Society, before the session closes, a brief sketch of those substances, the composition of which I have already established by analysis.

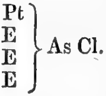
On adding a solution of bichloride of platinum to an alcoholic solution of triethylphosphine, of triethylarsine or triethylstibine as long as the colour of the platinum solution entirely disappears, the liquid deposits after a few moments magnificent slightly yellow or colourless crystals. The three salts thus formed may be considered as the chlorides of three compound ammoniums containing platinum and ethyle, united respectively with phosphorus, arsenic and antimony. They have the following composition:—

Chloride of Platophosphethylium...



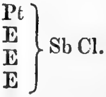
$$\left.\begin{matrix}\mathrm{Pt}\\ \mathrm{E}\\ \mathrm{E}\\ \mathrm{E}\end{matrix}\right\}\mathrm{P\,Cl}.$$

Chloride of Platarsethylium......



$$\left.\begin{matrix}\mathrm{Pt}\\ \mathrm{E}\\ \mathrm{E}\\ \mathrm{E}\end{matrix}\right\}\mathrm{As\,Cl}.$$

Chloride of Platostibethylium ....



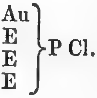
$$\left.\begin{matrix}\mathrm{Pt}\\ \mathrm{E}\\ \mathrm{E}\\ \mathrm{E}\end{matrix}\right\}\mathrm{Sb\,Cl}.$$

On substituting for the bichloride of platinum a solution of terchloride of gold, perfectly similar phenomena present themselves. The gold solution is entirely decolorized, and the colourless liquids deposit three gold compounds, which, remarkably enough, are of a dazzling white colour and silvery lustre.

Analysis has proved these salts to correspond to the platinum-salts just mentioned, the platinum being replaced by an equivalent quantity of gold.

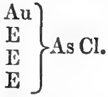
The formulæ of these substances are—

Chloride of Auro-phosphethylium..



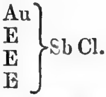
$$\left.\begin{matrix}\mathrm{Au}\\ \mathrm{E}\\ \mathrm{E}\\ \mathrm{E}\end{matrix}\right\}\mathrm{P\,Cl}.$$

Chloride of Auro-arsethylium ....



$$\left.\begin{matrix}Au\\E\\E\\E\end{matrix}\right\}As\,Cl.$$

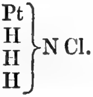
Chloride of Auro-stibethylium......



$$\left.\begin{matrix}Au\\E\\E\\E\end{matrix}\right\}Sb\,Cl.$$

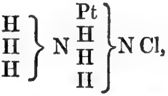
The complementary products formed in these reactions remain in the mother-liquors of the several salts, from which they separate in the form of oily compounds which gradually solidify into crystalline masses. These I have not yet examined.

A glance at the above formulæ shows that the new compounds which form the subject of this note correspond to the chloride of platammonium,



$$\left.\begin{matrix}Pt\\H\\H\\H\end{matrix}\right\}N\,Cl.$$

It remains to be ascertained whether the chloride of plato-diammonium,

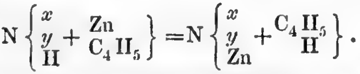


$$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N\left.\begin{matrix}Pt\\H\\H\\H\end{matrix}\right\}N\,Cl,$$

is likewise represented in the phosphorus-, arsenic-, and antimony-series.

"On a New Series of Compounds derived from Ammonia and its Analogues." By Edward Frankland, Ph.D., F.R.S.

Although zincethyle and its homologues are now well known to be capable of replacing electro-negative elements by ethyle, &c., yet it could scarcely have been anticipated, that substitutions of an almost opposite character would be effected by the same reagent; nevertheless I find that zincethyle is capable of removing one of the atoms of hydrogen in ammonia and its analogues, and of replacing it by zinc, thus forming a series of bodies which strongly remind us of the amide and nitride of potassium. The general nature of this reaction by which the compounds described below are produced, may be thus expressed:—



$$N\left\{\begin{matrix}x\\y\\H\end{matrix}+\begin{matrix}Zn\\C_4H_5\end{matrix}\right\}=N\left\{\begin{matrix}x\\y\\Zn\end{matrix}+\begin{matrix}C_4H_5\\H\end{matrix}\right\}.$$

When dry ammoniacal gas is passed through an ethereal solution of zincethyle, it is rapidly absorbed, and soon afterwards torrents of a combustible gas free from nitrogen begin to be evolved. After the current has been continued for more than an hour, the absorption altogether ceases, and the ethereal liquid deposits a copious white amorphous precipitate, which yielded on analysis numbers leading to the formula

$$N\left\{\begin{matrix}H\\H\\Zn.\end{matrix}\right.$$

The analysis of the combustible gas proved that 1 vol. consumed 3·43 vols. oxygen, and generated 1·95 vol. carbonic acid; numbers almost identical with those yielded by hydride of ethyle, with which the gas is also identical in specific gravity. These results prove that zincethyle acts upon ammoniacal gas in the following manner:—

$$N\left\{\begin{matrix}H\\H\\H\end{matrix}+\begin{matrix}Zn\\C_4H_5\end{matrix}\right\}=N\left\{\begin{matrix}H\\H\\Zn\end{matrix}+\begin{matrix}C_4H_5\\H\end{matrix}\right\}.$$

The solid product of the reaction, for which I propose the name *zincamide*, is a white amorphous body insoluble in ether, and instantly decomposed by water and alcohol, with evolution of great heat, and in such a manner as to regenerate ammonia. Thus with water the following equation expresses the reaction:—

$$N\left\{\begin{matrix}H\\H\\Zn\end{matrix}+\begin{matrix}H\\H\end{matrix}\right\}O_2=N\left\{\begin{matrix}H\\H\\H\end{matrix}+\begin{matrix}Zn\\H\end{matrix}\right\}O_2.$$

Heated with iodide of ethyle in a sealed tube at 145° C., zincamide gives iodide of zinc and iodide of diethylammonium—

$$N\left\{\begin{matrix}H\\H\\Zn\end{matrix}+2\begin{matrix}C_4H_5\\I\end{matrix}\right\}=N(C_4H_5)_2H_2I+ZnI.$$

Nitride of Zinc.

Zincamide can bear a temperature of 200° C. without decomposition, but at a low red heat it is decomposed into nitride of zinc and ammonia:—

$$3N\left\{\begin{matrix}H\\H\\Zn\end{matrix}\right.=2N\left\{\begin{matrix}H\\H\\H\end{matrix}\right.+N\left\{\begin{matrix}Zn\\Zn\\Zn.\end{matrix}\right.$$

Nitride of zinc is a grey pulverulent body, which is neither fused, decomposed, nor volatilized at a red heat out of contact with air. It is decomposed by water with great violence; in fact, if the nitride be merely moistened with water, it becomes red-hot. Several analyses prove that the formula of nitride of zinc is

$$N\left\{\begin{matrix}Zn\\Zn\\Zn.\end{matrix}\right.$$

The study of the products of its decomposition by water shows that the reaction may be thus expressed:—

$$N\left\{\begin{matrix}Zn\\Zn\\Zn\end{matrix}\right.+6HO=N\left\{\begin{matrix}H\\H\\H\end{matrix}+3\begin{matrix}Zn\\H\end{matrix}\right\}O_2.$$

Zincphenylimide.

$$N\left\{\begin{matrix}C_{12}H_5\\H\\Zn.\end{matrix}\right.$$

This body is produced by the action of zincethyle upon anhydrous aniline. The reaction is so violent that it requires to be moderated by diluting the zincethyle with ether. Heat is evolved, and a large quantity of a combustible gas is rapidly generated. At length the liquid becomes a white, semisolid, opake mass. Analysis proved the gas to be pure hydride of ethyle, whilst the solid body regenerated aniline in contact with water. The production of zincphenylimide may therefore be thus expressed :—

$$N\left\{\begin{matrix}C_{12}H_5\\H\\H\end{matrix}\right.+\left.\begin{matrix}Zn\\C_4H_5\end{matrix}\right\}=N\left\{\begin{matrix}C_{12}H_5\\H\\Zn\end{matrix}\right.+\left.\begin{matrix}C_4H_5\\H\end{matrix}\right\}.$$

Zincphenylimide is a white amorphous body very similar to zincamide. Like the latter body, it is instantly decomposed by water, aniline being regenerated :—

$$N\left\{\begin{matrix}C_{12}H_5\\H\\Zn\end{matrix}\right.+\left.\begin{matrix}H\\H\end{matrix}\right\}O_2=N\left\{\begin{matrix}C_{12}H_5\\H\\H\end{matrix}\right.+\left.\begin{matrix}Zn\\H\end{matrix}\right\}O_2.$$

Diethylzincamine.

$$N\left\{\begin{matrix}C_4H_5\\C_4H_5\\Zn.\end{matrix}\right.$$

This body is produced by the action of zincethyle upon diethylamine. The reaction requires to be aided by heat. Pure hydride of ethyle is evolved, and the following equation expresses the reaction :—

$$N\left\{\begin{matrix}C_4H_5\\C_4H_5\\H\end{matrix}\right.+\left.\begin{matrix}Zn\\C_4H_5\end{matrix}\right\}=N\left\{\begin{matrix}C_4H_5\\C_4H_5\\Zn\end{matrix}\right.+\left.\begin{matrix}C_4H_5\\H\end{matrix}\right\}.$$

Its reactions are quite similar to those of the bodies above described.

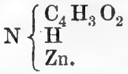
Zincoximide.

$$N\left\{\begin{matrix}C_2O_2\\H\\Zn.\end{matrix}\right.$$

Dry oxamide and pure zincethyle are without any action upon each other at ordinary temperatures, but the heat of a water-bath is sufficient to establish a violent reaction, torrents of pure hydride of ethyle are evolved, and zincoximide combined with zincethyle remains in the retort. The following equation expresses the reaction :—

$$N\left\{\begin{matrix}C_2O_2\\H\\H\end{matrix}\right.+2(ZnC_4H_5)=N\left\{\begin{matrix}C_2O_2\\H\\Zn\end{matrix}\right.+\left.\begin{matrix}Zn\\C_4H_5\end{matrix}\right\}+\left.\begin{matrix}C_4H_5\\H\end{matrix}\right\}.$$

Zincacetimide.



$$N\left\{\begin{array}{l}C_4H_3O_2\\H\\Zn.\end{array}\right.$$

Acetamide and zincethyle act upon each other very violently; a large quantity of gas is evolved, which analysis proves to be hydride of ethyle. Zincacetimide is contained in the residue as a white, amorphous, pulverulent solid, which is reconverted into acetamide by contact with water.

These reactions establish the fact, that by the action of zincethyle one of the atoms of hydrogen, in ammonia and analogous nitrogen compounds, combines with ethyle and becomes replaced by zinc. It will be interesting to extend this reaction to the nitryles, especially such as contain electro-negative radicals of the othyle family, since we may thus expect to obtain a new series of double radicals, amongst which the ketones will perhaps find a home. I hope soon to lay the results of this extension of the investigation before the Royal Society.

GEOLOGICAL SOCIETY.

[Continued from p. 78.]

December 2, 1857.—Col. Portlock, R.E., President, in the Chair.

The following communication was read:—

"On some peculiarities in the Microscopical Structure of Crystals, applicable to the determination of the Aqueous or Igneous Origin of Minerals and Rocks." By H. C. Sorby, Esq., F.R.S., F.G.S.

In this paper the author showed, that, when artificial crystals are examined with the microscope, it is seen that they have often caught up and enclosed within their solid substance portions of the material surrounding them at the time when they were being formed. Thus, if they are produced by sublimation, small portions of air or vapour are caught up, so as to form apparently empty cavities; or, if they are deposited from solution in water, small quantities of water are enclosed, so as to form *fluid-cavities*. In a similar manner, if crystals are formed from a state of igneous fusion, crystallizing out from a fused-stone solvent, portions of this fused stone become entangled, which, on cooling, remain in a glassy condition, or become stony, so as to produce what may be called *glass-* or *stone-cavities*. All these kinds of cavities can readily be seen with suitable magnifying powers, and distinguished from each other by various definite peculiarities.

From these and other facts, the following conclusions were deduced:—

1. Crystals containing only cavities with water were formed from solution.

2. Crystals containing only stone- or glass-cavities were formed from a state of igneous fusion.

3. Crystals containing both water- and stone- or glass-cavities

were formed, under great pressure, by the combined influence of highly heated water and melted rock.

4. That the relative amount of water present in the cavities may, in some cases, be employed to deduce the temperature at which the crystals were formed, since the accompanying vacuity is due to the contraction of the fluid on cooling.

5. Crystals containing only empty cavities were formed by sublimation, unless the cavities are fluid-cavities that have lost their fluid, or are bubbles of gas given off from a substance which was fused.

6. Crystals containing few cavities were formed slowly, in comparison with those of the same material that contain many.

7. Crystals that contain no cavities were formed very slowly, or by the cooling from fusion of a pure, homogeneous substance.

Applying these general principles to the study of natural crystalline minerals and rocks, it was shown that the fluid-cavities in rock-salt,—in the calcareous spar of modern tufaceous deposits, of veins, and of ordinary limestone,—and in the gypsum of gypseous marls, indicate that these minerals were formed by deposition from solution in water at a temperature not materially different from the ordinary. The same conclusions apply to a number of other minerals in veins in various rocks, and to many zeolites. The constituent minerals of mica-schist and the associated rocks contain many fluid-cavities, indicating that they were metamorphosed by the action of heated water, and not by mere dry heat and partial fusion.

The structure of the minerals in erupted lava proves that they were deposited from a mass in the state of igneous fusion, like the crystals in the slags of furnaces; but, in some of those found in blocks ejected from volcanos (for example, in nepheline and meionite), there are, besides stone- and glass-cavities, many containing water, the relative amount of which indicates that they were formed, under great pressure, at a dull red heat, when both liquid water and melted rock were present. The fluid-cavities in these aqueo-igneous minerals very generally contain minute crystals, as if they had been deposited on cooling from solution in the highly heated water. The minerals in trappean rocks have also such a structure as proves them to be of genuine igneous origin, but they have been much altered by the subsequent action of water, and many minerals formed in the minute cavities by deposition from solution in water.

The quartz of quartz-veins has a structure proving that it has been rapidly deposited from solution in water; and in some instances the relative amount of water in the fluid-cavities indicates that the heat was considerable. In one good case the temperature thus deduced was 165° C. (329° F.); and apparently, when the heat was still greater, mica and tinstone were deposited, and in some cases probably even felspar. There is, then, as has been argued by M. Elie de Beaumont, a gradual passage from quartz-veins to those of granite, and to granite itself; and there is no such distinct line of division between them as might be expected if one was a deposit from water, and the other a rock that had been in such a state of pure igneous fusion as the slags of our furnaces or the erupted lavas. When the constituent minerals of solid granite, far from contact with the stratified rocks, are examined, it is seen that they also contain fluid-cavities. This is especially the case with the quartz of coarse-grained, highly quartzose granites, in which there are so many, that the proportion of a thousand millions in a cubic inch is not at all unusual; and the enclosed water constitutes from one to two per cent. of the volume of the quartz. However, besides these fluid-cavities, the felspar and quartz contain excellent stone-cavities, precisely analogous to those in the crystals of slag, or erupted lavas; and thus the characteristic structure of granite is seen to be the same as that of those minerals formed under aqueo-igneous conditions in the blocks which are ejected from modern volcanos; and the very common occurrence of minute crystals inside the fluid-cavities still further strengthens this analogy.

The conclusion to which these facts appear to lead, is that granite is not a *simple igneous rock*, like a furnace-slag, or erupted lava, but is rather an *aqueo-igneous rock*, produced by the combined influence of liquid water and igneous fusion, under similar physical conditions to those existing far below the surface at the base of modern volcanos.

These deductions of the author, therefore, strongly confirm the views of Scrope, Scheerer, and Elie de Beaumont; and he agrees with them in considering it probable that the presence of the water during the consolidation of the granite was an instrumental, if not the actual cause of the difference between granite and erupted trachytic rocks.

December 16, 1857.—L. Horner, Esq., V.P.G.S., in the Chair.

The following communications were read:—

"On the Boring through the Chalk at Harwich." By Joseph Prestwich, Esq., F.R.S., F.G.S.

In this boring, which is made near the Pier at Harwich, the following succession of strata has been met with:—Earth, 10 ft.; Red Gravel, 15 ft.; London Clay, 23 ft.; Coarse dark Gravel, 10 ft.; Plastic Clay, 7 ft.; Bluish Clay with Greensand, 3½ ft.; Green and Red Sand intermixed, 5 ft.; Blue Clay, 3 ft.; Chalk with flints, 690 ft.; Chalk without flints, 160 ft.; Rocky Chalk in thin layers, 38 ft.; Greensand and Gault, 22 ft.; Gault with sand, 39 ft.; Dark Grey Slaty Rock, 44½ ft. Total 1070 feet. From this section it is evident that the Tertiaries, Chalk, Upper Greensand, and the Gault were met with in the usual order, though the last had less than the usual thickness; and that at a depth of 1025 feet an anomalous clay-slate occurred. This rock is without fossils, and is intersected with lines of joints or cleavage and of bedding, at considerable angles to the vertical axis of the bore-hole. This result of the boring at Harwich has necessarily an important bearing on the evidence furnished by the Kentish Town Well (Quart. Journ. Geol. Soc. vol. xii. p. 6); and, combined with the fact of coal-rocks having been found by the deep boring at Calais, tends to prove Mr. Godwin-Austen's view of the existence of a westward extension of a ridge of the crystalline and palæozoic rocks of the Franco-Belgian area beneath portions of the cretaceous deposits of the South-east of England. Mr. Austen considers that this palæozoic ridge may be partly composed of some of the coal-bearing strata, and that these may be covered in some parts of the South-east of England by little more than the cretaceous deposits; the Lower Greensand having been probably at some points the littoral or shoal deposits of the chalk-sea along this old ridge. Mr. Prestwich expressed his belief that the usual uniform extension of the Lower Greensand was certainly interrupted beneath London, and therefore presented a difficulty in the way of obtaining everywhere the supply of water from this formation which he had once anticipated; still however he thought the interruption in the deposit was but local, as the outcrop of the Lower Greensand to the north, west, and south of London was very uniform in its features.

"On a Granitic Boulder out of the Chalk of Croydon, and on the Extraneous Rock-fragments found in the Chalk." By R. Godwin-Austen, Esq., F.R.S., F.G.S.

The boulder which, together with some associated fragments and sand, formed the subject of this communication, was found by the workmen in a chalk-pit at Purley, about two miles south of Croydon. Mr. Symonds drew the attention of Dr. Forbes Young to this interesting discovery, and the latter gentleman secured what remained of the boulder after it had been much broken up, and presented it to the Society. The largest remaining fragment is apparently one end of an irregularly oval well-rounded boulder, originally about 3 feet long. The boulder was accompanied by some decomposing fragments of a felspathic trap-rock, and with a compact mass of siliceous sand, which Mr. Godwin-Austen carefully exposed on a visit to the chalk-pit. This collocated mass of rock-fragments and sand the author regarded as being truly water-worn beach-material, derived from some old coast-line of crystalline rocks. Other smaller specimens of crystalline rocks, quartzites, &c. have been found in the Chalk of the South-east of England. These are all water-worn: some are quite rounded; and many of them bear the remains of attached shells and zoophytes: but they are nearly always isolated in position, except at Houghton (Sussex), where they were met with scattered over a uniform level.

The author proceeded to describe the conditions of the "marginal sea-belt,"—where pebbles are found in existing seas, and where also certain molluscs and zoophytes having habits of attachment occur. From such a marginal zone floating sea-weed might have borne the majority of the extraneous pebbles now found in the Chalk. Of this formation, the author observed that the "White Chalk" ranged as far north as a line reaching from the North of Ireland to Riga, on the Baltic, and extended in a broad zone over North Germany. In North Europe the conditions of the deposit were very uniform over the Anglo-French area, where 800 feet is its average thickness, and where it is of deep-water origin. Its fauna, however, is somewhat anomalous; much of it has drifted from shallower zones of the sea-bed. The littoral or marginal shingles of the lower cretaceous series lie towards the west; that of the Lower Greensand is seen in the Farringdon Gravels; that of the Gault in the Halden Sands. The Chalk proper filled up the deeper and subsiding sea-bed at a period synchronous with the deposition of some of these littoral beds, and at the time of the greatest extension of the area of the Cretaceous Ocean, the littoral beds of which are recognizable in the South of Norway and Sweden, in Westphalia and Rhenish Prussia, &c., and the area of which probably may be regarded as reaching from the Rocky Mountains at the head of the Missouri, over Texas, Florida, the eastern side of the Alleghanies, the West Indies, and a broad belt of the Atlantic, to North Africa, Central and Northern Europe, with bold extensions into Western, Central, and even Eastern Asia. Central Europe then presented the aspect of a huge archipelago from its many extensive islands, of which one of the largest was an area comprising the chief part of France, the north-east of Britain, Ireland, Norway, Sweden, Lapland, and what are now the separating channels; together with a part of the Atlantic to the west and south. From the northern part of the old land-area the author believes that the granitic boulder of Croydon was derived. And, as it is, in his opinion, too massive to have been transported by floating trees, as Mr. C. Darwin describes an isolated rock-fragment to have been conveyed to the coral-islands of the Keeling group,—or by sea-weeds (the floating-powers of which the author has studied in the English Channel),—Mr. Godwin-Austen refers to an ice-floe as the agent by which such a block could alone have been lifted from the coast and conveyed far out to sea. The possible occurrence of rare and isolated boulders in the chalk-sea under such conditions was analogous, in Mr. Austen's opinion, to the occasionally extended voyage of icebergs at the present day to the coast of Ireland, the Azores, and even to the Madeira Islands.

XXI. *Intelligence and Miscellaneous Articles.*

THE TELESTEREOSCOPE.

To the Editors of the Philosophical Magazine and Journal.

77 Rose Street, Edinburgh,
Jan. 12, 1858.

GENTLEMEN,

ALLOW me to call your attention to the fact that Professor Helmholtz's *Telestereoscope*, described in your last Number, is essentially the same as the Pseudoscope (the exaggerating form of it), my description of which appeared in your Number for June 1853. This is no doubt another instance of what so frequently happens—two persons independently studying the same subject arriving at the same result.

Professor Helmholtz's instrument being designed for viewing distant objects, he gives a very cursory statement of its effects when directed to near ones,—and, if I understand it aright, not quite a correct statement. He says that if the large mirrors are kept parallel to the small ones, the "right" or natural relations may be maintained between the dimensions of the object in the direction of the depth of the field of view and those on the surface. Now this is not possible with any arrangement of the mirrors; for the two retinal images being formed by visual rays which enter the instrument at points considerably wider apart than the distance between the eyes, must always have their difference so exaggerated as to produce an exaggerated relief. It is true that when, in uniting the two images, the optic axes are made to converge to a distance considerably nearer than the object, the binocular image appears reduced in size; but this kind of reduction does not restore the natural relation between the two dimensions, although an optical reduction produced by *concave glasses* does. Perhaps it is the use of concave glasses in his instrument that has led the Professor into this mistake. But the fact is, that the reduction in size produced by increased convergence of the eyes is not proportional in the nearer and more distant parts of the same object; and hence, besides elongation in depth, there is a distortion in figure; thus a sphere always appears through the instrument as *an egg with its smallest end towards the observer*, whatever be the degree of optic convergence at which its two images are united.

The idea of combining my pseudoscope with the opera-glass so as to correct the flatness of the binocular images given by the latter, occurred to me very soon after my description of the pseudoscope was published in 1853; and I now send you a rough sketch of a stereoscopic opera-glass which I designed in November of the same year. You will observe that it is just Professor Helmholtz's Telestereoscope, with the object-glasses placed outside the large mirrors; the only other difference being, that I proposed to substitute reflecting prisms with concave refracting surfaces for the small mirrors and concave lenses. This sketch I sent with a note to Mr. Adie, optician here, but he could not see a sufficient advantage the instrument would have over the common opera-glass, to warrant him in undertaking its manufacture as I suggested. He returned the drawing, and it has lain by me ever since.

I am, Gentlemen,
Your obedient Servant,
WALTER HARDIE.

[We have not considered it necessary to reproduce the sketch forwarded by our correspondent, the difference being sufficiently pointed out in the text.—ED.]

ON THE RELATIONS EXISTING BETWEEN CERTAIN GROUPS OF CRYSTALLINE FORMS BELONGING TO DIFFERENT SYSTEMS.

BY C. MARIGNAC.

It has long since been remarked that in each system the different forms are not distributed by chance. They seem, on the contrary, to be combined in such a way as to form a certain number of groups, outside of which there are only a small number of forms pretty distant from one another, and which will perhaps some day become types of new genera.

It would be interesting to discover the common cause which determines the analogy of form in the different substances of the same group. If it may be explained sometimes with Mitscherlich, by analogy of atomic constitution, and sometimes also perhaps by a relation between the atomic volumes, most frequently the cause of this analogy cannot be suspected.

The rhombohedric system contains one of these groups which is very remarkable. Bromate of potash, besides the cubic form of which M. Rammelsberg has recently expressed some doubts, crytallizes in rhombohedra of 86° 18′. This form causes it to enter into a very numerous group, including very various simple and compound bodies, which, however, nearly all present a common and curious character, namely that by their nature, or by the analogy of their constitution with that of other compounds, one would be led to suppose that they had a cubic crystallization, but for the power of determining their forms.

The following are the bodies composing this group :—

Arsenic, tellurium, antimony, bismuth : their quality of simple bodies, and, especially as regards two of them, of simple metals, rendered it probable that they would have a form belonging to the regular system ;

Peroxide of iron, oxide of chrome, alumina and glucina, corresponding in constitution with oxide of antimony and arsenious acid ;

Bromate of potash ; the corresponding iodate is cubic.

The preceding substances particularly affect rhombohedric forms, and are all derived from rhombohedra of which the angle is comprised between 85° 4′ and 87° 4′. The following crystallize rather in hexagonal forms, but are naturally derived from rhombohedra of which the angle would be between 83° and 86° :

Oxide of zinc, sulphurets of cadmium and nickel, magnetic pyrites, iodide of silver, fluosilicate of soda, and bromate of didymium with 6 equivs. of water. Now we find in the regular system,—magnesia (periclase), sulphurets of zinc and cobalt, chloride and bromide of silver, the fluosilicates of potash and ammonia, and the bromates of magnesia, zinc and cobalt with 6 equivs. of water.

Besides these sixteen substances, the relations of which with bodies belonging to the regular system cannot be overlooked, there are ten others of which the forms are included in the same group, but for which we do not clearly perceive similar relations, although it is not impossible that they may exist. These are,—

Arseniuret and antimoniuret of nickel, osmiuret of iridium, stannate of soda, sulphate of lithia, the hyposulphates of strontia and lead, aldehydate of ammonia, nepheline and Cancrinite.

At the first glance this curious relation might seem to be explained by the ordinary theory of isomorphism,—the substances above mentioned being forms very nearly allied to the cube, and consequently isomorphous with the cube.

But if this were the cause which unites all these substances in this group, their forms ought to arrange themselves uniformly around that of the cube, regarded as a rhombohedron of 90°. This is not the case. The angle of the primitive rhombohedron for all these twenty-six bodies, varies between 83° and 87° 40′; its average value is about 85° 30′. From 88° to 94°, or between the most extended limits comprising the angle of 90°, there are only ten substances, amongst which only one, the chloroplatinate of æthylamine, is related in its nature to the compounds belonging to the cubic system. The author therefore considers that this hypothesis must be abandoned, but does not propose another.

In the square prismatic system there is also a tolerably numerous group of substances of which the forms may be derived from square octahedra very near to the regular octahedron. This group, however, presents less interest than the preceding one. Of thirty substances of which it is composed, there are only eight or ten which offer, in their constitution, some relations to the cubic system; these are,—chlorate and bromate of silver, iodate of ammonia, iodide and cyanide of mercury, braunite, and iodide of tetramethylammonium. On the other hand, the forms of these substances group themselves with tolerable uniformity around that of the regular octahedron, which they approach closely, so that there would be no difficulty in regarding them as really isomorphous with the bodies to which they approach in constitution, and which crystallize in the regular system.—*Comptes Rendus,* Oct. 26, 1857, p. 650.

ON THE COMPOSITION OF EPIDOTES, VESUVIANS AND GARNETS.

BY R. HERMANN.

1. *Epidotes.*—The old opinion that these minerals are similar in composition, so that their proportions of oxygen are $RO : R^2O^3 : SiO^2 = 1 : 2 : 3$ (which composition is still admitted by Rammelsberg), is rejected by the author. Nor does Scherer's opinion, that in epidotes water : RO and the alumina and oxide of iron : SiO^3 are polymerically isomorphous, so that their formula is $(RO)^3 (SiO^3)^4$, agree with the author's analyses of epidotes in which he determined the protoxide of iron.

Hermann rather supposes that in epidotes 3 atoms RO may displace 1 atom R^2O^3, and that the fundamental mixture of all minerals crystallizing in the form of epidote consists of the molecule $(RO, R^2O^3)^2 SiO^2$. This fundamental molecule may combine with various quantities of diversely composed accessory molecules without injury to its form.

In the epidotes, in the restricted sense, the accessory molecule consists of RO, HO; in the orthites, of HO.

The general formula of the epidotes in the restricted sense is therefore $= (RO, R^2O^3)^2 SiO^2 + nRO, HO$, and that of the orthites $=(RO, R^2O^3)^2 SiO^2+nHO$.

In all epidotes, properly so called, therefore, the sum of the atoms of oxygen of $(RO+R^2O^3)$ must be equal to the sum of the atoms of oxygen of (SiO^2+HO); in all orthites, on the contrary, the sum of the atoms of oxygen of $(RO+R^2O^3)$ will be equal to the atoms of oxygen of SiO^2, to which various quantities of water may then be added.

2. The *vesuvians* were formerly regarded as minerals possessing a similar constitution to the garnets, the proportions of oxygen in $RO : R^2O^3 : SiO^2$ being in the relation of 1 : 1 : 2. The author, however, found another proportion, which has been confirmed by Scherer and Rammelsberg.

Rammelsberg supposes that the vesuvians are constituted according to the proportions $3(RO)^3 SiO^3 + 2R^2O^3 SiO^3$; Scherer, on the contrary, gives them the constitution $(RO)^3 (SiO^3)^2$.

Here also the author supposes that the bases RO and R^2O^3 are mutually displaced. Their fundamental molecule must be expressed, just like that of the epidotes and garnets, by $(RO, R^2O^3)^2, SiO^2$. This fundamental molecule in the vesuvians also includes small and variable quantities of water. In the vesuvians, therefore, the sum of the atoms of oxygen of $RO+R^2O^3$ will be equal to the number of atoms of oxygen of the silicic acid combined therewith, to which also various quantities of water may be added. The general formula of the vesuvians would consequently be $(RO, R^2O^3)SiO^2+nHO$.

3. The *garnets* contain RO, R^2O^3 and SiO^2, usually in a proportion in which the oxygen corresponds with the numbers 1 : 1 : 2. In the garnets, indeed, the proportion of oxygen of $RO : R^2O^3 : SiO^2$ is subject to variation. In all cases, however, the sum of the atoms of oxygen of $RO+R^2O^3$ is equal to the atoms of oxygen of SiO^2. The garnets consequently have the same fundamental molecule as the epidotes and vesuvians, namely $(RO, R^2O^3) SiO^2$, and are therefore, as they also contain the same chemical constituents as those minerals, polymorphic with them. In individual cases the variations of $RO : R^2O^3$ go so far that R^2O^3 is completely displaced. The minerals which, according to the author, belong here are the following:—

Garnet group $= (RO, R^2O^3)^2 SiO^2$.

1. Garnet $= (RO, R^2O^3)^2 SiO^2$.
2. Sodalite $= 6(RO, R^2O^3)^2 SiO^2+NaCl$.
3. Hauyne $= 3(RO, R^2O^3)^2 SiO^2+CaO SO^3$.
4. Nosean $= 6(RO, R^2O^3)^2 SiO^2+NaO SO^3$.
5. Ittnerite $= (RO, R^2O^3)^2 SiO^2+n(NaO SO^3)$.
6. Helvine $= 3(RO^2) SiO^2+Mn$.

—*Journ. für Prakt. Chemie*, vol. lxx. p. 321.

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[FOURTH SERIES.]

MARCH 1858.

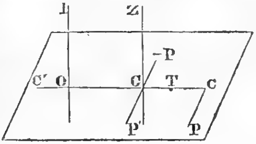
XXII. *On the Percussion of Bodies.* By M. Poinsot*.

Chapter I.

Theory of the centres of percussion, and of spontaneous rotation, as well as of other new centres possessing remarkable properties with respect to the motion of bodies.

I. On the centre of percussion.

1. LET us consider a free body of any form whatever, and let us suppose it to receive a sudden impulse in virtue of the action of a single force P, the direction of which passes at an arbitrary distance from its centre of gravity G. From this centre G let fall a perpendicular G C upon the direction of the force P. The foot C of this perpendicular, where the impulse P may be supposed to be immediately applied, is the point which I shall call the *centre of percussion*.



2. The body which has received this impulse being left to itself, if we consider the individual forces with which its several molecules are animated at any period whatever during its motion, we shall find that all these forces are reducible to a single one, identical with the original impulse P which set the body in motion. This conclusion evidently results from the general principle of the *conservation of forces and moments*.

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* The researches of Poinsot are too well known to all mathematicians to render necessary any indication of their merits. Mathematicians at this day, too, are so well acquainted with the current mathematical literature of our continental neighbours, that for them even an announcement of the publication, in *Liouville's Journal* for September 1857, of Poinsot's most recent

Hence at the same distance from its centre of gravity G, there will always exist a *centre of percussion* C in the body; that is to say, a point to which, if the body were at rest, a single force might be applied which would be capable of producing the motion which actually exists, and hence there will always be a point by means of which the movement of the body may be arrested, either by there applying a single force equal and contrary to the original impulse, or by there presenting an obstacle or fixed point for the body to strike against.

3. But although a body whose motion is due to a single impulse preserves a centre of percussion throughout its subsequent motion, we cannot affirm that there is always a centre of percussion in a moving body; for its motion may have arisen from the action of forces incapable of reduction to a single one, in which case no single force can exist capable of giving to the body the precise motion it possesses, and consequently there can be no centre of percussion in the body. In another place we shall examine the motion of a body animated by any forces whatever; here we shall merely consider the particular case where the body in question has been set in motion by a single impulse P, and we shall, moreover, assume the direction of this impulse to be contained in a plane passing through the centre of gravity G, and perpendicular to one (G Z) of the three principal axes of the body.

4. Let M always represent the mass of the body, and MK^2 its

memoir is superfluous; for their use, an English version of this memoir is certainly not called for.

Nevertheless, the works of the able author of the *Théorie nouvelle de la rotation des corps* are far from being so familiar to Englishmen generally as they deserve to be. Even apart from the results they contain, and solely in virtue of the method they manifest, Poinsot's memoirs, attentively studied, have a peculiar value, especially to a student whose own method of working has not yet become habitual.

Nowhere do we meet with greater clearness in the treatment of the subject, nowhere with closer reasoning on the problem in hand, or a happier combination of veritable analysis and synthesis. Calculus,—that general servant whose convenient readiness is often abused—is always at hand to assist and confirm, but never to direct. Poinsot may be said always to work *with*, but never to entrust work *to*, this his servant; he knows too well, indeed, how invaluable, for a full grasp of the whole question, is that insight which is alone the reward of hard, close, and attentive reasoning.

The translation of the following memoir, with the consent of the author and with the advantage of occasional suggestions from him, has been an easy, and certainly a pleasant task. The original research, it may be well to state, was virtually completed no less than twenty years ago, although it has been published but a few months; a knowledge of this fact may explain some things, and will certainly not diminish the value of the whole. With respect to the occasional notes and references which will be found throughout the memoir, it is only fair to add that none of them appear in the original.—T. A. Hirst.

moment of inertia, that is to say, *the sum of the products of all its particles by the squares of their respective distances from the axis* G Z *under consideration.* It will be at once seen *that the square, whose side is the line* K, *is merely the mean of the squares of the distances of all the equal particles of the body from the principal axis of rotation,* GZ. For each body it is a given, constant quantity, dependent solely upon the figure formed by the different points composing that body.

This being understood, let h be the distance C G from the centre of gravity G to the centre of percussion C, and let us see to what the whole movement of the body, caused by the application of this unique impulse P, resolves itself.

II. ON THE SPONTANEOUS CENTRE OF ROTATION.

5. The force applied at C may be replaced by another equal, parallel, and like-directed force P′, applied at G, and by a couple (P, −P), applied to the arm $CG = h$, and having the moment P h.

The force $P' = P$, applied at the centre of gravity C, imparts a common velocity $v = \frac{P}{M}$ to all the particles of the body; and the couple whose moment is Ph, causes the body to turn around its *principal axis* G Z with an angular velocity $\theta = \frac{Ph}{MK^2}$*.

In consequence of this double movement, a point O, situated in the production of the line CG beyond the centre of gravity and at the distance $OG = a$ from the same, would be endued at one and the same time with two contrary velocities, the one $v = \frac{P}{M}$, the other $a\theta = \frac{aPh}{MK^2}$. Consequently, in order to find, upon the production of CG, that point O of the body with respect to which these two contrary velocities are equal, we have merely to set

$$\frac{P}{M} = \frac{aPh}{MK^2},$$

from which equation we immediately deduce

$$ah = K^2,$$

and consequently, for the distance a of the point O from the centre of gravity G, the value

$$a = \frac{K^2}{h}.$$

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* *Théorie nouvelle de la rotation des corps,* par M. Poinsot, 1re partie, art. 46.

6. Thus upon the line drawn from the centre of percussion through the centre of gravity, and on the opposite side of this latter centre, there is always a point O of the body which, during the first instant, remains at rest, or in other words, receives no motion whatever from the percussion applied at C. The same may be said of all points of the line OI, drawn through O parallel to the principal axis GZ.

For an instant, therefore, the whole motion of the body resolves itself into a simple rotation around this line OI, just as if the same were fixed; this line OI is called the *spontaneous* axis of rotation, since it might be said that at the first instant the body itself makes choice of the same. But as the only point of this spontaneous axis which will here enter into consideration is the point O in line with the centres of gravity and percussion, we shall refer to O as the *spontaneous centre* of rotation.

7. The point of the body which constitutes the spontaneous centre of rotation has, therefore, a velocity equal to zero, and during the instant under consideration the body turns around this point.

Since the centre of gravity has a velocity expressed by $a\theta$*, and since a is the distance of G from O, it follows that the body has the same angular velocity θ around the spontaneous axis that it has around the centre of gravity G during the translation of this centre through space.

Hence any point whatever at the distance y from the spontaneous centre O has the velocity $y\theta$; for instance, the centre of percussion C, at the distance $a+h=l$, has the velocity $l\theta$.

It will be observed that the spontaneous centre O is not, like the centre of gravity G, a point which remains always the same in the interior of the body: it changes every instant, or rather, it is a new point of the body which, every successive instant, becomes the spontaneous centre, just as it is a new point which becomes the centre of percussion. In fact, since these two centres C and O, together with the centre G, must necessarily be situated on a right line perpendicular to the path described by the centre of gravity, and at the constant distances h and a, respectively, from this centre G, it is clear that during the movement of the body C and O describe, as it were, two circumferences of circles around the centre of gravity whose radii are h and a respectively, or, to speak more accurately, the several points of these circumferences become, successively, the centres of percussion and of spontaneous rotation.

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* For $v=\frac{P}{M}=\frac{P}{M}\cdot\frac{ah}{K^2}=a\theta$, since $\frac{ah}{K^2}=1$.

COROLLARIES.

8. The equation

$$ah = K^2,$$

which gives the distance of the point O from the centre of gravity G, shows that the position of the spontaneous centre depends neither upon the mass M of the body, nor upon the intensity P of the impulse applied at the centre of percussion C, but solely upon the distance h between this point C and the centre of gravity G. The product ah having always the same value, it follows that a increases in the same proportion as h decreases; thus the nearer the centre of percussion approaches the centre of gravity, the more the spontaneous centre on the other side recedes from the same, and *vice versâ.*

9. If we suppose $h=0$, we find $a=\infty$. Hence one might say that when the centre of percussion coincides with the centre of gravity, the spontaneous centre is infinitely distant: but, strictly speaking, there is then no spontaneous centre of *rotation*; for, since the body, in this case, receives a shock at its centre of gravity, the only effect will be a simple translation in space.

10. On the other hand, if we suppose $h=\infty$, we find $a=0$; that is to say, the spontaneous centre coincides with the centre of gravity. But, strictly speaking, there is no longer a centre of percussion in this case; for if the body turns around its own centre of gravity, which remains fixed in space, the percussion P, which set it in motion,—no matter at what point we suppose the same to be applied,—must necessarily be zero, otherwise the centre of gravity would not be motionless, but would have a finite velocity $\frac{P}{M}$. Thus there is no simple and finite percussion P which can give rise to a spontaneous centre coincident with the body's centre of gravity.

All that can be said is, that if the centre of percussion is infinitely distant from the centre of gravity, the spontaneous centre is infinitely near to the same; but in dynamics it does not follow from this that these two points of the body can ever coincide, for the one G will have the finite velocity $\frac{P}{M}$, whilst the velocity of the point O is from its very nature zero.

In one way only can we imagine the centre of gravity and the spontaneous centre strictly to coincide; it is by supposing $P=0$, $h=\infty$, and the product or moment Ph equal to a *finite* quantity. Under this mathematical hypothesis, the body would turn around its centre in virtue of the finite moment Ph, and this centre would remain at rest in consequence of $P=0$. It must be confessed, however, that a percussion *zero* applied at an *infinite* distance furnishes to the mind no distinct conception, and that thereby we perceive neither percussion nor centre of percussion.

To explain this peculiar case, therefore, it will be necessary to consider it apart. It is evident that if the body turns around its centre of gravity, it does so because the impulse which it has received proceeded, not from the application of a single force, but from that of a *couple.* Here, therefore, we have to do, not with a percussion zero, but with a percussion of a different nature, where there is neither centre nor determinate arm to consider; for it is a couple which enters into the question, and this couple may be changed at will into innumerable others having an equivalent effect.

III. RECIPROCAL PROPERTIES OF THE CENTRE OF PERCUSSION AND OF THE SPONTANEOUS CENTRE.

11. The same equation

$$ah = K^2,$$

which expresses the relation between the distances h and a of the centre of percussion, and of the spontaneous centre from the centre of gravity, also shows that these two centres are, to a certain extent, *reciprocal*; that is to say, if the body were to receive an impulse at the point O, which would then become the centre of percussion, rotation would take place around the point C, which would become the spontaneous centre.

12. If the distance $a+h$ between these two reciprocal centres be represented by l, then since $ah=K^2$, we shall have indifferently

$$l = a + \frac{K^2}{a} \text{ or } l = h + \frac{K^2}{h}.$$

13. This distance l between the two reciprocal centres may be greater than any given line, but it can never become zero. The line 2K is the least value it can have; for, on seeking the *minimum* value of l, we find $a=K$, and consequently $h=K$, whence 2K is the distance between the two reciprocal points of a given body which are nearest each other.

IV. ON THE CENTRE OF *MAXIMUM* PERCUSSION.

14. According to what has been above demonstrated, if the body under consideration actually turns around a point O, situated at the distance a from its centre of gravity G, the whole movement of the body may be attributed to the action of a single force applied to the point C, at the distance $l = a + \frac{K^2}{a}$ from the centre of rotation O; and hence all the different forces with which the several molecules of the body are at this moment animated, are reducible to a single one, passing through the point

C, in a direction perpendicular to the line OG. If, therefore, an equal and contrary force were opposed at this point C, or if a fixed obstacle were there presented to the body, all its motion would be destroyed.

From this it would appear as if this point C were that by means of which the body, turning around the point O, would strike any obstacle or fixed point it might encounter with the greatest possible force; hitherto, in fact, this point or centre of percussion has been regarded as that corresponding to the greatest percussion which the body is capable of producing against an obstacle, and it is precisely by this property of *maximum* percussion that authors most frequently define this point, as may be seen in the *Encyclopédie,* and in most treatises on Mechanics*.

It will be seen, however, that the point by means of which the body strikes with the greatest force is not the above-mentioned ordinary centre of percussion, but a new point T, situated between the same and the centre of gravity G, to which it will be convenient to give a particular name. Or, should it be deemed advisable to avoid new terms, then, since the point C is in reality nothing more than the centre of the unique impulse with which the body is animated, we might call it the *centre of impulsion,* and reserve the name *centre of percussion* for the new point T to which it is really appropriate, since T is the point of greatest percussion of the body. But in order not to change denominations already accepted, I shall simply refer to the point T as the centre of *maximum* percussion. With respect to its precise position in the body, we shall find that its distance from the centre of rotation O is the geometrical mean between the

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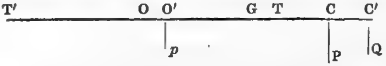
* In the *Encyclopédie ou dictionnaire raisonnée des Sciences,* 1765, compiled under the direction of d'Alembert, we find the centre of percussion thus defined: "C'est le point dans lequel le choc ou l'impulsion d'un corps qui en frappe un autre est la plus grande qu'il est possible . . . . ou bien, le point dans lequel toute la force de percussion du corps est supposée ramassée."

In the *Dictionnaire des sciences mathématiques pures et appliquées, par une société d'anciens élèves de l'Ecole Polytechnique,* 1840, we find, added to the above definition, the following: "C'est le point autour duquel, l'élan des parties (du corps) est balancé de chaque côté, de manière à être arrêté par un obstacle immuable à ce point, et à y rester sans agir sur le centre de suspension." We will add, that Barlow, in his article on Mechanics in the 'Encyclopædia Metropolitana,' defines the centre of percussion by this last property solely. It is only as a "partial deduction" that he afterwards adds (on p. 139), "When a pendulum, vibrating with a given angular velocity, strikes an obstacle, the effect of the impact will be greatest if it be made at the centre of percussion; for, in this case, the obstacle receives the whole revolving motion of the pendulum; whereas, if the blow be struck in any other point, a part of the motion of the pendulum will be employed in endeavouring to continue the rotation."

distances of the centre of gravity, and of the ordinary centre of percussion from the same point O.

But to arrive at this theorem, let us first seek the percussion which the rotating body would produce at a *fixed* point C′, situated at any distance x from the centre of gravity G. Afterwards we shall see what value of x would render this percussion a *maximum.*

15. At the moment of impact we may regard the body as if, after having been previously in a state of repose, it was suddenly acted upon by a single force P applied at the point C. We may always conceive this force P, too, to be decomposed into two other parallel forces; the one, Q, being applied at the point C′, and the other, p, at O′, the *reciprocal* of C′; that is to say, at the point which would correspond, as spontaneous centre of rotation, to the point C′ considered as a centre of percussion. The point C′ being at the distance x from G, the point O′ is on the other side at the distance $\frac{K^2}{x}$. The distance between the components Q and p, therefore, is $x+\frac{K^2}{x}$; that between P and p is $h+\frac{K^2}{x}$: hence by the theory of parallel forces* we have, for the component Q applied at C′, the value



$$Q=P\cdot\frac{K^2+hx}{K^2+x^2};$$

and for the other component p applied at O′, the value

$$p=P\cdot\frac{x^2-hx}{K^2+x^2}.$$

Now of these two components, the latter p which strikes at O′ can produce no percussion at the fixed point C′, since this point C′ is the spontaneous centre with respect to the point O′ where the force p is applied. The only force which remains to strike the obstacle C′, therefore, is the component Q, which is directly applied to that point; whence it follows that the percussion produced by the body against the fixed point C′, placed at the distance x from the centre of gravity, is exactly expressed by the function

$$Q=P\cdot\frac{K^2+hx}{K^2+x^2},$$

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* Poinsot's *Eléments de Statique,* art. 28.

P being the impulse with which the body is moved, and h the distance CG of this impulse from the centre of gravity.

16. We may remark, *en passant*, that if we suppose

$$x=-\frac{K^2}{h}=-a,$$

that is to say, if the obstacle C′ be presented at the spontaneous centre O, the percussion Q becomes zero, as it ought to do.

17. If we suppose $x=0$, that is, if we place the obstacle before the centre of gravity, the percussion Q is equal to the force P itself.

If we suppose $x=h$, in which case the obstacle C′ is applied at the point C, the percussion Q is again equal to P.

Thus with its centre of percussion the body does not strike more forcibly than with its centre of gravity. There is this difference between the two cases, however: when the body strikes with its centre of percussion, its whole motion is entirely arrested; whereas when it strikes with its centre of gravity, it loses its motion of translation merely and, after the impact, preserves its former rotation θ around the centre of gravity G.

18. It will also be seen that when x is negative and greater than $\frac{K^2}{h}$ or a,—in which case the obstacle C′ is placed on the same side of the centre of gravity as the spontaneous centre O, and beyond the same,—the percussion Q becomes negative or contrary in direction to the impulse P; so that, to be struck by the body at all, the obstacle C′ must then be presented on the other side, or in the rear of the body with respect to its translation in space.

But the question at present before us is, to find the distance x of the point T where the percussion Q is a *maximum*.

19. To find this remarkable point, we have merely to differentiate the preceding expression $P.\frac{K^2+hx}{K^2+x^2}$, x being thereby regarded as the sole variable, and to equate the resulting expression for $\frac{dQ}{dx}$ to zero: this gives us the quadratic equation

$$hx^2+2K^2x-hK^2=0;$$

or, since $\frac{K^2}{h}=a$,

$$x^2+2ax-K^2=0,$$

whence we deduce

$$x=-a\pm\sqrt{K^2+a^2},$$

the required distance of the point T from the centre of gravity G (see figure, art. 15).

If we wish to consider the distance λ between the point T and the spontaneous centre O, then since this distance is $x+a$, the above equation gives

$$\lambda = \sqrt{K^2+a^2} = \sqrt{al};$$

whence results the following theorem:—

"The distance λ of the centre of *maximum* percussion from the spontaneous centre O of rotation of the body is the geometrical mean between the distances of the centre of gravity, and of the ordinary centre of percussion from the same point O."

20. We may here observe that this theorem is susceptible of another enunciation. For since MK^2 represents the moment of inertia of the body with respect to the principal axis which passes through its centre of gravity, and since a represents the distance between the point O and this centre G, $M(K^2+a^2)$ will, as we know, be the moment of inertia of the body with respect to the parallel axis OI passing through the spontaneous centre O*. Hence K^2+a^2 is the mean of the squares of the distances of the several molecules of the body from this axis OI. Considering, therefore, the preceding expression

$$\lambda = \sqrt{K^2+a^2},$$

we may say that "if the body actually turns around a spontaneous axis, the distance from the same to the centre of *maximum* percussion is equal to the side of the square which is a mean between the squares of the distances of the several molecules of the body from the axis in question, or in simpler terms, is equal to the *arm*† of inertia with respect to this axis."

21. The double sign $\pm$ with which this distance λ is affected, indicates that there are two points of the same nature at equal distances to the right and to the left of the spontaneous centre O. The first T falls between the centre of gravity G and the centre of ordinary percussion C; the second T′ falls on the other side, beyond the point O.

The percussions to which both correspond are *maxima*, but *opposite in direction*. The first T is the centre of a maximum percussion greater than the impulse P, but having the same direction. The second T′ also corresponds to a *maximum* percussion; but this is always less than the impulse P, and opposed to it in direction.

Thus there is a point T with which the body strikes in front, not only more forcibly than with the centre of percussion C itself, but also more forcibly than with any other point; and at the same time there is a second centre T′ with which the body also

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* *Théorie nouvelle de la rotation des corps*, 1^{re} partie, art. 77.

† Ibid. 2^{de} partie, art. 2.

strikes with the greatest possible force, but it does so in the rear of its own translation through space.

We see, further, that these two centres T and T′ are mutually reciprocal. For the equation

$$x^2+2ax-K^2=0,$$

which gives their distances from the centre of gravity, shows that the product of these distances is equal to the last and constant term $-K^2$. Consequently, if one of these points were considered as an ordinary centre of percussion, the other would be the corresponding spontaneous centre of rotation.

Corollaries.

22. If we suppose $h=0$ in the foregoing expressions, we have the particular case of a body subject to the sole action of an impulse P passing through its centre of gravity. The percussion Q which the body is capable of giving at the distance x from this centre is then expressed by

$$Q=P\cdot\frac{K^2}{K^2+x^2};$$

and the point T, where this percussion is a maximum, is at the distance $x=0$, in other words, at the centre of gravity.

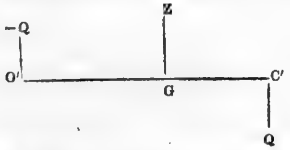
Thus when a body possesses only a translatory motion through space, the centre of *maximum* percussion is unique, and coincides with the centre of gravity,—a conclusion which is almost self-evident.

23. If we suppose $a=0$, the distance of the centre of *maximum* percussion will be

$$x=\pm K.$$

In this particular case the body is acted upon by a couple merely, and consequently has no other motion than that of a simple rotation around one of its principal axes. Since in this case the centres of rotation and gravity coincide, the centre of *maximum* percussion is no longer an absolutely determined point; its distance K from the centre of gravity is alone determined, so that it may be taken anywhere on the circumference of a circle described around the centre of gravity with the radius K.

This particular case may be treated directly thus: let N be



the moment of the couple tending to turn the body around the principal axis GZ, and conceive this couple changed to an equivalent one (Q, $-$Q) having, for its arm, the line $x+\frac{K^2}{x}$ joining any point C′ which we wish to consider with its reciprocal O′. The force with which the rotating body will strike at C′, *i. e.* at the distance x from G, will be

$$Q=\frac{N}{O'C'}=N\cdot\frac{x}{K^2+x^2};$$

whence it follows, as before, that

$$x=\pm K$$

corresponds to the *maximum* of Q.

Thus when a free body, whose moment of inertia with respect to one of its three principal axes is represented by MK^2, turns around this axis, it is at the distance K from the same, and in the plane of the other two axes that the rotating body will be able to strike an obstacle or a fixed point with the greatest possible force.

24. For example, if the body is a straight, homogeneous, prismatic bar whose length is 2L, we know that, with respect to an axis through the bar's centre of gravity and perpendicular to its length,

$$K=\frac{L}{\sqrt{3}};$$

hence when the bar rotates around this axis, the point with which it will strike with the greatest force is at the distance $\frac{L}{\sqrt{3}}$ from its centre of gravity.

For a homogeneous sphere with radius R, we have

$$K=R\sqrt{\frac{2}{5}},$$

and it is at this distance from its centre that the rotating sphere strikes most forcibly.

General Remark.

25. We have thus established the existence, in all bodies, of new centres enjoying very remarkable properties; centres whose positions, like that of the ordinary centre of percussion, depend neither upon the mass, nor upon the quantity of motion of the body, but solely upon the position of the point around which spontaneous rotation takes place: so that if the body turns on its own centre of gravity, the centres of percussion in question depend purely upon its form.

The difference between the old and the present theory will be at once remarked, as will also the accordance between the latter and the precise notion of percussion which we ought to possess. Take, for instance, the case of a body turning around its own centre of gravity; if we were to seek, according to the old theory, the particular point known as the *centre of percussion,* we should find it to be situated at an infinite distance from the centre of gravity, and that the force of percussion altogether vanishes. From this one might conclude that a rotating body, whose centre of gravity is at rest, is not capable of striking an obstacle or of communicating motion to any material point which is presented to it, a conclusion as opposed to experience as it is to our theory. In fact, we find that if a body turns around its own centre of gravity with an angular velocity θ, or, in other words, if the body is animated by a couple whose measure is $MK^2.\theta$, it is capable of striking at a distance x from its centre of gravity with a force

$$Q=MK^2\theta.\frac{x}{K^2+x^2},$$

that the *maximum* percussion takes place at the distance $x=K$ precisely, and that the intensity of the same is measured by the quantity of motion

$$\tfrac{1}{2}M.K\theta,$$

so that the body strikes with this centre just as if the half of its mass M were there concentrated.

In the same manner we find that the reciprocal point on the other side of G, at the distance $x=-K$, is also capable of striking in an opposite direction, as if the other half of the body's mass were there concentrated; hence we see that during the motion of the body its whole mass is, as it were, appropriated in equal shares by these two reciprocal centres. In the next article, however, we shall show that this last property is merely a particular case of a general one which all pairs of reciprocal points possess.

V. New properties of any two mutually *reciprocal* centres in a body.

26. It has been already shown (art. 15), that when animated by an impulse P passing at the distance h from its centre of gravity, the body strikes at C′, whose distance from G is x, with a force Q expressed by

$$Q=P.\frac{hx+K^2}{x^2+K^2};$$

and at the point O′, reciprocal to C′, with a force

$$p=P.\frac{x^2-hx}{x^2+K^2}.$$

If in these expressions we replace h by its value $\frac{K^2}{a}$, and P by its value $Ma\theta$, we shall have

$$Q=(a+x)\theta \, . \, M\frac{K^2}{x^2+K^2},$$

$$p=\left(a-\frac{K^2}{x}\right)\theta \, . \, M\frac{x^2}{x^2+K^2};$$

now $a+x$ being the distance of the point C′ from the spontaneous centre of rotation of the body, $(a+x)\theta$ is the actual velocity of this point C′; hence, since the percussion Q at this point is measured by the product of its velocity into a mass

$$m=M \, . \, \frac{K^2}{x^2+K^2},$$

we may say that the point C′ strikes with the same force as it would do were this fractional part of the whole mass M there concentrated.

Similarly, the factor $\left(a-\frac{K^2}{x}\right)\theta$ in the second expression being the velocity of the point O′, we see that this point, reciprocal to the first, strikes as if it were charged with the mass $n=M\frac{x^2}{x^2+K^2}$, in other words, with the same force as that with which it would strike if this other fractional part of the mass M were there concentrated.

But it is at once manifest from the above values of the masses m and n, *first*, that their sum $m+n$ constitutes the whole mass M of the body, and *secondly*, that they are to each other in the ratio

$$K^2 : x^2 = \frac{K^2}{x} : x,$$

that is to say, they are inversely proportional to the distances of the two points C′ and O′ from the centre of gravity G of the body.

Considering, therefore, any two centres, reciprocal to one another, and the respective percussions they are capable of producing, we may say that two such points strike exactly as if the whole mass of the body were divided between them in shares inversely proportional to their distances from its centre of gravity.

27. We may, consequently, conceive the body to be replaced by the right line C′O′, considered as an inflexible rod without mass, but loaded at its extremities with the two material points m and n in question. This rod would be gifted, not only at its two extremities, but also at every other point of its direction,

with the same capability of percussion as the body itself. For the rod would, as we have just seen, not only have the same mass and the same centre of gravity, but it would also have the same moment of inertia with respect to this centre G. In fact, the moment of inertia of the two massive points m and n is

$$mx^2+n\,.\,\frac{K^4}{x^2};$$

replacing, however, the first term mx^2 by its equal nK^2, and the factor nK^2 of the second term by its equal mx^2, this expression becomes

$$nK^2+mx^2\,.\,\frac{K^2}{x^2}=(m+n)K^2=MK^2;$$

that is to say, the moment of inertia in question is equal to that of the body.

Our ideal rod, therefore, under the influence of the same impulse P, would have the same spontaneous centre and the same angular velocity; it would consequently be capable, at each of its points, of the same force of percussion as is the body at each of the corresponding points.

COROLLARY.

ON THE CENTRE OF OSCILLATION OF A HEAVY BODY.

Hereby we may see why a heavy body suspended from a point O oscillates exactly as would a simple pendulum of the length OC equal to the distance from the point O to its *reciprocal* C in the body; for we may conceive the body to be replaced by an immaterial rod CO, loaded at its two ends with the portions m and n of the body's mass, and in virtue of the same applied force P, the movement of this rod will be exactly the same as that of the body itself. But since the force P is here equal to the weight of the body, its direction passes through the centre of gravity G, which is at the same time that of the massive points m and n; whence it follows that the force P may be resolved into two, p and q, applied at these points, and proportional to their respective masses m and n. If the point O becomes fixed, therefore, the movement of the point m is destroyed, and the point n at C remains abandoned to the simple force q which is its natural weight.

Remark.

28. Since, when the body moves, the point C′ taken at the distance x from its centre of gravity strikes with the same force as if the fraction $\frac{K^2}{x^2+K^2}$ of the mass M were there collected, and since $x=0$ is the only value of x which can render this

fraction equal to unity, it follows that the centre of gravity is the only point with respect to which it can be affirmed that the body strikes as if its whole mass were concentrated in the point of impact. The quantity of this percussion is $Ma\theta$.

29. If we consider the centre of percussion C, corresponding to $x=h$, we shall find for the force of percussion at this point, the expression

$$Q=(a+h)\theta\,.\,\frac{MK^2}{h^2+K^2}=l\theta\,.\,M\frac{a}{l}\,;$$

whence we see that this centre C strikes as would a free point moving with the same velocity $l\theta$, and charged with the fraction $\frac{a}{l}$ of the mass of the body. If we consider its quantity merely, this percussion is also $Ma\theta$, that is to say, it is the same as that which appertains to the centre of gravity; it differs from the latter, however, in being the percussion of a less mass moving with a greater velocity. If the obstacle is an absolutely fixed point, these two percussions may be regarded as perfectly equivalent, since in both cases the same quantity of motion is destroyed. But if the obstacle is a *free* massive point opposed to the movement of the body, the two percussions in question can no longer be regarded as identical. For it is evident that the body, by striking with its centre of percussion C, would impart to the massive point μ the velocity

$$v=\frac{Ma\theta}{\mu+M\frac{a}{l}}\,;$$

whereas by striking with its centre of gravity it would only impart to the point the velocity

$$v=\frac{Ma\theta}{\mu+M},$$

which is less than the foregoing in consequence of l being greater than a.

30. We see, then, that by striking with its centre of percussion, a body imparts more motion to a free point μ, previously at rest, than if it had struck the same with its centre of gravity. But the centre C of percussion is not on this account the point by means of which the body could communicate the greatest possible velocity to the point μ; neither does the centre T of *maximum* percussion possess this property, but another new point, whose distance from the centre of gravity depends upon the ratio between the masses M and μ. In fact let V be the velocity imparted to the massive point μ when the body M strikes the same with a point at the distance x from the centre of gra-

vity G; then, according to the ordinary laws of collision of bodies, a point having the mass $M\frac{K^2}{x^2+K^2}$, and moving with the velocity $(a+x)\theta$, will impart to a massive point μ, at rest, a velocity

$$V=\frac{M\frac{K^2}{x^2+K^2}(a+x)\theta}{M\frac{K^2}{x^2+K^2}+\mu}=\frac{MK^2(a+x)\theta}{(M+\mu)K^2+\mu x^2}.$$

And setting

$$\frac{dV}{dx}=0,$$

in order to find the point which corresponds to the *maximum* of communicated velocity V, we shall have

$$x^2+2ax-\left(1+\frac{M}{\mu}\right)K^2=0;$$

whence we deduce, for the distance of this point from the spontaneous centre of rotation, the value

$$x+a=\pm\sqrt{a^2+K^2\left(1+\frac{M}{\mu}\right)},$$

which evidently depends upon the ratio $\frac{M}{\mu}$. This centre of greatest communicated velocity, therefore, is a new point which coincides with the centre C of ordinary percussion only when

$$M:\mu=l:a,$$

and which cannot coincide with the centre T of *maximum* percussion unless

$$\frac{M}{\mu}=0,$$

which is equivalent to the supposition that the point μ has an infinitely great mass, or in other words, that it is a *fixed* point. It is scarcely necessary to remark, that this result accords perfectly with what has been above established as to the *maximum* of percussion which a body is capable of producing against a fixed obstacle.

In another place we shall consider these new questions relative to the percussion of a body against a free point; at present, however, we shall continue to suppose that the body strikes against a fixed point, or in other words, against a free point whose mass μ is supposed to be infinitely great.

VI. On certain new and remarkable centres in moving bodies.

31. Let us suppose that the body really comes into collision with an obstacle or fixed point C′ at the distance x from the centre of gravity G; the component Q will be destroyed, and the body will remain under the sole influence of the component p applied at O′. This force p will act upon the body just as if the latter were free; for the point C′, where the obstacle was presented, being the spontaneous centre with respect to the point O′, considered as a centre of percussion, it is evident that the obstacle cannot in any way alter the effect of the force p.

Thus the body which, before collision, was animated by the force P applied at the distance h from the centre of gravity G, will, after the shock, move under the influence of a new force,

$$p = \frac{Px^2 - Phx}{x^2 + K^2}$$

applied at the distance $-\frac{K^2}{x}$ from the same centre G.

The original velocity of the centre of gravity, which was

$$u = \frac{P}{M},$$

will therefore be changed to another u', where

$$u' = \frac{p}{M};$$

or replacing Ph by MK$^2\theta$,

$$u' = \frac{ux^2 - K^2\theta x}{x^2 + K^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and the original rotation, which was

$$\theta = \frac{Ph}{MK^2},$$

will be changed to

$$\theta' = -\frac{pK^2}{x\,.\,MK^2} = \frac{K^2\theta - ux}{x^2 + K^2}. \quad . \quad . \quad (2)$$

32. This being established, several simple and easily answered questions suggest themselves.

In the first place we may inquire at what distance x, that is to say, at what point C′ must the obstacle be presented in order that, after impact, the centre of gravity of the body may move in an opposite direction—in fact rebound—or may proceed onward with the greatest possible velocity or, if we please, with any given velocity whatever.

In the second place we may ask at what point the obstacle must be presented in order that, after the shock, the body may turn around its own centre of gravity in a direction either contrary to, or the same as its present rotation θ, and that with the greatest possible or, if we please, with a given angular velocity.

Here, then, are new and remarkable points in the body presented for consideration, points which it will be convenient to distinguish by particular names.

The first may be called centres of *reflexion* or *progression* according as the centre of gravity of the body is reflected, or continues its forward motion with the new velocity in question. The term *centres of reflexion,* however, may be applied to both, provided we admit that this reflexion may be positive or negative; *positive* when the body actually rebounds, and *negative* when it pursues its course with the new velocity it has acquired.

In a similar manner the second points may be called *centres of conversion;* this conversion being regarded as *negative* when the body, instead of changing the direction of its actual rotation θ, continues to turn on itself in the same direction as before.

ON THE CENTRE OF *MAXIMUM* REFLEXION.

33. In order, then, to find the point against which, if the body strikes, it will be reflected with the greatest possible velocity, we have merely to make $u' =$ a *maximum*, or $\frac{du'}{dx} = 0$. By so doing, the distance x of this centre will be found from the quadratic equation

$$\theta x^2 + 2ux - \theta K^2 = 0;$$

whence, replacing u by $a\theta$, we deduce

$$x = -a \pm \sqrt{a^2 + K^2} = -a \pm \lambda,$$

an expression identical with that found in art. 19 for the distance of the centre of *maximum* percussion.

Thus *the centre of greatest reflexion is the same as that of greatest percussion;* a result which might have been immediately seen, since the equation $\frac{du'}{dx} = 0$, which gives the value of x corresponding to the *maximum* of u', is the same as the equation $\frac{dp}{dx} = 0$; but the latter is itself the same as $\frac{dQ}{dx} = 0$, which corresponds to the *maximum* of Q.

34. Of the above two values of x, the first, $\lambda - a$, which is positive and less then h, gives for u' the value

$$u' = -\frac{(\lambda - a)\theta}{2},$$

which is negative, or in other words, has a sign unlike that of

$u=a\theta$: so that this first centre is that of real reflexion, where, after the shock, the body moves in a direction opposite to its actual motion u. On the contrary, the second root

$$x=-(\lambda+a),$$

which is negative, gives for u' the value

$$u''=\frac{(\lambda+a)\theta}{2},$$

which is positive, or of the same sign as u; so that the other centre of *maximum* reflexion is in reality a centre of *progression*, that is to say, a point by striking against which in the rear of its translatory motion, the body is precipitated forward with the greatest possible velocity.

35. When $\theta=0$, that is to say, when the body has merely a translatory motion through space, the above equation gives $x=0$; whence we see that the centre of *maximum* reflexion then coincides with the centre of gravity of the body; but $x=0$ renders the reflexion u' zero, so that in this case the movement of the body is destroyed, as it evidently should be.

36. When $u=0$, that is to say, when the body merely rotates on itself, we find

$$x=\pm\text{K} \text{ and } u'=\mp\frac{\text{K}\theta}{2}.$$

Thus *for a body turning on itself, the centre of* maximum *reflexion is at the distance* K *from the axis of rotation*: hence it is at this distance that the obstacle must be presented in order that the centre of gravity of the body, at present at rest, may be suddenly animated with the greatest possible velocity; this velocity—expressed by $\frac{\text{K}\theta}{2}$—will be the half of that which the striking point of the body possesses.

[To be continued.]

XXIII. *On Pseudomorphic Tremolite encrusted with Carbonate of Lime and Magnesia, being apparently the mineral described by* Dufrénoy *under the name of Miascite. By* Alphonse Gages, *Esq., Curator of the Museum of Irish Industry*[*].

IN the supplementary part of his *Traité de Minéralogie* (vol. iii. p. 770, edit. 1837), M. Dufrénoy has described, under the name of Miascite (Miaskite), two very distinct substances. The first is a grayish felspathic rock, composed chiefly of felspar, uniaxal mica, and elæolite; it was first described by G. Rose in the account of the journey of Humboldt, Ehrenberg and G. Rose,

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[*] Communicated by the Author.

to the Ural Mountains. The name of Miaskite was given to this rock from its occurrence in the hills in the neighbourhood of Miask in Siberia.

The second substance described under the name Miaskite was also derived from the locality just named, whence it was sent to Mr. Adam. M. Dufrénoy examined it and considered it to be dolomite.

In examining the collection of minerals in the Museum of Irish Industry, I found a specimen labelled Miaskite, and answering perfectly to the description given by M. Dufrénoy of the substance examined by him.

The remarkable structure of this mineral, formed, as it were, of a series of crystalline fibres arranged parallel to one another, may be compared to a bundle of flax or of thread completely encrusted with saline matter, the crystals of which have disposed themselves in the direction of the fibres. This peculiar structure gives it the aspect, at first sight, of fossil wood; but a close examination led me to suspect that it was the result of pseudomorphic action. Having removed a fragment of the external part of the specimen by splitting it in the direction of the fibres, I introduced it into very weak hydrochloric acid; the result obtained, after some days of contact with the acid, confirmed my supposition. The acid dissolved a quantity of lime and magnesia, and left an asbestos-like skeleton.

Having submitted another portion to analysis, the following numbers were obtained as the result:—

Carbonate of lime	57·483
Carbonate of magnesia	40·510
Sesquioxide of iron and alumina .	0·375
Water and organized matter . .	0·239
Asbestiform skeleton	1·595
	100·202

If we deduct the iron, water and skeleton, &c., and calculate the relative proportions of carbonate of lime and of carbonate of magnesia in 100 parts of the mixed carbonates, we obtain the following results:—

Carbonate of lime	58·660
Carbonate of magnesia	41·339
	99·999

True dolomite, or $CaO\,CO^2 + MgO\,CO^2$, would give the following composition in 100 parts:—

Carbonate of lime	54·201
Carbonate of magnesia	45·798
	99·999

The mineral analysed may therefore be considered as a mixture of dolomite and calcite in the following proportions:—

$CaO\,CO^2 + MgO\,CO^2$	90·262
$CaO\,CO^2$	9·738
	100·000

The insoluble skeleton, when dried, had the appearance of an asbestiform tremolite, and its analysis gave the following results:—

Silica	68·181
Magnesia	28·909
Alumina with traces of iron . .	2·181
	99·271

It is probable therefore that the skeleton was tremolite, from which water, impregnated with carbonic acid, had removed the whole of the lime. Thus if we deduct the per-centage of lime and calculate the remaining numbers in 100 parts, in the analysis of a specimen of tremolite from Wermland made by Bonsdorff, and compare the results with the preceding analysis of the skeleton, we shall get the following numbers:—

	Tremolite from Wermland analysed by Bonsdorff.	Tremolite, supposing the whole of the lime removed.	Asbestiform skeleton.
Silica	59·75	69·565	68·181
Lime	14·11		
Magnesia	25·00	29·107	28·909
Protoxide of iron . .	0·50	0·582	
Fluorine	0·94	1·094	
Water	0·10	0·116	
	100·40	100·464	

The water which exists in the mineral evidently belongs to the skeleton; but as its quantity could not be absolutely determined, owing to the presence of organic matter, I have not attempted to calculate a formula for the asbestiform skeleton.

Another explanation of the origin of the skeleton suggests itself, namely that the mineral was not hornblendic, but augitic; for example, like the asbestiform diopside from Zillerthal, examined by Meitzendorff. When augitic minerals are acted upon by water containing carbonic acid in solution, the lime is removed and nearly the whole of the magnesia is left behind, of which the Rensselærite of Beck is an example. Numerous other examples of this kind have been given by Beudant, Svanberg, &c.

Whatever may have been the original mineral, it must have been considerably modified before the encrustations began. The proportions which the skeleton bears to the whole mass of the mineral in its present form is so small, that some of the original fibrous mineral must have been wholly removed before the remainder began to be encrustated.

The organic matter noted in the analysis appeared to have been derived from infiltrated water, and followed the direction of the fibres. When a fragment of the mineral was heated in a small glass tube, the junction of the fibres was well marked by black lines from the charred matter.

The peculiar character of the pseudomorph, especially if we assume that it was tremolite, which is so frequently found in calcareous rocks, suggests the idea that many of the fibrous varieties of dolomite may have been formed in a similar way. It would be worth while to examine some specimens of these dolomites from this point of view.

XXIV. *On the Formation of Indigo-blue.*—Part II.

By Edward Schunck, *Ph.D., F.R.S.*

[Concluded from p. 133.]

Action of Alkalies on Indican.

In the first part of this paper I have described in general terms the effect produced on indican by alkalies. I shall now proceed to give a more detailed account of this process of decomposition, and of the products to which it gives rise.

When a watery solution of indican is mixed with caustic soda it turns of a dark yellow colour, but no further apparent change takes place. If, however, after the mixture has been left to stand for several days, a portion of it be mixed with an excess of sulphuric acid and boiled, it deposits dark flocks, which after being collected on a filter and washed, are found to contain no indigo-blue, and to be entirely soluble in boiling alcohol. The alcoholic solution has a fine purple colour, and gives only a slight precipitate with acetate of lead. Hence it follows, that by the action of the alkali indican is converted into a body which by decomposition with acids yields indirubine. This body may be prepared in the following manner. A watery solution of indican having been mixed with baryta-water is left to stand until a portion of it, on being boiled with an excess of muriatic acid, no longer yields indigo-blue, but only indirubine. The baryta is then precipitated with sulphuric acid, the excess of the latter is removed by means of carbonate of lead, the liquid is filtered, and after sulphuretted hydrogen has been passed through it, it is filtered again from the precipitated sulphuret of lead and then evaporated by means of a current of air in the apparatus above described. The dark yellow syrup left after evaporation is treated with alcohol, in which a great part dissolves, and the alcoholic solution is then mixed with twice its volume of æther, which causes a milkiness and produces a syrupy deposit consisting chiefly of indiglucine. The liquid, after it has become clear, is evaporated spontaneously, when it leaves a yellow transparent glutinous residue, having a bitter taste, which cannot be distinguished in outward appearance from indican itself. This residue, when dissolved in water and treated with acid, still gives indirubine in a state of tolerable purity.

On attempting, however, to prepare this substance on a somewhat larger scale, I found it difficult to arrest the process at this stage. As soon as the solution ceased to give indigo-blue with acids, it began to yield with acids a mixture of indirubine and indiretine, and at length it gave indiretine only, after which no further change took place. By allowing a watery solution of indican mixed with baryta-water to stand until the decomposition had arrived at its last stage, and then treating the solution in the way just described, a substance resembling the preceding was obtained in the form of a brown syrup, to which I propose to give the name of

Indicanine.

This substance has the following properties. Its taste is bitter like that of indican. When heated on platinum it swells up very much and burns, leaving a bulky carbonaceous residue. When heated in a tube it gives fumes, condensing to a brown liquid, which after some time becomes filled with a quantity of white crystalline needles. It is perfectly soluble in alcohol and æther. The alcoholic solution gives with an alcoholic solution of acetate of lead a bright sulphur-yellow precipitate, which dissolves when more acetate of lead is added and the liquid is boiled, forming a yellow solution, in which ammonia again produces a yellow precipitate like the first. The watery solution gives only a slight precipitate with acetate of lead, but the filtered liquid yields a copious yellow precipitate on the addition of ammonia. When the watery solution is mixed with sulphuric acid and boiled, it slowly deposits a quantity of brown resinous particles, which are entirely soluble in caustic soda, and consist of indiretine and a little indifuscine. On adding caustic soda to a watery solution of indicanine it becomes dark yellow, and on being boiled disengages ammonia, but exhibits no further change. The analysis of the lead compound, prepared by adding acetate of lead to the alcoholic solution, filtering and washing with alcohol, yielded the following results:—

0·7840 grm., dried first *in vacuo* and then at 100° C., burnt with oxide of copper and chlorate of potash, gave 0·6115 grm. carbonic acid and 0·1480 water.

1·0350 grm. gave 0·2225 grm. chloride of platinum and ammonium.

0·3785 grm. gave 0·3010 grm. sulphate of lead.

Hence was deduced the following composition :—

	Eqs.		Calculated.	Found.
Carbon	40	240	21·06	21·27
Hydrogen . .	23	23	2·01	2·09
Nitrogen . .	1	14	1·22	1·35
Oxygen . . .	24	192	16·88	16·78
Oxide of lead .	6	670·2	58·83	58·51
		1139·2	100·00	100·00

After deducting the oxide of lead, the amount of the other constituents in 100 parts, as compared with the calculated composition, is as follows :—

	Eqs.		Calculated.	Found.
Carbon	40	240	51·17	51·26
Hydrogen . .	23	23	4·90	5·03
Nitrogen. . .	1	14	2·98	3·25
Oxygen . . .	24	192	40·95	40·46
		469	100·00	100·00

In the first part of this paper I gave an analysis of the lead compound of a substance having the formula $C^{40} H^{26} NO^{27}$, which differed therefore in composition from this merely by containing the elements of 3 equivalents more of water. As it was impossible to analyse these substances in an uncombined state, there were no means of ascertaining whether in that state they had the same composition, as was most probably the case.

Indicanine is formed from indican simply by the latter taking up water and losing 1 equivalent of indiglucine, as will be seen from the following equation :—

$$\left.\begin{array}{ll}\text{1 eq. Indican} & C^{52} H^{31} NO^{34} \\ \text{2 eqs. Water} & \underline{\quad H^{2} \quad O^{2}} \\ & C^{52} H^{33} NO^{36}\end{array}\right\} = \left\{\begin{array}{ll}C^{40} H^{23} NO^{24} & \text{1 eq. Indicanine.} \\ \underline{C^{12} H^{10} \quad O^{12}} & \text{1 eq. Indiglucine.} \\ C^{52} H^{33} NO^{36} & \end{array}\right.$$

The indiglucine formed in the process is contained in the brown syrupy deposit which falls on adding æther to the alcoholic solution of the indicanine. Some of this deposit, after the liquid had been poured off, was dissolved again in alcohol; the solution was mixed with an excess of alcoholic solution of acetate of lead, which produced a brown glutinous precipitate, and to the filtered liquid was added an excess of ammonia, which gave a bulky sulphur-yellow precipitate. This precipitate was collected on a filter, washed with water, and decomposed with sulphuretted hydrogen, and the filtered liquid was agitated with animal charcoal until it had lost the yellowish tint which it possessed at first. The liquid having been again filtered was mixed with acetate of lead and ammonia, which produced a milk-white precipitate. This precipitate, after being filtered off, was redissolved in a mixture of alcohol and acetic acid, and by the addition of a small quantity of ammonia a white precipitate was again produced, which was filtered off and washed with alcohol.

1·0230 grm. of this precipitate, dried *in vacuo*, gave 0·4780 grm. carbonic acid and 0·1510 water.

0·6095 grm. gave 0·5780 grm. sulphate of lead.

In 100 parts it contained, therefore,—

Carbon	12·74
Hydrogen.	1·64
Oxygen	15·85
Oxide of lead	69·77
	100·00

If the oxide of lead, the amount of which stands in no simple relation to that of the other constituents, be deducted, the composition of the body combined with it will be represented by the formula $C^{12} H^{9} O^{11}$, which is that of anhydrous indiglucine, as will be seen from the following calculation:—

	Eqs.		Calculated.	Found.
Carbon . . .	12	72	42·60	42·14
Hydrogen. . .	9	9	5·32	5·42
Oxygen . . .	11	88	52·08	52·44
		169	100·00	100·00

The manner in which indiretine and indifuscine are formed from indicanine needs no explanation, since the composition of the latter differs from that of indican merely by the elements of 1 equivalent of indiglucine. It is, however, difficult to explain why indicanine, by decomposition with acids, should yield only these products, and no indigo-blue, indirubine or indifulvine, which might, as far as their composition is concerned, be produced at the same time, and I am quite unable to assign any cause for this phænomenon. It seems to me very probable that the indiretine and indifuscine which are formed when pure indican in large quantities is decomposed with acids, owe their origin to the conversion of a portion of the indican into indicanine before the acid has had time to effect the more complete decomposition of this portion into indigo-blue, or indirubine and indiglucine.

Indican is decomposed, when its watery solution is heated for a length of time, in exactly the same manner as by means of alkalies. After the solution has been heated for some time, it no longer gives any indigo-blue when a portion of it is boiled with sulphuric acid. If it be now evaporated in the same apparatus as that used for evaporation of solutions of indican, it leaves a brown syrup, a great part of which dissolves in alcohol. On adding æther to the alcoholic solution, a syrupy deposit of indiglucine is produced, followed by the separation of crystals of leucine. If the liquid be filtered and evaporated, it leaves a brown glutinous residue, having the properties of indicanine. The lead compound, which was obtained in the form of a sulphur-yellow precipitate by adding acetate of lead to the alcoholic solution, was, after being filtered off and washed with alcohol, submitted to analysis, when it gave the following results:—

1·2580 grm., dried first *in vacuo* and then in the water-bath, gave 0·8320 grm. carbonic acid and 0·2040 water.

1·5655 grm. gave 0·2740 grm. chloride of platinum and ammonium.

0·7620 grm. gave 0·6610 grm. sulphate of lead.

These numbers correspond in 100 parts to—

Carbon	18·03
Hydrogen	1·80
Nitrogen	1·09
Oxygen	15·26
Oxide of lead	63·82
	100·00

The oxide of lead being deducted, the substance combined with it was found to have a composition agreeing with the formula $C^{40} H^{24} NO^{25}$, as will be seen by a comparison of the calculated composition with that found by experiment:—

	Eqs.		Calculated.	Found.
Carbon	40	240	50·20	49·85
Hydrogen	24	24	5·02	4·98
Nitrogen	1	14	2·92	3·01
Oxygen	25	200	41·86	42·16
		478	100·00	100·00

When a watery solution of indican or indicanine is evaporated in contact with the air, either spontaneously or with the assistance of heat, a portion of it is always converted into a substance which is insoluble not only in æther but also in alcohol. That the formation of this substance is due to the action of oxygen on indicanine is proved by analysis. Its formation, moreover, is promoted by heating the solution of indicanine with peroxide of lead, the filtered liquid, after the dissolved lead has been removed with sulphuretted hydrogen, leaving on evaporation a residue which is insoluble in alcohol. It differs, however, in composition, according as the solution of indican has been evaporated spontaneously or with the assistance of heat. The body which is formed when a watery solution of indican is spontaneously evaporated in contact with the air, I propose to call—

Oxindicanine.

So much of this body is produced during the preparation of indican, that I found it unnecessary to prepare it purposely. When the residue left after the evaporation of the watery solution of indican by means of a current of air, as described above, is treated with cold alcohol, the greatest part of the oxindicanine formed during the process remains undissolved. It may be purified simply by dissolving it in a little water and precipitating again with a large quantity of alcohol. Its appearance is that of a brown glutinous substance, which on being left to stand over sulphuric acid becomes almost dry, and assumes the appearance of gum. It is insoluble in absolute alcohol, and only slightly soluble in dilute alcohol. When heated on platinum it swells up very much and burns, leaving a considerable carbonaceous residue. It yields, when heated in a tube, strong-smelling fumes, but only a slight trace of crystalline sublimate. Its taste is nauseous, but not bitter. Its watery solution gives with acetate of lead a copious, dirty-yellow precipitate, and the filtered liquid gives a pale, primrose-yellow precipitate on the addition of ammonia, or of a large excess of alcohol. When the watery solution is mixed with sulphuric acid and boiled, it slowly deposits brown flocks, which have the properties of indifuscine, while the liquid contains indiglucine. For the purpose of determining its composition I employed the lead compound, prepared by adding acetate of lead to the watery solution, filtering, and washing with water.

I. 0·8515 grm. of this precipitate, dried first *in vacuo* and then at 100° C., gave 0·7310 grm. carbonic acid and 0·1735 water.

1·1640 grm. gave 0·2575 grm. chloride of platinum and ammonium.

0·5185 grm. gave 0·3460 grm. sulphate of lead.

II. 1·2640 grm. of another preparation gave 1·0495 grm. carbonic acid and 0·2430 water.

1·5500 grm. gave 0·2505 grm. chloride of platinum and ammonium.

0·7730 grm. gave 0·5250 grm. sulphate of lead.

Hence the composition in 100 parts was as follows:—

	I.	II.
Carbon	23·41	22·64
Hydrogen	2·26	2·13
Nitrogen	1·38	1·01
Oxygen	23·85	24·25
Oxide of lead	49·10	49·97
	100·00	100·00

After deducting the oxide of lead, the first analysis gives a composition agreeing with the formula $C^{40}H^{22}NO^{31}$, whereas the second leads to the formula $C^{40}H^{23}NO^{32}$, as is shown by a comparison of the calculated numbers with those deduced from the above analyses.

	Eqs.		Calculated.	I.	Eqs.		Calculated.	II.
Carbon	40	240	45·80	45·99	40	240	45·02	45·25
Hydrogen	22	22	4·19	4·44	23	23	4·31	4·25
Nitrogen	1	14	2·67	2·71	1	14	2·62	2·01
Oxygen	31	248	47·34	46·86	32	256	48·05	48·49
		524	100·00	100·00		533	100·00	100·00

If the second formula be adopted as the correct one, it follows that indicanine is simply converted into oxindicanine by taking up 8 equivalents of oxygen. The formation of indifuscine from oxindicanine takes place in consequence of the separation from the latter of 1 equivalent of indiglucine, 4 equivalents of carbonic acid, and 3 equivalents of water, in accordance with the following equation:—

$$1\text{ eq. Oxindicanine } C^{40}H^{23}NO^{32} = \left\{\begin{array}{lllll} C^{24} & H^{10} & NO^{9} & 1\text{ eq. Indifuscine.} \\ C^{12} & H^{10} & O^{12} & 1\text{ eq. Indiglucine.} \\ C^{4} & & O^{8} & 4\text{ eqs. Carbonic acid.} \\ & H^{3} & O^{3} & 3\text{ eqs. Water.} \end{array}\right.$$

$$\overline{C^{40}H^{23}NO^{32}}$$

The acetic acid which is produced when indifuscine is formed from indicanine does not make its appearance in this case. Indeed the 8 equivalents of oxygen which indicanine absorbs in its conversion into oxindicanine are just sufficient, when added to the oxygen already contained in 1 equivalent of acetic acid, to convert the carbon and hydrogen of the latter into carbonic acid and water.

When a solution of indican is evaporated in contact with the air with the assistance of heat, and the residue which remains is treated with strong alcohol, there is left undissolved a brown glutinous substance, which has the properties of oxindicanine, but a different composition. The lead compound of this substance was prepared by dissolving the latter in water, adding acetate of lead, decomposing the precipitate with sulphuretted hydrogen, adding a little acetate of lead to the filtered liquid, filtering again, and precipitating completely with sugar of lead. The precipitate, which was of a dirty-yellow colour, was filtered off, and washed first with water and then with alcohol.

I. 1·3345 grm., dried first *in vacuo* and then at 100° C., gave 0·9875 grm. carbonic acid and 0·2370 water.

1·5825 grm. gave 0·4305 grm. chloride of platinum and ammonium.

0·8960 grm. gave 0·6630 grm. sulphate of lead.

II. 1·3675 grm. of another preparation gave 1·0485 grm. carbonic acid and 0·2470 water.

1·5955 grm. gave 0·4325 grm. chloride of platinum and ammonium.

0·8895 grm. gave 0·6485 grm. sulphate of lead.

These numbers lead to the following composition :—

	Eqs.		Calculated.	I.	II.
Carbon . .	28	168	20·27	20·18	20·91
Hydrogen .	16	16	1·93	1·97	2·00
Nitrogen. .	1	14	1·68	1·70	1·70
Oxygen . .	23	184	22·22	21·71	22·35
Oxide of lead.	4	446·8	53·90	54·44	53·04
		828·8	100·00	100·00	100·00

The following Table shows the composition of the substance, after deducting the oxide of lead, as compared with that required by theory :—

	Eqs.		Calculated.	I.	II.
Carbon . .	28	168	43·97	44·29	44·52
Hydrogen. .	16	16	4·18	4·32	4·25
Nitrogen . .	1	14	3·66	3·73	3·62
Oxygen . .	23	184	48·19	47·66	47·61
		382	100·00	100·00	100·00

This body may for the sake of distinction be called *Oxindicasine.* It is formed from oxindicanine by the latter taking up water and losing 1 equivalent of indiglucine, since—

$$\left.\begin{array}{ll}\text{1 eq. Oxindicanine} & C^{40}H^{23}NO^{32}\\ \text{3 eqs. Water} & \quad\;\; H^{3}\quad O^{3}\end{array}\right\} = \left\{\begin{array}{ll} C^{28}H^{16}NO^{23} & \text{1 eq. Oxindicasine.}\\ C^{12}H^{10}\quad O^{12} & \text{1 eq. Indiglucine.}\end{array}\right.$$

$$C^{40}H^{26}NO^{35} \qquad\qquad C^{40}H^{26}NO^{35}$$

It is possible that there may exist a body which bears to oxindicasine the same relation that indicanine does to oxindicanine. This body would be *Indicasine,* and would differ from indicanine by containing the element of 1 equivalent of indiglucine less. The following analyses of a lead compound, which was obtained as a pale yellow precipitate when a large quantity of alcohol was added to the liquid filtered from the lead compound of oxindicasine, seem to countenance the idea that such a body really exists.

I. 0·9705 grm. of this precipitate, after being completely washed with alcohol and then dried, at first *in vacuo* and then at 100° C., gave 0·5740 grm. carbonic acid and 0·1645 water.

1·2145 grm. gave 0·1210 grm. platinum.

0·6325 grm. gave 0·5420 grm. sulphate of lead.

II. 1·3280 grm. of another preparation gave 0·7795 grm. carbonic acid and 0·2145 water.

1·5600 grm. gave 0·1435 grm. platinum.

0·8965 grm. gave 0·7715 grm. sulphate of lead.

Hence was deduced the following composition:—

	Eqs.		Calculated.	I.	II.
Carbon . .	28	168	15·90	16·13	16·00
Hydrogen .	20	20	1·89	1·88	1·79
Nitrogen .	1	14	1·32	1·41	1·30
Oxygen . .	23	184	17·44	17·53	17·59
Oxide of lead	6	670·2	63·45	63·05	63·32
		1056·2	100·00	100·00	100·00

After deducting the oxide of lead, the composition in 100 parts, as compared with the theoretical composition, is as follows:—

	Eqs.		Calculated.	I.	II.
Carbon . .	28	168	43·52	43·65	43·62
Hydrogen .	20	20	5·18	5·08	4·88
Nitrogen .	1	14	3·62	3·81	3·54
Oxygen . .	23	184	47·68	47·46	47·96
		386	100·00	100·00	100·00

Now if the formula of this substance be doubled, and the formula of oxindicasine be deducted, the remainder will be the formula $C^{28} H^{24} NO^{23}$, since

$$C^{56} H^{40} N^2 O^{46} = C^{28} H^{16} NO^{23} + C^{28} H^{24} NO^{23}.$$

The body represented by the last formula and 1 equivalent of indiglucine contain together the elements of indicanine and water, for

$$C^{28} H^{24} NO^{23} + C^{12} H^{10} O^{12} = C^{40} H^{23} NO^{24} + 11HO.$$

It has therefore the composition which theory would assign to indicasine, and the substance represented by the formula $C^{28} H^{20} NO^{23}$ is probably a mixture in equal proportions of indicasine and oxindicasine. By decomposition of the lead compound with sulphuretted hydrogen and evaporation of the filtered liquid, this substance is obtained in the form of a brown syrup, which cannot be distinguished in appearance or properties from oxindicanine or oxindicasine.

I did not enter into a more minute examination of these bodies, since their formation from indican is the only point of interest in their history.

XXV. *On the direct Magnetic Influence of a distant Luminary upon the Diurnal Variations of the Magnetic Force at the Earth's Surface.* *By the* Rev. H. Lloyd, *D.D., D.C.L.* &c.*

IT has been usual to ascribe the ordinary diurnal variations of the terrestrial magnetic force to solar heat, either operating directly upon the magnetism of the earth, or generating thermo-electric currents in its crust. The credit of these hypotheses has been somewhat weakened by the discovery of a variation which is certainly independent of any such cause, namely, the lunar variation of the three magnetic elements; while at the same time new laws of the solar diurnal change have been established, which are deemed to be incompatible with the supposition of a thermic agency. There has been, accordingly, a tendency of late to recur to the hypothesis that the sun and moon are themselves endued with magnetism, whether inherent or induced; and it is therefore of some importance to determine the effects which such bodies would produce at the earth's surface, and to compare them with those actually observed.

I have endeavoured, in what follows, to solve this question, on the assumption that the supposed magnetism of these luminaries is inherent. The result will show the insufficiency of the hypothesis to explain the phænomena; and will therefore bring us one step nearer to their explanation, by the removal of one of their supposed causes.

Let x, y, z be the coordinates of any point of a fixed magnet, referred to three rectangular axes passing through its middle point; a, b, c those of a distant magnetic element m; and e their mutual distance. Then, if μ denote the quantity of free magnetism contained in the element ds of the magnet at the point (x, y, z), the force exerted by μ on m is

$$\frac{m\mu ds}{e^2};$$

and its resolved portions in the directions of the three axes of coordinates are

$$\frac{m(a-x)\mu ds}{e^3}, \quad \frac{m(b-y)\mu ds}{e^3}; \quad \frac{m(c-z)\mu ds}{e^3}.$$

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* Communicated by the Author.

Let the magnitudes of the lines connecting the points (a, b, c) and (x, y, z), respectively, with the origin be denoted by u and s, and let the angle contained by their directions be ω. Then

$$e^2 = u^2 - 2us\cos\omega + s^2 ;$$

and if s be so small in comparison with u that the squares and higher powers of $\frac{u}{s}$ may be neglected,

$$e^{-3} = u^{-3}\left(1 + \frac{3s}{u}\cos\omega\right).$$

Again, if α, β, γ denote the angles contained by the axis of the magnet with the three axes of coordinates,

$$x = s\cos\alpha, \quad y = s\cos\beta, \quad z = s\cos\gamma.$$

Substituting these values in the expressions for the components of the force above given, integrating, and observing that $\int \mu ds = 0$, we have, for the components of the total force exerted by the magnet on the magnetic element,

$$\frac{\mathrm{M}m}{u^3}\left(3\frac{a}{u}\cos\omega - \cos\alpha\right),$$

$$\frac{\mathrm{M}m}{u^3}\left(3\frac{b}{u}\cos\omega - \cos\beta\right),$$

$$\frac{\mathrm{M}m}{u^3}\left(3\frac{c}{u}\cos\omega - \cos\gamma\right);$$

in which we have put, for abridgement, $\mathrm{M} = \int \mu s ds$. The angle ω is connected with α, β, γ by the relation

$$u\cos\omega = a\cos\alpha + b\cos\beta + c\cos\gamma.$$

Now let the point (a, b, c) be on the earth's surface, and let us suppose, for simplicity, that the acting magnet is in the plane of the equator. Let that plane be taken as the plane of (x, y), and the line connecting the centre of the magnet and that of the earth as the axis of x. Then, if the distance of the acting magnet be considerable, relatively to the earth's radius, b and c are small in comparison with a, and we may neglect the small quantities of the second order, $\frac{b^2}{a^2}, \frac{c^2}{a^2}, \frac{bc}{a^2}$. Wherefore, substituting for $\cos\omega$ its value, the components of the acting force become

$$\frac{\mathrm{M}m}{a^3}\left(2\cos\alpha + \frac{3b}{a}\cos\beta + \frac{3c}{a}\cos\gamma\right),$$

$$\frac{\mathrm{M}m}{a^3}\left(-\cos\beta + \frac{3b}{a}\cos\alpha\right),$$

$$\frac{\mathrm{M}m}{a^3}\left(-\cos\gamma + \frac{3c}{a}\cos\alpha\right).$$

Now, if D denote the distance of the centre of the magnet from the centre of the earth, r the earth's radius, λ the latitude of the point (a, b, c) on its surface, and θ the angle contained by the meridian passing through it with that containing the acting magnet,

$$a = \mathrm{D} - r\cos\lambda\cos\theta, \quad b = r\cos\lambda\sin\theta, \quad c = r\sin\lambda.$$

Hence the maximum values of $\frac{b}{a}, \frac{c}{a}$ are equal to $\frac{r}{a}$; and if we disregard the terms containing them in comparison with the rest, the preceding values are reduced to

$$2\frac{\mathrm{M}m}{\mathrm{D}^3}\cos\alpha, \quad -\frac{\mathrm{M}m}{\mathrm{D}^3}\cos\beta, \quad -\frac{\mathrm{M}m}{\mathrm{D}^3}\cos\gamma.$$

Now, in place of a single magnet, let there be an indefinite number distributed in any manner throughout the entire magnetic body; and let us make, for abridgement,

$$\Sigma(\mathrm{M}\cos\alpha) = \mathrm{P}, \quad \Sigma(\mathrm{M}\cos\beta) = \mathrm{Q}, \quad \Sigma(\mathrm{M}\cos\gamma) = \mathrm{R}.$$

Then, if the radius of this body be small in comparison with its distance, we may neglect the variations of D, and we shall have for the three components of the acting forces,

$$\mathrm{X} = \frac{2m\mathrm{P}}{\mathrm{D}^3}, \quad \mathrm{Y} = \frac{-m\mathrm{Q}}{\mathrm{D}^3}, \quad \mathrm{Z} = \frac{-m\mathrm{R}}{\mathrm{D}^3}.$$

In order to determine the effect of these forces upon a freely suspended horizontal magnet at the earth's surface, we must resolve X and Y in the direction of the tangent and of the radius of the parallel of latitude. The resolved forces are, respectively,

$$\mathrm{X}\sin\theta + \mathrm{Y}\cos\theta, \quad \mathrm{X}\cos\theta - \mathrm{Y}\sin\theta.$$

Again, resolving the forces Z and $\mathrm{X}\cos\theta - \mathrm{Y}\sin\theta$ in the direction of the tangent to the meridian, and in the direction of the radius of the earth, we have finally the three components, viz.

$\mathrm{X}\sin\theta + \mathrm{Y}\cos\theta$, horizontal, and directed eastward;
$\mathrm{Z}\cos\lambda + (\mathrm{X}\cos\theta - \mathrm{Y}\sin\theta)\sin\lambda$, horizontal, and directed northward;
$-\mathrm{Z}\sin\lambda + (\mathrm{X}\cos\theta - \mathrm{Y}\sin\theta)\cos\lambda$, vertical, towards centre.

Of these, the latter has no effect upon the horizontal magnet; the moment of the two former to turn it is

$$(\mathrm{X}\sin\theta + \mathrm{Y}\cos\theta)\cos\delta - \{\mathrm{Z}\cos\lambda + (\mathrm{X}\cos\theta - \mathrm{Y}\sin\theta)\sin\lambda\}\sin\delta,$$

δ denoting the magnetic declination; or, substituting for X, Y, Z their values,

$$\frac{m}{\mathrm{D}^3}\Big\{\cos\delta(2\mathrm{P}\sin\theta - \mathrm{Q}\cos\theta) - \sin\delta(2\mathrm{P}\cos\theta + \mathrm{Q}\sin\theta)\sin\lambda$$
$$-\mathrm{R}\cos\lambda\sin\delta\Big\}.$$

But the moment of the earth's magnetism, opposed to this, is

$$U\Delta\delta \sin 1',$$

in which U denotes the horizontal component of the earth's magnetic force. Wherefore

$$\Delta\delta = \frac{1}{D^3 U \sin 1'}\Big\{\sin\theta(2P\cos\delta - Q\sin\lambda\sin\delta) - \cos\theta(2P\sin\lambda\sin\delta + Q\cos\delta) + R\cos\lambda\sin\delta\Big\}.$$

At the equator, this is reduced to

$$\Delta\delta = \frac{1}{D^3 U \sin 1'}\Big\{\cos\delta(2P\sin\theta - Q\cos\theta) + R\sin\delta\Big\}.$$

To determine the effect of the magnetic body upon the horizontal component of the earth's magnetic force, we must resolve the horizontal parts of the disturbing forces, viz. $X\sin\theta + Y\cos\theta$, acting eastward, and $Z\cos\lambda + (X\cos\theta - Y\sin\theta)\sin\lambda$, acting northward, in the direction of the magnetic meridian. We have thus

$$\Delta U = (X\sin\theta + Y\cos\theta)\sin\delta + \{Z\cos\lambda + (X\cos\theta - Y\sin\theta)\sin\lambda\}\cos\delta$$

$$= \frac{1}{D^3}\Big\{\sin\theta(2P\sin\delta + Q\sin\lambda\cos\delta) + \cos\theta(2P\sin\lambda\cos\delta - Q\sin\delta) - R\cos\lambda\cos\delta\Big\};$$

and at the equator,

$$\Delta U = \frac{1}{D^3}\Big\{\sin\delta(2P\sin\theta - Q\cos\theta) - R\cos\delta\Big\}.$$

Lastly, if V denote the vertical component of the earth's force, we have

$$\Delta V = -Z\sin\lambda + (X\cos\theta - Y\sin\theta)\cos\lambda$$

$$= \frac{1}{D^3}\Big\{(2P\cos\theta + Q\sin\theta)\cos\lambda + R\sin\lambda\Big\};$$

a result which, as might have been anticipated, is independent of the magnetic declination. At the equator,

$$\Delta V = \frac{1}{D^3}(2P\cos\theta + Q\sin\theta).$$

From the foregoing we learn:—

1. That the effect of a distant magnetic body on each of the three elements of the earth's magnetic force consists of two parts, one of which is *constant* throughout the day, while the other varies with the *hour-angle of the luminary*.

2. Each of these parts varies inversely as the cube of the distance of the magnetic body.

3. The variable part will give rise to a *diurnal inequality*, having one maximum and one minimum in the day, and subject to the condition

$$\Delta_\theta + \Delta_{\pi+\theta} = 0.$$

The third of these laws does not hold, with respect either to the solar- or to the lunar-diurnal variation. Thus, in the solar-diurnal variation of the declination, the changes of position of the magnet throughout the night are comparatively small, and do not correspond, with change of sign only (as required by the foregoing law), to those which take place at the *homonymous* hours of the day. The phænomena of the lunar-diurnal variation are even more opposed to the foregoing law, the variation having two maxima and two minima of nearly equal magnitude in the twenty-four lunar hours, and its values at homonymous hours having for the most part the same sign. Hence the phænomena of the diurnal variation are *not* caused by the *direct magnetic action* of the sun and moon.

It is true that if we proceed another step in the approximation, and include in the values of the disturbing forces the terms containing the first powers of $\frac{b}{a}$, $\frac{c}{a}$, the former will produce in the resulting values of $\Delta\delta$, ΔU, and ΔV, terms containing $\sin 2\theta$, $\cos 2\theta$, and giving rise therefore to a *semidiurnal* inequality. But the coefficient $\frac{3r}{D}$, by which these terms are multiplied, amounts in the case of the sun only to $\frac{1}{8000}$, while in that of the moon it is about $\frac{1}{20}$; and the magnitude of the semidiurnal inequality should bear to that of the diurnal the ratios designated by these small fractions. The facts are altogether opposed to this result. The coefficient of the solar diurnal inequality of the declination at Dublin, in the mean of the entire year, is $3'\cdot52$, while that of the semidiurnal is $2'\cdot13$,—nearly two-thirds of the former. In the case of the lunar-diurnal variation, the semidiurnal inequality is *greater* than the diurnal.

XXVI. *On the Action of Nitric Acid on Glycerine.—First Memoir.* *By* Dr. H. Debus[*].

THE constitution of glycerine may be considered, according to the excellent investigations of Berthelot, Luca and Wurtz, to be analogous to that of alcohol. If the atomic group C^3H^5[†] replaces three atoms of hydrogen in three atoms of water, a formula is obtained which expresses the composition of

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[*] Communicated by the Author.

[†] C=12, H=1, O=16.

glycerine. The propylic alcohol stands to glycerine most probably in the same relation as common alcohol stands to glycol, $C^2 H^6 O^2$. Many facts are in accordance with this view, viz. the formation of propionic acid from glycerine during fermentation. The action of nitric acid on common alcohol produces a substance for which I proposed the name of glyoxylic acid. It appeared probable, according to the above theoretical considerations, that, under similar conditions, an acid might be formed from glycerine homologous with glyoxylic acid. The experiments which I undertook have realized this expectation.

One part of glycerine is mixed with a little more than its bulk of water, and the mixture placed in a long and narrow bottle. By means of a glass tube drawn out to a point at one end, one part and a quarter of red fuming nitric acid is allowed to collect under the mixture of glycerine and water in such a way that two distinct layers, the one of acid and the other of diluted glycerine, are formed. The bottle thus charged is put into a place where it can remain without being disturbed for six or eight days. The temperature should be about 20° C. The nitric acid gradually mixes with the glycerine, the liquid assumes a blue colour, and a brisk evolution of gas takes place which lasts for several days. As soon as the generation of gas ceases and the liquid has become colourless, the reaction may be considered to be at an end; but generally I allowed a few days more before I proceeded with the following operations.

In order to get rid of the excess of nitric acid and other volatile compounds, the contents of the bottle were evaporated in small portions on the water-bath till the liquid assumed the consistency of a strong syrup. The latter was dissolved in water, neutralized with chalk, and the lime-salts produced were precipitated with strong alcohol. A pasty precipitate was obtained. The liquid, which still contained one or two substances which do not crystallize, was carefully poured off and placed aside. The precipitate was treated with boiling water, and the solution filtered off from the undissolved carbonate and oxalate of lime. To the clear filtrate was added slaked lime till it became distinctly alkaline. The slaked lime removes a syrupy substance which interferes with the crystallization of the body desired. The precipitate was filtered off, the excess of lime thrown down from the solution by carbonic acid, and the filtrate from the carbonate of lime concentrated by evaporation. On allowing it to stand, a large quantity of beautiful crystals was obtained. These crystals are the lime-salt of a substance for which I propose the name of glyceric acid. They may be easily purified by recrystallization. Of the other bodies produced by the action of nitric acid on glycerine, I shall treat on another occasion.

Glyceric Acid.

In order to prepare this substance, a quantity of glycerate of lime is dissolved in water and the lime precipitated by its equivalent of oxalic acid. If the exact quantity of the latter, as required by calculation, be taken, no oxalic acid or lime can be discovered in the liquid. With this agrees the following experiment:—

6·001 grms. of glycerate of lime were precipitated with 2·643 grms. oxalic acid. The precipitate gave in the usual way 2·095 grms. carbonate of lime. Consequently from 100 parts of glycerate of lime 13·96 parts of calcium were precipitated in the shape of oxalate. The salt contains 13·98 parts of calcium.

The filtrate from the oxalate of lime which contained the glyceric acid was evaporated on the water-bath until it assumed the consistency of syrup, and then placed over sulphuric acid for a few days. No crystals could be obtained. Some of the syrupy acid was dried at temperatures commencing with 100° C. and rising gradually to 140° C. In this manner a substance of a light brownish colour, and of the appearance of gum-arabic, was obtained.

Glyceric acid thus prepared is highly hygroscopic. A few grains exposed in a platinum crucible to the atmosphere for some hours, became quite liquid. Heated on a piece of platinum-foil it melts, gives off acid fumes, and burns after a short time with a bright flame. The solution of glyceric acid in water has a very pleasant acid taste, similar to tartaric acid; it decomposes the carbonates, coagulates milk, and dissolves zinc and iron. From the iron solution, caustic potash does not completely precipitate the metallic oxide.

A solution of sulphate of copper added to glyceric acid along with caustic potash does not produce a precipitate, provided the potash is in excess. On boiling, a strong brown precipitate is formed. The analytical results obtained with a substance of such physical properties as glyceric acid cannot be expected to be very exact. Before I attempted an analysis, it appeared necessary to obtain further information with regard to its purity. Although made from perfectly pure materials, it had assumed during its preparation a brownish colour. Were the acid still what it was supposed to be, it ought to yield, on neutralization with carbonate of lime, the well-crystallizing glycerate of lime. Some solid glyceric acid, dried at 140° C., was dissolved in water and saturated carefully with lime. On partly evaporating the liquid, a crop of beautiful crystals, with all the properties of glycerate of lime, was obtained. The dish containing them and the mother-liquor was now placed under a bell-glass over sulphuric acid. After a few days the mother-liquor was evaporated. The dry residue consisted of glycerate of lime; but the crystals were covered with a very thin coating of the appearance of varnish. The quantity of the latter was very small, but it showed that a little of the glyceric acid had suffered a change during its preparation. The dry acid could not be converted into powder. A little capsule of platinum-foil contained the substance, dried for ten hours at 105° C. It was weighed within a closed glass tube, placed in the combustion-tube, and well surrounded with chromate of lead.

I. 0·144 grm. gave 0·210 grm. carbonic acid and 0·068 grm. water.

II. 0·110 grm. gave 0·162 grm. carbonic acid and 0·053 grm. water.

In 100 parts:—

	I.	II.
Carbon	39·77	40·16
Hydrogen	5·24	5·35
Oxygen	...	...

The formula $C^3 H^4 O^3$ agrees best with these numbers:—

Carbon . . .	3	36	40·90
Hydrogen . .	4	4	4·54
Oxygen . . .	3	48	54·56
		88	100·00

Pyroracemic acid has the formula $C^3 H^4 O^3$. A salt of this acid mixed with sulphate of iron turns red: a salt of glyceric acid does not show this reaction. The compounds of the latter also differ in composition and properties so much from those of pyroracemic acid, as to clearly establish them as distinct substances. Although glyceric acid is but little fitted for analytical operations, its compounds answer this purpose well. Those which I have examined crystallize well, and may easily be obtained in a state of perfect purity. The general formula of the glycerates is $C^3 H^5 MO^4$, and the corresponding hydrogen compound, $C^3 H^6 O^4$. Therefore, if the formula for glyceric acid, as deduced from the analyses described above, be correct, it must lose one atom of water at 105° C., and be able to take it up again on coming in contact with bases.

Biglycerate of Potash, $C^3 H^5 KO^4 + C^3 H^6 O^4$.

A certain quantity of glyceric acid was divided into two equal parts; one was neutralized with carbonate of potash and then added to the other part. On evaporating this liquid, first on the water-bath, and afterwards *in vacuo* over sulphuric acid, small white crystals were formed. These crystals, separated from the mother-liquor by means of filtering-paper, were dried over sulphuric acid at the ordinary temperature. 0·377 grm. gave 0·114 grm. chloride of potassium.

In 100 parts:—

	Found.	Calculated. $C^3H^5KO^4+C^3H^6O^4$.
Potassium . .	15·8	15·8

If this substance be really biglycerate of potash, it ought to decompose one equivalent of carbonate of lime, and give rise, for each equivalent of potassium it contains, to the formation of one equivalent of glycerate of lime.

0·584 grm. of the supposed biglycerate of potash was dissolved, and the acid solution neutralized with pure carbonate of lime. After boiling it for some time and then filtering it from the undecomposed carbonate, the dissolved lime was precipitated from the filtrate with oxalate of ammonia and converted by the usual method into carbonate of lime. Thus 0·119 grm. of the latter, corresponding to 0·047 grm. calcium, was obtained. Since, according to theory, 250 parts of $C^3H^5KO^4+C^3H^6O^4$ ought to dissolve a quantity of carbonate of lime containing 20 parts calcium, therefore the above 0·584 grm. substance taken should dissolve 0·046 grm. calcium in the shape of carbonate.

	Found.	Calculated.
Calcium . . .	0·047	0·046

From these determinations it follows that the compound in question is biglycerate of potash, and the formula expressing its composition $=C^3H^5KO^4+C^3H^6O^4$. 0·408 grm. of the same was kept for two hours at a temperature of 105° C.; it melted to a clear, transparent, colourless liquid, and lost in weight only 0·005 grm. The same quantity of substance exposed for two hours to 135° C. lost 0·028 grm. The salt had assumed after this treatment a brown colour, and was partly decomposed. The compound $C^3H^5KO^4+C^3H^6O^4$ seems therefore not to contain water of crystallization.

Glycerate of Lime, $C^3H^5CaO^4+H^2O$.

This compound crystallizes in small shining crystals, which frequently resemble lactate of lime in appearance. But if both are viewed by means of a microscope, a striking difference is recognized. Glycerate of lime consists apparently or rhombic plates, and lactate of lime of needles. The crystals of $C^3H^5CaO^4+H^2O$ dissolve easily in hot and cold water, but not in alcohol. Between 130° and 140° C. they melt, and lose quickly one atom of water; on dissolving the substance after this treatment in water, the original salt, $C^3H^5CaO^4+H^2O$, with all its properties, can be obtained again. The solution of glycerate of lime does not change the colour of litmus or turmeric paper, and yields no precipitates with the nitrates of silver and lead, and the chlorides of copper and iron. Boiled with nitrate of silver, a black precipitate is produced, and the addition of a little ammonia causes the sides of the vessel to become covered with a bright coating of silver.

Glycerate of lime, when heated on a piece of platinum foil, increases much in volume, like glycolate of lime, smells like burning sugar, and is converted into carbonate of lime. Exposed to a high temperature in a little retort, a yellow oil and a liquid like acetone distil off, whilst a black residue remains.

I. 0·279 grm., dried at the ordinary temperature over sulphuric acid and burnt with chromate of lead, gave 0·258 grm. carbonic acid and 0·129 grm. water.

0·225 grm. of the same substance, dissolved in water and precipitated with oxalate of ammonia, gave 0·079 grm. carbonate of lime.

II. 0·337 grm. from another preparation, dried and burnt in the same way as the substance under I., furnished 0·311 grm. carbonic acid and 0·154 grm. water.

0·348 grm. gave 0·123 grm. carbonate of lime.

A small quantity heated strongly with potassium gave a residue which did not contain cyanide of potassium; another specimen burnt with a mixture of hydrated lime and oxide of potassium did not evolve ammonia.

According to these experiments, glycerate of lime contains no nitrogen.

In 100 parts:—

	I.	II.
Carbon	25·21	25·16
Hydrogen . .	5·13	5·07
Calcium	14·04	14·13
Oxygen	55·62	55·64
	100·00	100·00

The formula $C^3H^5CaO^4+H^2O$ requires—

Carbon	3	36	25·17
Hydrogen	7	7	4·89
Calcium	1	20	13·98
Oxygen	5	80	55·96
		143	100·00

The above compound loses at 135° one atom of water. 1·204 grm. dried over sulphuric acid, lost at 135° C. 0·146 grm. water.

0·476 grm. from another preparation lost at 135° C. 0·058 grm. water. Consequently 100 parts contain—

	I.	II.	Theory.
Water.	12·12	12·18	12·5

and the formula for the anhydrous compound $=C^3H^5CaO^4$.

In order to ascertain if more than one atom of water could be expelled from glycerate of lime, a certain quantity of it, dried at the ordinary temperature, was kept for a few hours at about 175° C. In consequence of this high temperature the substance increased considerably in bulk, perhaps twenty times more than its original volume, and was partly decomposed. The loss in weight was scarcely 2 per cent. more than corresponds to one atom of water. Consequently no more water can be expelled by application of heat from $C^3H^5CaO^4$. Glycerate of lime which has been heated just to the point at which decomposition takes place, is extremely hygroscopic. Some of it thrown into a beaker containing only a few drops of water, disappeared as by magic, forming a brown syrupy solution. After the latter had been standing for a few days it had become one solid mass of crystals, the shape and properties of which were the same as those of $C^3H^5CaO^4+H^2O$. From these experiments it appears that glycerate of lime can exist in two modifications: one extremely hygroscopic, and soluble in very little water, the other less soluble in water. I have made similar observations with the zinc-salt.

Biglycerate of lime does not appear to exist. From 3·622 grms. of glycerate of lime, half the lime was precipitated by 0·798 grm. of oxalic acid, and the filtrate from the precipitate evaporated to the consistency of strong syrup and allowed to stand in the exsiccator for some days. The crystals formed were placed on filtering-paper and dried *in vacuo*.

0·370 grm. lost at 140° C. 0·042 grm. water.

0·692 grm., dissolved in water and precipitated with oxalate of ammonia, gave 0·238 grm. carbonate of lime.

In 100 parts:—

	Found.	Theory. $C^3H^5CaO^4+H^2O$.
Water	11·35	12·5
Calcium	13·7	13·98

These numbers correspond without doubt to the neutral salt. The acid mother-liquor did not yield any crystals after standing many days.

Glycerate of Ammonia, $C^3H^5(NH^4)O^4$.

This substance was obtained by precipitating glycerate of lime with its equivalent quantity of oxalate of ammonia, and concentrating the filtrate from the oxalate of lime by evaporation in the water-bath. A little ammonia escapes with the steam, and therefore it is necessary to add a few drops of this reagent to the liquid in order to render it neutral again. It is then placed along with some sulphuric acid under a bell-glass and left to crystallize. In this way beautiful prismatic crystals of the desired substance are obtained.

0·355 grm., dried *in vacuo* and burnt with chromate of lead, gave 0·388 grm. carbonic acid and 0·248 grm. water.

For 100 parts:—

				Found.
Carbon	3	36	29·26	29·80
Hydrogen . . .	9	9	7·31	7·76
Nitrogen . . .	1	14		
Oxygen	4	64		

The salt attracts moisture very quickly, and easily loses ammonia. From these facts, the difference between the numbers obtained by calculation and experiment may be explained. A concentrated solution of glycerate of ammonia gives off ammonia on boiling; gradually the liquid assumes a yellow colour, and the smell of ammonia disappears. The dry compound melts on application of heat, and comports itself like the concentrated solution. The latter does not change the colour of litmus-paper, and when diluted with a little water, produces no precipitates with the nitrates of lead and of silver, nor with the chlorides of iron and of calcium.

Glycerate of Zinc, $(C^3H^5ZnO^4)^2+H^2O$.

Into a warm solution of glyceric acid, carbonate of zinc was introduced till the acid reaction had nearly disappeared and the filtered liquid was concentrated by evaporation. After allowing it to stand for some time, small colourless crystals of the above compound were obtained. It is easily soluble in cold and warm water: sulphuretted hydrogen removes the zinc from the solution completely.

0·290 grm., dried *in vacuo* and burnt with chromate of lead, gave 0·265 grm. carbonic acid and 0·113 grm. water.

In 100 parts:—

Carbon	24·92
Hydrogen. . . .	4·32

Calculated according to the formula $2(C^3H^5ZnO^4)+H^2O$:—

Carbon	24·55
Hydrogen . . .	4·09

0·341 grm. of the same substance lost at 140° C. 0·022 grm. water. Consequently 100 parts would lose 6·4 parts of water.

If we assume that one atom of water H^2O escapes from the compound $2(C^3 H^5 ZnO^4) + H^2O$, the loss ought to be 6·1 per cent.

0·214 grm. dried at 140° C., furnished 0·206 carbonic acid and 0·075 grm. water.

0·4075 grm. of the same substance furnished 0·121 grm. oxide of zinc.

100 parts contain, according to these determinations,—

Carbon	26·23
Hydrogen	3·89
Zinc	23·83

The formula $C^3 H^5 ZnO^4$ corresponds to—

Carbon	3	36	26·16
Hydrogen	5	5	3·63
Zinc	1	32·6	23·69
Oxygen	4	64	
		137·6	

The substance, the analysis of which has just been communicated, was now exposed to a higher temperature than 140° C.

0·341 grm., dried *in vacuo,* was kept for some time at 140° C., and subsequently for one hour at 170° C. The glycerate of zinc thus treated had lost 0·059 grm. in weight, and was partly decomposed. On its then being treated with boiling water it did not dissolve completely, but left a brown residue. The filtrate from the latter could be evaporated to the consistency of syrup without yielding crystals. When evaporated to dryness *in vacuo,* a mass, consisting evidently of two substances, was obtained. Consequently the formula of the anhydrous zinc-salt is $C^3 H^5 ZnO^4$.

Glycerate of Lead, $C^3 H^5 PbO^4$,

may be obtained easily by the following method:—To a diluted and warm solution of glyceric acid pure oxide of lead is gradually added in small portions, till the acid reaction of the liquid is nearly destroyed, and the filtered solution concentrated by evaporation on the water-bath. If it be now allowed to stand in a cold place for some time, a hard crystalline crust of the above substance is formed on the sides of the evaporating basin. If the glyceric acid employed be too concentrated, the lead compound separates immediately after the addition of the oxide, as a white crystalline powder.

Glycerate of lead dissolves with difficulty in cold, but pretty easily in hot water: the solution does not alter blue litmus-paper. Some of the solid compound, well dried, was placed with some iodide of æthyle in a sealed glass tube and kept for seventy hours at 110° C. Only a very small portion of the glycerate of lead was decomposed. In a second experiment the temperature was raised to 145° C., and in a third to 170° C. without better success. Although in the two last cases the heating was continued for two days, the residue which was left after the excess of the iodide of æthyle had been distilled off was inconsiderable, and of a brown unpromising appearance. It was not advisable to employ higher temperatures because the lead-salt decomposes at about 170° C.

0·535 grm., dried at the ordinary temperature over sulphuric acid, furnished 0·331 grm. carbonic acid and 0·119 grm. water.

0·52 grm. of the same substance gave 0·277 grm. oxide of lead.

100 parts contain:—

Carbon	16·87
Hydrogen	2·47
Lead	49·44

Calculated according to $C^3 H^5 PbO^4$:—

Carbon . .	3	36	17·24
Hydrogen. .	5	5	2·39
Lead . . .	1	103·7	49·71
Oxygen . .	4	64	
		208·7	

The glycerate of lead could be heated to 140° C. without losing in weight or altering in colour; but at 165° C. it turned brown and was decomposed. After having been kept an hour and a half at this temperature, the loss in weight did not amount to two-thirds of an atom of water. Consequently the formula of the anhydrous salt is $C^3 H^5 PbO^4$. A series of experiments undertaken for the purpose of obtaining the æthyle compound of this acid have not yet been successful. How the lead-salt comports itself with iodide of æthyle has already been mentioned. The silver-salt could be prepared, but was found so very changeable as to be unfit for further experiments. Some anhydrous glyceric acid dissolved in absolute alcohol, and hydrochloric acid gas passed through the solution, did not yield, either at the ordinary temperature or at 100° C., the desired substance. Nor did the distillation of $C^3 H^5 CaO^4$ and Ae.K.O, SO^3 give any satisfactory results.

The compounds of glyceric acid are represented by the formula $C^3 H^5 MO^4$, or, according to the atomic weights used by many chemists, $C^6 H^5 MO^8$. The corresponding hydrogen compound is $C^3 H^6 O^4$ or $C^6 H^6 O^8$. If glycerine be regarded as a body analogous to the alcohols, the formation of glyceric acid may be compared to that of acetic acid from common alcohol.

$$C^2H^6O + O^2 = C^2H^4O^2 + H^2O.$$

Alcohol. Acetic acid. Water.

$$C^3H^8O^3 + O^2 = C^3H^6O^4 + H^2O.$$

Glycerine. Glyceric acid. Water.

Glyoxylic and glyceric acids and their compounds are homologous. Further experiments are required to determine whether glyceric acid is mono- or bibasic.

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XXVII. *On the Law of* Bode, *with a remarkable coincidence in reference to the Satellite System of Jupiter; and on the Rotation of a Heavenly Body. By* Mr. Septimus Tebay, *B.A., Head Master of Queen Elizabeth's Grammar School, Rivington, Lancashire*[*].

THE actual distances of the planets from the sun were not known to the ancients, but their relative distances were known with considerable accuracy. It was from the relative distances that Kepler established his third law. This distinguished geometer had noticed the regular increase in the distances of the planets proceeding outwards from the sun, and predicted from analogy that a planet would hereafter be discovered in the enormous space between the orbits of Mars and Jupiter, no planet at that time being known to occupy this interval.

The discovery of Uranus by Sir William Herschel in 1781, gave additional weight to the prediction of Kepler; the distance of Uranus being nearly double the distance of Saturn from the sun. The remarkable law of Bode, which was about this time discovered, caused astronomers to scrutinize more minutely the space between Mars and Jupiter, in hopes of detecting the unknown world which had hitherto eluded observation; and early in the present century four small planets were actually discovered, thus completing the chain of interplanetary spaces. At the present time not fewer than fifty-one of these small planets, or asteroids, have been observed. They are supposed by many persons to be the fragments of a large planet, which from some unknown cause has burst. Their mean distances are all nearly equal, and (with sufficient accuracy) the same as that pointed out by Bode's law. By this law, if the mean distance of the earth be denoted by unity, the mean distances of the other planets are approximately as follows:—

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[*] Communicated by the Author.

Mercury	·0+·4= ·4
Venus	·3+·4= ·7
Earth	·6+·4= 1·0
Mars	1·2+·4= 1·6
Asteroids	2·4+·4= 2·8
Jupiter	4·8+·4= 5·2
Saturn	9·6+·4=10·0
Uranus.	19·2+·4=19·6
Neptune	38·4+·4=38·8

If we compare these numbers with the actual distances as set down in the following Table, we shall see that there is a very close agreement, with one exception, viz. that of Neptune:—

	Mean distance.	Mass.
Mercury	·387	0·12
Venus	·723	0·88
Earth	1·000	1·00
Mars	1·523	0·13
Ceres	2·767	
Jupiter	5·202	338·45
Saturn	9·538	101·03
Uranus.	19·183	14·78
Neptune	30·2	24·57

Here the continuity in the law of Bode, which assigns the distances of all the other planets with such remarkable exactness, ceases. Still, when we consider the infinite wisdom displayed by the great Creator of the universe in the mechanism of our planetary system—since it has been ascertained beyond doubt that it is a system of wonderful stability and equilibrium, and that there does not exist a single inequality of any considerable amount—that the masses of the planets seem to follow no law whatever, but that the mean distances do follow a very remarkable progression—is it not probable that there may yet be a law almost, if not entirely, independent of the masses themselves, by which the planets are linked together, and of which the law of Bode is only an approximation? The problem which I long ago proposed to myself, is the following:—The limits of the solar system being given, is it possible to assign the orbits of the intermediate planets so that at any time they shall be least disturbed? To take a more simple case: suppose there were only three planets, Mercury, Venus, and the Earth; and supposing Mercury and the Earth to move in elliptic orbits; to assign the orbit of Venus so that at any time she shall be least disturbed. Since an undisturbed planet describes an ellipse, it would seem that Venus will be least disturbed when the path described is most nearly elliptical.

I have not succeeded in obtaining a satisfactory solution of this problem, so long as the third planet is taken into account; but on considering the motion of a planet disturbed by an *interior* one, I have fallen (in my lucubrations) on a rather singular coincidence, which I now proceed to describe.

Let a be the semi-major axis of the elliptic orbit of a disturbed planet (m), a' the semi-major axis of the elliptic orbit of the disturbing planet (m'). Then, neglecting the excentricities, we have

$$\delta r = \frac{1}{6} m' a^3 \frac{dA^{(0)}}{da} + \&c.,$$

where

$$A^{(0)} = -\frac{2}{a}\left\{1 + \left(\frac{1}{2}\right)^2 \left(\frac{a'}{a}\right)^2 + \left(\frac{1.3}{2.4}\right)^2 \left(\frac{a'}{a}\right) + \&c.\right\}.$$

Now it is remarkable that the expression

$$a^3 \frac{dA^{(0)}}{da} = a\left\{1 + 3\left(\frac{1}{2}\right)^2 \left(\frac{a'}{a}\right)^2 + 5\left(\frac{1.3}{2.4}\right)^2 \left(\frac{a'}{a}\right)^4 + \&c.\right\}$$

admits of a minimum value, viz. when

$$1.3\left(\frac{1}{2}\right)^2 \left(\frac{a'}{a}\right)^2 + 3.5\left(\frac{1.3}{2.4}\right)^2 \left(\frac{a'}{a}\right)^4 + \&c. = 1.$$

Perhaps the readiest way of determining $\frac{a'}{a}$ from this equation, is by the rule of double position. Let u_n be the general term of the series; then putting $\frac{a'}{a} = \alpha$, we have

$$u_n = (2n-1)(2n+1)\left\{\frac{1.3\ldots\overline{2n-1}}{2.4\ldots 2n}\right\}^2 (\alpha)^{2n},$$

$$u_{n+1} = (2n+1)(2n+3)\left\{\frac{1.3\ldots\ \overline{2n+1}}{2.4\ldots 2(n+1)}\right\}^2 (\alpha)^{2(n+1)};$$

$$\therefore\ \frac{u_{n+1}}{u_n} = \frac{2n+3}{2n-1}\left\{\frac{2n+1}{2(n+1)}\right\}^2 (\alpha)^2;$$

$$\begin{aligned}\therefore\ \log u_{n+1} = \log u_n &+ 2\log\overline{2n+1} + \log\overline{2n+3} \\ &-2\log\overline{n+1} - \log\overline{2n-1} - 2\log 2 \\ &+2\log(\alpha).\end{aligned}$$

Suppose $\alpha = \frac{1}{2}$. Then

$$u_1 = \tfrac{3}{16},\quad \log u_1 = \bar{1}{\cdot}2730013;$$

and the other terms are calculated from the formula

$$\begin{aligned}\log u_{n+1} = \log u_n &+ 2\log\overline{2n+1} + \log\overline{2n+3} \\ &-2\log\overline{n+1} - \log\overline{2n-1} - 4\log 2.\end{aligned}$$

Thus we find

$$\begin{array}{ll}
\log u_1 = \bar{1}{\cdot}2730013, & \log u_7 \ = \bar{4}{\cdot}7178693, \\
\log u_2 = \bar{1}{\cdot}1200339, & \log u_8 \ = \bar{4}{\cdot}1762574, \\
\log u_3 = \bar{2}{\cdot}7275880, & \log u_9 \ = \bar{5}{\cdot}6272125, \\
\log u_4 = \bar{2}{\cdot}2648165, & \log u_{10} = \bar{5}{\cdot}0723701, \\
\log u_5 = \bar{3}{\cdot}7675362, & \log u_{11} = \bar{6}{\cdot}5128775, \\
\log u_6 = \bar{3}{\cdot}2495999, & \log u_{12} = \bar{7}{\cdot}9495712.
\end{array}$$

Hence we have

$$1 - 1\,.\,3\left(\frac{1}{2}\right)^2\left(\frac{1}{2}\right)^2 - 3\,.\,5\left(\frac{1\,.\,3}{2\,.\,4}\right)^2\left(\frac{1}{2}\right)^4 - \&\text{c.} = {\cdot}6 \text{ nearly.}$$

So that the supposition $\alpha = \frac{1}{2}$ is too small. The above numbers may be conveniently employed in calculating the values of u'_1, u'_2, &c. corresponding to any other value of α. For

$$\log u'_n = \log u_n + 2n \log(2\alpha).$$

Suppose $\alpha = \frac{5}{8} = {\cdot}625$;

$$\therefore\ \log u'_n = \log u_n + n \times {\cdot}19382;$$

and we shall find, as before,

$$1 - 1\,.\,3\left(\frac{1}{2}\right)^2\left(\frac{5}{8}\right)^2 - 3\,.\,5\left(\frac{1\,.\,3}{2\,.\,4}\right)^2\left(\frac{5}{8}\right)^4 - \&\text{c.} = -0304.$$

Therefore the supposition $\alpha = {\cdot}625$ is too great.

Suppose $\alpha = {\cdot}621$,

$$\therefore\ \log u'_n = \log u_n + n \times {\cdot}1882432.$$

We shall find in the same way,

$$1 - 1\,.\,3\left(\frac{1}{2}\right)^2({\cdot}621)^2 - 3\,.\,5\left(\frac{1\,.\,3}{2\,.\,4}\right)^2({\cdot}621)^4 - \&\text{c.} = {\cdot}0025.$$

Hence the true value of $\frac{a'}{a}$ lies between ·621 and ·625. By the rule of double position we find

$$\frac{a'}{a} = {\cdot}6213,$$

which is very near the true value. If the excentricities of the two orbits be taken into account, we shall have

$$\delta r = \frac{1}{6}\, m' a^3 \frac{dA^{(0)}}{da} + \alpha e^2 + \beta e e' + \gamma e'^2$$
$$+ \alpha' e^3 + \beta' e^2 e' + \gamma' e e'^2 + \delta' e'^3 + \&\text{c.},$$

where α, β, &c. are functions of a, a', &c.

If e, e' be regarded as independent variables, we have

$$0 = 2\alpha e + \beta e' + 3\alpha' e^2 + 2\beta' e e' + \gamma' e'^2 + \&\text{c.},$$
$$0 = 2\gamma e' + \beta e + 3\delta' e'^2 + 2\gamma' e e' + \beta' e'^2 + \&\text{c.}$$

These equations are satisfied by

$$e=0, \quad e'=0.$$

We therefore have this curious result. The mean motions in the elliptic and disturbed orbits being equal, the mean distances will be most nearly equal when $\frac{a'}{a}=\cdot 6213$, and at the same time the elliptic orbits of the disturbed and disturbing planets have no excentricities independent of perturbation.

If n, n' be the mean motions, we have, neglecting the masses,

$$\frac{n'}{n}=\left\{\frac{1}{\cdot 6213}\right\}^{\frac{3}{2}}=2\cdot 042;$$

or the periodic time of the second planet is a little more than double that of the first.

Our attention is next directed to the satellite system of Jupiter; for we know that the orbits of the first three satellites have no excentricities independent of perturbation, at least the excentricities are very small; and the periodic time of the second satellite is a little more than double that of the first, and the periodic time of the third a little more than double that of the second. The mean distances of the first three are

$$3\cdot 03, \quad 4\cdot 81, \quad 7\cdot 68,$$

and

$$\frac{3\cdot 03}{4\cdot 81}=\cdot 62, \quad \frac{4\cdot 81}{7\cdot 68}=\cdot 62.$$

A closer agreement could scarcely be expected. I do not wish it to be understood that this has reference to any law of mechanical cosmogony; if, however, the time be regarded as indefinitely great, the disturbed planet will be out of its elliptic place by the least possible amount when the above conditions are satisfied. This is curious, and it is the only approach which I have been able to make towards a verification of the law of Bode, out of twelve years' application. Even in this case there is a pretty tolerable agreement in some instances, as will be seen from the following Table, where the number opposite each pair of planets is the ratio of their mean distances.

Mercury and Venus	·534
Venus and Earth	·732
Earth and Mars	·656
Mars and Ceres	·550
Ceres and Jupiter	·531
Jupiter and Saturn	·545
Saturn and Uranus	·500
Uranus and Neptune	·635

Note on the Rotation of a Heavenly Body.

When a distant body is attracted by a central force, the resultant attraction does not in general pass through the centre of gravity of the attracted body, but produces an angular acceleration about an axis passing through this point. For instance, let the attracted body be a uniform straight rod, whose dimensions are small in comparison with its distance from the central force; and suppose the centre of gravity of the rod to move in a circle about the centre of attraction with a uniform angular motion (n). Suppose, for simplicity, that the rod is situated in the plane of motion, and that ϕ is its inclination to a line drawn through the centre of the circle. If t be the time which has elapsed since the centre of the rod was in the initial line, the equation of angular motion is

$$\frac{d^2\phi}{dt^2}=\alpha^2\sin 2(nt-\phi),$$

where α is a small constant quantity. The equation of motion is the same for any solid body whatever.

If the equation be written in the form

$$-\frac{d^2(nt-\phi)}{dt^2}=\alpha^2\sin 2(nt-\phi),$$

it immediately admits of integration;

$$\therefore\ \frac{d(nt-\phi)}{dt}=\pm\sqrt{c^2+\alpha^2\cos 2(nt-\phi)},$$

c being an arbitrary constant. In order that the motion may be real for all values of t, it appears that c must not be less than α.

When $c=\alpha$, the equation can be completely integrated. In this case we have

$$\frac{d(nt-\phi)}{dt}=\pm\alpha\sqrt{2}\cos(nt-\phi);$$

$$\therefore\ \alpha\sqrt{2}\,.\,t+\beta=\pm\frac{1}{2}\log e\frac{1+\sin(nt-\phi)}{1-\sin(nt-\phi)};$$

$$\phi=nt-\sin^{-1}\frac{e^{\pm 2(\alpha\sqrt{2}\,.\,t+\beta)}-1}{e^{\pm 2(\alpha\sqrt{2}\,.\,t+\beta)}+1};$$

$$\therefore\ \frac{d\phi}{dt}=n\pm\frac{2\alpha\sqrt{2}}{e^{\pm(\alpha\sqrt{2}\,.\,t+\beta)}+e^{\mp(\alpha\sqrt{2}t+\beta)}}.$$

If t be increased indefinitely, this becomes

$$\frac{d\phi}{dt}=n;$$

that is, the motion of rotation is ultimately equal to the angular motion in the orbit, and takes place in the same direction. This is analogous to the moon's motion.

If a sphere be covered, or partially covered, with water, and made to revolve about a central force, but not necessarily about an axis within itself, the water will be disturbed; and since it does not attain its greatest elevation instantaneously, the central force will tend to give it a rotatory motion. The motion will be similar to that investigated above, but much more complicated. The motion of a body nearly spherical, and having a large mountain on its surface, will also be similar.

Rivington, February 8, 1858.

XXVIII. *On the Evidence of a Graduated Difference between the Thermometers of Air and of Mercury below* 100° *C. derived from* M. Regnault's *observations on the Tension of Aqueous Vapour.* *By* J. J. WATERSTON, *Esq.*

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE comparison between the thermometers of air and of mercury made by M. Regnault, and of which an account is given in the *Ann. de Chim.* vol. v. 3rd series, may seem, from the eminent character of the observer, to justify the withdrawal of our confidence from the results of MM. Dulong and Petit's previous inquiry on the same subject (*Ann. de Chim.* 1817): nevertheless, before yielding to this conclusion, there is certain evidence in favour of the accuracy of MM. Dulong and Petit's deductions, which appears to me to merit attention, inasmuch as it is derived from the observations of M. Regnault himself in another field of research. Hoping that you may consider the subject of sufficient importance, I have drawn up the following paper for the Philosophical Magazine, giving an account of this evidence, and an explanation of the processes of computation by means of which it was obtained. The graphical projection into which the results are cast (fig. 2) enables the proofs of the graduated difference in question to appeal to the eye with but little demand on the attention of the reader. Nevertheless, all the details necessary to enable him fully to check those results will, I trust, be found set forth distinctly and without reserve or bias.

§ 1. In M. Regnault's *Etudes sur l'hygrométrie* (*Ann. de Chim.* vol. xv.) we are presented with observations on the tensions of the vapour given by six mixtures of sulphuric acid and water in different proportions, at temperatures ranging between 4° and

52°. Each of these observations I subjected to the following process of computation. Adding 274 to the temperature, I divided the corresponding tension by the sum, and extracted the sixth root of the quotient. This was laid off by convenient scale as ordinate to the temperature. The points thus obtained ranged distinctly in straight lines; each mixture presenting a gradient of density. These gradients were found all to converge to one point, which was situated on the base-line upon which the temperatures were marked off, and from which the ordinates were measured (*i. e.* the axis of temperature). The position of this central point corresponded to the temperature —77°.

M. Regnault's observations on the tension of the vapour of pure water below 100° being treated in the same way, presented also a straight line directed to the same point. [See Appendix, I.]

The same system of projection was applied to observations on the vapours of other liquids collected from scientific journals and publications. In all cases they seemed to range in straight lines, some of which were parallel to others, and some converged towards a point in the axis of temperature, *e. g.* the gradients of sulphuric æther, cyanogen, and ammonia vapour, show parallelism to that of water. Alcohol radiates from the same point on the axis as water.

§ 2. The chart containing the observations thus projected was submitted to the Royal Society in 1852. It will not be necessary to reproduce that chart here, as I shall show independently how exactly M. Regnault's observations on steam conform to the straight line; and the further proof of what has been stated with respect to the vapours of the mixtures of sulphuric acid and water, may be obtained by inspecting the Table of comparative tensions given by M. Regnault at pp. 179, 180, 181, vol. xv. *Ann. de Chim.* The convergence of gradients of density obviously implies a constancy in the ratios of tension at the same temperature [see Appendix, II.], and in the above-mentioned Table we have columns in which this ratio has been computed for each degree.

Beginning with the last mixture, in which there are 18 equivalents of water, the ratio, it will be remarked, varies irregularly between ·828 and ·841, and the greatest divergence from the mean value is equal to an error of $\frac{1}{500}$th of an inch of the column of mercury. In the next, the ratio varies between ·677 and ·701 and the implied maximum error, about $\frac{1}{400}$th of an inch. The same slight divergence characterizes them all; and although the first and second show a considerable diminution of the ratio at the higher temperature, yet the observed tensions are so small, that an error of not more than $\frac{1}{700}$th of an inch of mercury would account for the difference.

§ 3. The chart above referred to as being in the archives of the Royal Society, contains observations on the density of the vapour of alcohol, sulphuric æther, and sulphuret of carbon, carried up to about 200°. They were made in sealed graduated tubes, so that the results should be uninfluenced by a deviation from Mariotte's law, which must in some degree affect all densities derived from measurements of tension. In projecting these, the temperatures were reduced to the standard of the air-thermometer by a formula derived from MM. Dulong and Petit's observations on the expansion of mercury and glass from 0° to 300°. [See Appendix, III.] These represent mercury in glass to be 3° in advance of the air-thermometer at 200°, and I felt certain that with such corrections the points conformed more nearly to a straight line than if they were uncorrected. M. Regnault does not admit of any correction being required up to 200°, and further, that at about 50° the air-thermometer is behind the mercury.

If the expansibility of mercury increases with the temperature, it is plain that it must be behind the air-thermometer at temperatures between 100° and 0°, unless there is compensation from glass diminishing in its expansibility to the same amount as mercury increases in its rate of expansion. But M. Regnault and M. Pierre both agree with MM. Dulong and Petit, as to glass experiencing an augmentation in its coefficient of dilatation. Hence, if there is continuity in the law of expansion of mercury, the conclusion is inevitable that the mercury is behind the air between the fixed points of the scale. The amount computed by the formula is 0°·512 at 50°, 0·481 at 60°, 0·413 at 70°, 0·31 at 80°, and 0·171 at 90°.

§ 4. Let us first proceed on the hypothesis that the thermometer employed by M. Regnault in his researches on the tension of aqueous vapour from 100° to 50°, corresponded with the formula, and was actually behind a perfect air-thermometer exactly the amount specified at the respective temperatures. Let us also, for the present, assume that the gradient of density with the temperatures thus corrected is exactly a straight line. The projection of M. Regnault's observations with the temperatures *uncorrected* will not range exactly in a straight line, but in an arch concave towards the axis of temperature. Thus, *e. g.* 50° by the mercury indicates 50°·51 by the air: we have therefore, by disregarding the correction, applied to abscissa 50° the ordinate of density that belongs to 50°·51; to abscissa 60° the ordinate of density that belongs to 60°·47; to abscissa 70° the ordinate that belongs to 70°·42, &c.

Suppose the scales of projection on the chart to be so arranged that the line that approximately represents M. Regnault's observations is inclined at an angle of 45° to the axis of temperature, the distance of the points above the straight line measured in the vertical is equal to 0·51 at 50°, to 0·48 at 60°, to 0·42 at 70°, &c. It is hardly possible to recognize this deflection on the chart, however large the scale employed, but it may be made quite apparent to the eye by projecting the points orthographically with the distances extremely foreshortened in the direction of the gradient.

The points computed from M. Regnault's observations with temperatures uncorrected being laid down as before described on a plane surface; suppose we look along the line of these points with the eye close to the plane on which they are marked, we shall see inequalities of position under a highly exaggerated aspect, in consequence of the extreme foreshortening of the longitudinal intervals between them. Suppose the same observations with M. Regnault's temperatures corrected by the formula are also projected in this way; we obtain the means of effectively comparing their positions, and thus of testing the value of the assumptions made with respect to the correction of the thermometer and the gradient of density.

§ 5. Let S (Plate I. fig. 1) be the standard point on the chart corresponding to the density of steam at 100°, and *m* the point corresponding to density at 50°. Join S*m* (in the figure, to save confusion, the lines joining the several points and S are not drawn), and draw *m*G parallel to the axis, and SG perpendicular to it. In *m*G, take *m*a equal to the assumed correction of the mercury thermometer at 50°, viz. 0°·512. We have $\frac{mG}{SG}=$ cotangent inclination of *m*S to axis; $\frac{aG}{SG}=$ cotangent inclination of aS to axis; and $ma=$ difference of cotangents × SG. In the same way we have $nb=$ difference of cotangents of inclination of *n*S and bS to axis × SK = the assumed correction of the mercury thermometer at 60°, and so on for the other points.

Let the orthographical projection be made in the direction Sa inclined to the plane of the paper about 1°. The effect will be to reduce distances in the direction aS to one-sixtieth, and maintain them in the same relative proportion: thus SB to SA (fig. 2) is equal to bK to aG (fig. 1) $=(100^\circ-60^\circ{\cdot}481)$ to $(100^\circ-50^\circ{\cdot}512)$. The differences *m*a, *n*b &c. (fig. 1) are reduced in the ratio of $\sqrt{\frac{1}{2}}$, the scales of the chart being supposed to be adjusted so as to make $SaG=45^\circ$. Also *m*a (fig. 1) becomes MA (fig. 2), nearly perpendicular to AS; the abscissa of M from S upon SA being 50, while $SA=48{\cdot}488$, and the ordinates are the difference of cotangents.

The following Table contains in column 3 the cotangents of the inclination of the straight lines that connect the standard point 100° with the points corresponding to the respective densities at the temperatures 50°, 60°, 70°, 75°, 80°, and 90°. In column 4 are given the cotangents for the same densities computed with the temperatures corrected by the formula. [Appendix, III.] The mode of computing the values of (h) the cotangents is explained in Appendix, III. The equation is

$$h=\frac{t_1-t}{\sqrt[6]{\frac{e_1}{t_1}}-\sqrt[6]{\frac{e}{t}}}$$

in which t is the temperature reckoned from the zero of gaseous tension (found by adding 273°·89 to the temperature by thermometer), e the tension in millimetres. The tabular numbers are the numbers resulting from computing the above formula divided by 100. The letters refer to fig. 2, and the columns (6, 7) of differences show the differences between the cotangent at A, and the others respectively.

Tensions.	Temperatures.	Cotangents.		Points on fig. 2.	Diff. of cotangents.	
1	2	3	4	5	6	7
mm 91·98	50°	1·58854	....	M ...	+·01733	
	50·512	....	1·57121	... A	·0	·0
148·79	60	1·59025	....	N ...	+·01904	
	60·481	....	1·56983	... B	....	−·00138
233·09	70	1·59357	....	O ...	+·02236	
	70·413	....	1·57006	... C	....	−·00115
288·50	75	1·59571	....	P ...	+·02450	
	75·366	....	1·57053	... D	....	−·00068
354·64	80	1·59816	....	Q ...	+·02695	
	80·310	....	1·57155	... E	....	+·00034
525·45	90	1·60387	....	R ...	+·03266	
	90·171	....	1·57428	... F	....	+·00307
760	100	....	....	S		

The mode of drawing fig. 2 will now be apparent. MA, NB, OC, &c. are parallel; the distance between AM and NB = (60·481−50·512); the distance between AM and CO = (70·413−50·512) &c. upon one scale, and by another scale the cotangent differences are laid off on AM: those with positive signs on one side of A towards M; those with negative signs on the other side of A towards L. Lines are then drawn to S, and the points of the projection are found at the intersection of these lines with their respective parallels. Thus,

AM = ·01733 by scale 1 inch to ·01 = 1·733 inch.
An = ·01904 „ „ = 1·904 „
&c.

Sn intersects NB in NS which is the point corresponding to M. Regnault's observations at 60°, and So intersects CO in O, which is the point of the same for 70°, &c.

§ 6. Thus M N O P Q R S represent M. Regnault's observations with temperatures uncorrected, and A B C D E F S the same with temperatures corrected by the formula. If this latter coincided with the straight line AS, we should have the desired proof at once of the thermometric formula, of the law of density, and of the extreme accuracy of M. Regnault's observations.

With reference to the slight apparent deviation, we must keep in view that the tensions employed are, strictly speaking, not those actually observed, but those given in the Table at the end of M. Regnault's memoir computed from the empirical formula, $\log e = a + b\alpha^t - c\beta^t$, the constants of which were determined from the graphically equalized observations at 0°, 25°, 50°, 75°, and 100°. There is abundant evidence in the memoir of the remarkable efficiency of this formula to represent the observations within their limits of error. It does not profess to represent the true line, but it must coincide with it at the five points, and at intermediate temperatures cannot be more than a small fraction of a degree removed from it.

The line A B C D E F S thus represents the formula of interpolation, and its relation to the straight line A S, it will be remarked, is precisely what might be expected to subsist between an empirical and a true curve of tension. It intersects, and *intersection,* not *contact,* is the character of such formulæ, as proved by the differences between the computed and observed quantities being alternately of different signs.

If we suppose the point B shifted to k, C to l, &c., and the points N, O, P shifted the same respective amounts, these points, N, O, P, &c., trace out the curved line M w w w, which conforms nearly to a circular arc. There is little doubt that this curve represents the true equalized densities observed by M. Regnault, while M, N, O, &c. represents the densities by the empirical formula. We have thus a semi-arch and semi-chord defining the graduated difference between the air- and mercury-thermometers below 100°.

§ 7. The only evidence of real discrepancy is to be found at D, which ought to coincide with j. If we take $A\alpha = \frac{1}{11}$ AM and $D\delta = \frac{1}{11}$ DP, then $\alpha\delta$S is a straight line. Thus the difference jD, which does not amount to more than $\frac{1}{100}$th of a degree, may indicate that the formula gives the correction $\frac{1}{10}$th in excess throughout. Otherwise it may be caused by it not perfectly representing the curve of expansion of mercury in glass between 50° and 100°. Otherwise M. Regnault's graphical curve of equalized observations may thus far be removed from perfect accuracy, or it may be caused by the deviation from Mariotte's law affecting the air-thermometer. (See § 18.)

That so small an adjustment should bring M. Regnault's observations into *perfect* harmony both with the law of density and with the thermometric formula constructed upon MM. Dulong and Petit's data, cannot be viewed as otherwise than remarkable. Can such a coincidence be accidental? Is it illusory?

§ 8. The coefficient of the expansion of mercury in glass must change with the quality of the glass. M. Pierre found the coefficient of glass between 0° and 100° to vary from ·0026 to ·0019*, which is from about $\frac{1}{6}$th to $\frac{1}{10}$th the coefficient of mercury; hence the absolute expansion of mercury in glass may vary to the extent of $\frac{1}{22}$nd part.

How this may influence the comparison of thermometers, we may perhaps most easily understand by means of a diagram such as fig. 3. Let A D represent the curve of expansion of mercury in glass with the greater coefficient, and A C the same with the lesser coefficient. Let $DC = \frac{1}{22} DB$, and $AB = 100°$. If the arch A C were flatter than the arch A D in the proportion of B C to B D, there could be no difference between the thermometers, and the formula of correction for one would apply to the other. The subtense of the arch of expansion represents the uniform rate of the air-thermometer, and its extension beyond 100° is situated below the curve of expansion, showing the air-thermometer to be behind the mercury. If the *absolute* amount of this difference in the two thermometers continued to have the same ratio C B to C D above 100° as below, the *relative* amount would be the same, and thermometers of different kinds of glass would keep pace with each other.

§ 9. The evidence on this point is rather conflicting. M. Regnault states (*Ann. de Chim.* vol. v. p. 102) that the different kinds of glass not only have coefficients of dilatation absolutely different, but they all follow different laws in the dilatation. The glasses which have the *smallest* coefficient appear to experience a *less* rapid augmentation of dilatation with the temperature. This is precisely what is required to admit of thermometers constructed of different kinds of glass keeping pace with each other. On the other hand, M. Pierre states (*Ann. de Chim.* vol. v. p. 438) that the thermometer with crystal reservoir is constantly in advance upon the thermometer with common glass reservoir, and the difference increasing generally with

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* *Ann. de Chim.* vol. xv. p. 335.

the temperature, may extend to several degrees while the two are being heated to 250° or 300°. Another adverse statement advanced by M. Pierre, is that exactly the same coefficient of dilatation for the same glass apparatus is not always found, although the circumstances are apparently identical. If such uncertainty is the normal state of thermometric measurement, how are we to account for the results shown in fig. 2? In the face of them, it seems impossible to accept the comparative thermometric observations of MM. Pierre and Regnault as conclusive.

§ 10. Besides the difference in the march of the air- and mercury-thermometers, there must be a difference in the value of the individual degrees marked on the two thermometers. Employing the formula [Appendix, III.] to obtain an approximate value of the mercury degrees in terms of the degrees of the air-thermometer, we find that at 0° the value of a degree, or $\frac{1}{100}$th of the distance between the freezing and boiling temperatures, is $\frac{24}{23}$ds the value at 100°. At the lower part of the scale, the value exceeds the mean by $\frac{1}{48}$th part, and at the higher point it is $\frac{1}{48}$th part less than the mean. This inequality may at times have a sensible effect on the quantitative determinations of physicists. Thus the mechanical equivalent of heat is, according to Mr. Joule, 772 for 1° F., at temperature about 60° F. If this were reduced to the standard value of a degree of the air-thermometer, we should have to reduce it by $\frac{1}{60}$th part, viz. to 759.

§ 11. There is another point referred to by M. Regnault as a source of uncertainty in measurements by the air-thermometer. The deviation from the primary laws of elastic fluids which he discovered must have some effect in disturbing that ratio of equality between the increments of volume and temperature, or of pressure and temperature, upon which the value of the instrument depends.

The air-thermometer may indicate temperature by change of volume under constant pressure, or by change of pressure with volume constant. M. Regnault's observations make the proportionate change of volume slightly larger than the change of pressure between the standard points of the thermometer; the difference being perhaps within the limits of error.

With increased densities these proportionate changes sensibly augment at the rate of about $\frac{1}{261}$ per atmosphere, and Messrs. Joule and Thomson's experiments with plugs, indicate a deviation from the law of Mariotte of about $\frac{1}{272}$ per atmosphere at ordinary temperatures, and a probability that this amount increases in descending the scale of temperature. [See paper on Deviation from the Primary Laws of Elastic Fluids, Phil. Mag., Oct. 1857.] The deviation consists in the elastic force being *less* than the inverse of the density by $\frac{1}{272}$ per atmosphere at ordinary temperatures. How the measurement of temperature is likely to be affected by these, we may perhaps appreciate most distinctly by means of a diagram (fig. 5).

§ 12. Let CQ represent the interval of temperature between the freezing and boiling of water, CD and QP the corresponding volumes of air under constant pressure at those temperatures. Join PD, and extend it to meet CQ produced in A. We have (PQ−CD) : CD : : CQ : CA, or according to Regnault, 0·36706 : 1·00000 : : 100° : 272°·44.

Suppose the temperature to descend from C to B, the corresponding volume would be BF if the primary laws of gases were entire. But the deviation, although as yet not perfectly known in a case such as this, points to a lesser volume BE as that which the thermometer would actually exhibit under the influence of the augmented density and diminished temperature, both of which unite in affecting the relation between tension and density.

§ 13. The locus of the point E is unknown, but it must be curvilinear, as both D and P are to be found in it; and its curvature is convex upwards, showing that between any two points D and P the air-thermometer is in advance of true temperature. As the amount of this difference depends on the degree of curvature, it may be worth while to inquire as to its probable maximum between 0° and 100°. Certainty is of course not to be expected, but I think it can be shown that the complete solution of the question is possible by employing the mode of experiment with plugs, suggested by Professor Thomson, and carried out to a certain extent by that gentleman and Mr. Joule under the auspices of the Royal Society.

§ 14. Suppose the air that occupies the volume DC is compressed into half the space (dC); we know, if it passed through a plug and were allowed to expand from the greater to the lesser density without performing work, that a thermal depression will be exhibited which indicates that the pressure at the double density is *less* than two atmospheres by about $\frac{1}{272}$. (See paper in Philosophical Magazine above referred to.) If dC represents the volume at two atmospheres, we should have the point d situated $\frac{1}{272} \times \frac{\text{DC}}{2}$ below the middle point between D and C. We have now to suppose the air represented by DC to pass through a plug into air of half an atmosphere tension, *i. e.* to have its volume changed to CD_1. The absolute amount of the deviation is $\frac{1}{2} \times \frac{1}{272}$, that is, CD_1 exceeds twice CD by $\frac{1}{544} \times CD_1$, or $\frac{1}{272} \times CD$. In the same way CD_2 exceeds twice CD_1 by $\frac{1}{4 \times 272} \times CD_2$, or $\frac{1}{272} \times CD$; and CD_n exceeds twice CD_{n-1} by $\frac{1}{2^n \times 272} \times CD_n$, or $\frac{1}{272} \times CD$. Put $\rho = \frac{1}{272}$ and $CD = 1$, we have

$$\begin{aligned} CD_1 &= 2 + \rho, \\ CD_2 &= 2CD_1 + \rho = 4 + 3\rho, \\ CD_3 &= 8 + 7\rho, \\ CD_n &= 2^n + \rho(2^n - 1) = (1 + \rho)2^n, \end{aligned}$$

when n is indefinitely great. If we now return from CD_n with a gas that strictly conforms to the law of Mariotte, and suppose the volume to be diminished until the tension of one atmosphere is regained, it is evident that at this point the volume, instead of being $CD = 1$, is $C\delta = (1 + \rho) = 1 + \frac{1}{272}$; and generally for any pressure m atmospheres at this temperature, the ratio of *non-deviation* to *deviation* volumes is $\frac{1 + m\rho}{1}$, and the *absolute* value of the difference of these volumes in terms of volume unity at one atmosphere is $\frac{1}{272}$, a constant absolute magnitude*.

§ 15. Thus far, while confining our view to one temperature, we require to make no assumption in advance of the results strictly derived from Messrs. Thomson and Joule's experiments, but the influence of a change of temperature has not as yet been distinctly made out.

In passing carbonic acid through plugs at temperatures up to nearly 100°, the thermal effect seemed to vary in the inverse ratio of the square of (AQ) the G temperature. (Phil. Trans. 1854, p. 336.)†

If this is the case with one gas, it is probably a general law applicable at least approximately to all. Assuming it to apply

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* This result is dependent on the assumption that the thermal effect is, at constant temperature, proportional to the difference of pressures.

† This law is indicated by Prof. Thomson's equation at p. 337, Phil. Trans. 1854, which is constructed on a formula of Mr. Rankine. *This formula is palpably erroneous, nevertheless Prof. Thomson's computed results agree exactly with experiment!* See paper on Deviation referred to in § 11.

to air, it is easy to compute $Q\pi$, and thence the asymptote $\pi\delta H$, and the equation of the curve EDP.

The degree of curvature assigned to it by this law of temperature, and the consequent aberration of the air-thermometer, are probably in excess of the truth. The inquiry may thus serve to mark the extreme limit of the disturbing influence of the deviation from the primary laws of elastic fluids on the air-thermometer. By an extension of the experiments with plugs, there is little doubt that sufficient data could be obtained for computing it exactly between 0° and 100°.

§ 16. In the paper above referred to, I have shown that the deviation from Mariotte's law follows a ratio compounded of the thermal effect directly, and the G temperature inversely; hence the thermal effect being assumed to vary inversely as the square of the G temperature, the deviation must follow the inverse cube of the G temperature thus,

$$\frac{\delta D}{CD} : \frac{\pi P}{PQ} = \frac{1}{CD^3} : \frac{1}{PQ^3},$$

or

$$\delta D : \pi P = \frac{1}{CD^2} : \frac{1}{PQ^2} = \frac{1}{C\delta^2} : \frac{1}{\pi Q^2} = \frac{1}{CH^2} : \frac{1}{QH^2} \text{ nearly.}$$

We thus obtain the position of π. Next, by joining π and δ and producing to meet the axis of temperature in H, we determine the *non-deviation* zero of gaseous tension, which thus computed is 1°·80 below A, the *deviation* zero derived from the expansion of air between 0° and 100°.

We thus fix the ratio HC to AC, and the equation of the curve is found by assuming $E\beta$ to follow the ratio of the temperature given above, viz. $\left(\frac{HC}{HB}\right)^2$. The equation for the locus of E, the curve EDP, thus determined is

$$(E\beta) = x = \delta D \cdot \frac{HC^2}{y^2} = \frac{1}{272} \cdot \frac{HC^2}{y^2},$$

in which y is the temperature reckoned from the non-deviation zero H, and x the portion of the ordinate to y intercepted between the asymptote $H\pi$ and the curve.

§ 17. We have now obtained the means of computing the versed sine of arch DP, and thence the correction required upon the air-thermometer at 50°.

The value of $M\mu$ thus computed is .	·002594
The mean between $D\delta$ and $P\pi$ is . .	·002821
The difference is	·000227

which is the versed sine at 50°. Comparing this with ·36706, which represents 100°, we deduce 0°·0618, or about $\frac{1}{16}$th as the fraction of a degree which the air-thermometer is in *advance* of true temperature at 50°.

At 200°, by a similar computation, it is $\frac{1}{4}$th of a degree *behind* the true temperature.

§ 18. It will be remarked that the differences are of contrary signs to the differences between the mercury- and air-thermometers, and that between 0° and 200°; a reduction of $\frac{1}{8}$th in the correction of the mercury-thermometers derived from the formula [Appendix, III.] would compensate almost exactly for this slight aberration from uniform expansion.

Appendix.—I.

This expressed by symbols is $\sqrt{\frac{e}{t}}$ [=AE, fig. 4], in which e is the tension and t the temperature [=CE], by air-thermometer reckoned from [C] the zero of gaseous tension.

The ranging of the points in a straight line or gradient is shown by the quotient of the difference of ordinates by difference of abscissæ being constant: thus $\left[\frac{\mathrm{CF-CE}}{\mathrm{BF-AE}}\right]=\frac{t_1-t}{\sqrt[6]{\frac{e_1}{t_1}}-\sqrt[6]{\frac{e}{t}}}=h=$ a constant ratio = cotangent of the inclination of the gradient to the axis of temperature $\left[=\frac{\mathrm{AD}}{\mathrm{BD}},\text{ fig. 4}\right]$.

The value of t_0 when $e=0$ is thus $t_0=t_1-h\sqrt[6]{\frac{e_1}{t_1}}=g=$ the temperature of the zero of tension of aqueous vapour [=CG].

These constants, h and g, may thus be obtained from two observations of tension, and the general equation for the tension in terms of the temperature is

$$e=\left\{\frac{t-g}{h}\right\}^6 t.$$

Assuming AS (fig. 2) to be the true gradient of steam, we have

$$\left.\begin{array}{r} g=197^\circ{\cdot}05 \\ \log h=2{\cdot}1962446 \end{array}\right\}\text{French measures,}$$

$$\left.\begin{array}{r} g=354^\circ{\cdot}7 \\ \log h=2{\cdot}7281741 \end{array}\right\}\text{English measures.}$$

This gradient coincides nearly with the observations of Dr. Ure and the American Institute at high pressures, but it is too high for those of the French Academy; also for those of M. Regnault above 100°. These last were taken by a process different from that employed for the observations below 100°. On examining them minutely I find that they are not continuous; there is a dislocation at

100°, which can only be accounted for by some fault in the observations. Take the tension at 111°·74, as given by M. Regnault, and compute its projection on fig. 2. The cotangent is 1·65406 (see Table, *ante*), and the difference ·08285. This gives T as the position of this point (fig. 2), and P Q R S T as the line of M. Regnault's observations. It will be remarked that the law of continuity is manifestly broken at S, the critical point at which the *mode* of observation underwent a change. Compare R S T with Q R S, or F S H with E F S; also the angle ρ R S with s S R.

II.

When the gradients radiate from the same point, it is evident that in the general equation $e=\left\{\frac{t-g}{h}\right\}^6 t$, the value of g is the same for all. Hence comparing the densities of two vapours thus related, it is obvious that at the same temperature they must vary inversely as the sixth power of h: also the tensions must be in this proportion. Thus between pure alcohol and aqueous vapour the ratio at the same temperature will be found to be 2·3192.

When two gradients are parallel, the value of g differs by a constant amount, and h is the same for both; hence on comparing the densities of two vapours thus related, we find equal densities not at the same temperatures, but at the same constant interval of temperature $(g-g')$. As an example, the vapour of sulphuric æther taken at 69°·55 below the temperature at which the density of the vapour of water may happen to have been observed, will be found at all parts of the scale to have $4\frac{1}{9}$ times its specific gravity, being the relative proportion of their chemical equivalents. This applies to Dalton's observations only.

III.

This formula is

$$t_m=\frac{Bt_a}{A-t_a}-\left(\frac{t_a}{D}+\frac{t_a{}^3}{C^3}\right),$$

in which

t_a is temperature by air-thermometer,
t_m temperature by mercury-thermometer,
$\log B=3·7145723$ $\log D=0·78587$
$\log A=4539°·617$ $\log C^3=6·43303$.

The data upon which it is constructed are taken from the original memoir of MM. Dulong and Petit, "On the Expansion of Mercury and Standard of Temperature" (*Ann. de Chim.* vol. vii. 1817, p. 136). They are as follows:—

Between 0° and 100° the rate of expansion of mercury is $\frac{1}{5550}$.

Between 0° and 200° (by air-thermometer) the rate is . $\frac{1}{5425}$.

Between 0° and 300° (by air-thermometer) the rate is . $\frac{1}{5300}$.

The denominators of these coefficients being in arithmetical progression, indicates that the curve of expansion approximates closely to a hyperbola, with the axes of coordinates parallel to the asymptotes. We find, accordingly, by drawing that curve through the coordinate points at 0°, 100°, and 200°, that it will be found also to pass within $\frac{1}{500}$th of a degree of the point at 300°.

The data as to the expansion of mercury in glass are as follows:—

Between 0° and 100° the rate of expansion is $\frac{1}{6480}$.

Between 0° and 200° „ „ $\frac{1}{6378}$.

Between 0° and 300° „ „ $\frac{1}{6318}$.

From these we obtain the following volumes of glass:—

0°	1·000000
100°	1·002586
200°	1·005508
300°	1·009120

By considering the volume to increase, partly as the temperature and partly as the cube of the temperature, we may represent the curve of expansion exactly by an equation of the form

$$\frac{t}{\mathrm{D}}-\frac{t^3}{\mathrm{C}^3}=v-1.$$

By combining this with the equation for the hyperbola in which mercury expands, the above formula for t_m in terms of t_a was constructed.

The original equation for the hyperbola is

$$\overline{a-t}\,\overline{v-b}=c^2,$$

in which the constants a, b, and c^2 are determined from three observations. When $t=0$, we have $v=1$, and the equation becomes $a\overline{1-b}=c^2$. Let $m=$ absolute increase of volume from 0°; then $v-m=1$, $v-b=1+m-b$, and $c^2=\overline{a-t}(1+m-b)$, and

$$m=\frac{c^2-\overline{1-b}\,(a-t)}{a-t}=\frac{\overline{1-b}\,t}{a-t}.$$

In the formula, B represents $1-b$, and A represents a.

The numerical values of B and A are fixed, because t_m coincides with t_a at 100°.

The expansibility of the glass corresponds with the maximum observed by M. Pierre (*Ann. de Chim.* vol. xv. p. 335). The minimum is about three-fourths the maximum. If we adjust the formula to the minimum expansion of glass, and compute the value of the correction at 50°, we shall find that it does not sensibly differ from the correction with thermometer glass of maximum expansion, the amount of difference being less than $\frac{1}{100}$th of a degree.

The alteration of the formula to conform to a change in the quality of the glass requires a change in the first term, which contains the part due to the expansion of mercury, as well as the two last terms, which contain the part due to the expansion of glass.

Let r = the ratio in which the coefficient of glass has to be reduced, and x the ratio of the change that has to be made simultaneously in the first term. We have at 100°, $t_m = t_a$; and by division,

$$\frac{Bx}{A-100}-\left(\frac{10000}{C^3}+\frac{1}{D}\right)r-1=0;$$

hence

$$x=\frac{A-100}{B}\left\{1+r\left(\frac{10000}{C^3}+\frac{1}{D}\right)\right\}.$$

The coefficient of glass being taken at M. Pierre's minimum, fixes $r=\frac{3}{4}$, and $\log x = 9{\cdot}9841437$, and the formula becomes

$$x\frac{Bt_a}{A-t_a}-\left(\frac{t_a^3}{C^3}+\frac{t_a}{D}\right)r=t_m.$$

If we now compute the difference between this thermometer and the former, we shall find that it is $\frac{1}{100}$th of a degree behind at 50°, and 1°·52 in advance at 300°.

This seems to prove that the influence of a difference in the quality of the glass of which thermometers are composed can have no sensible effect on the graduated difference brought out by the projection (fig. 2).

In conformity with what M. Regnault remarks as to glasses with the lesser coefficient experiencing a less rapid augmentation of dilatation with the temperature, it is probable that the term involving the cube should be changed to a lower power of the temperature when the coefficient $\frac{1}{D}$ is diminished.

22 London Street, Edinburgh,
January 7, 1858.

J. J. Waterston.

XXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 152.]

June 18, 1857.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"Remarks on a New Class of Alcohols." (Second Note.) By A. W. Hofmann, LL.D., F.R.S., and Auguste Cahours, F.C.S.

In a former communication (Phil. Mag. Oct. 1856, p. 309), we endeavoured to delineate the characters of a new alcohol—the Allylic alcohol, which is the prototype of a new class of alcohols. We have since continued these researches in order to complete the history of this remarkable compound.

In submitting to the Royal Society the full account of our experiments upon the subject, we beg leave to mention in this abstract briefly some additional compounds which we have examined since our last communication.

Sulphide of Allyle (Garlic Oil), $C_{12}H_{10}S_2$.—When iodide of allyle is allowed to fall drop by drop into a concentrated alcoholic solution of protosulphide of potassium, a very energetic action ensues, the liquid becomes very hot, and an abundant crystalline deposit takes place of iodide of potassium. It is important that the iodide of allyle should only be added gradually to avoid spirting, by which a part of the product would be lost. As soon as the action ceases, the liquid is mixed with a slight excess of sulphide of potassium; addition of water now separates a light yellowish limpid oil possessing a strong smell of garlic. When rectified, this liquid becomes colourless, it boils at 140° C., and gives with nitrate of silver a crystalline precipitate soluble in a boiling mixture of alcohol and water, and separating from this solvent in the form of beautiful white needles, absolutely identical with those which the natural essence of garlic produces. Sulphide of allyle also comports itself with corrosive sublimate and with bichloride of platinum exactly like the natural garlic oil.

Allyle-Mercaptan, $C_6H_6S_2$.—When in the process for the preparation of the preceding compound the protosulphide of potassium is replaced by the hydrosulphate of sulphide of potassium, a volatile product is obtained, having a similar but more ethereal odour. This substance acts with energy upon protoxide of mercury, with which it forms a compound, dissolving in boiling alcohol, and separating from it on cooling in the form of pearly scales of remarkable brilliancy, which present the greatest resemblance to the mercaptide of mercury. The liquid obtained in the above-mentioned reaction boils at 90°, and possesses a composition and an aggregate of properties which closely resemble those of the mercaptan of the ethyle-series, being, in fact, the allylic mercaptan, $C_6H_6S_2$.

Allylamine, C_6H_7N.—By boiling cyanate of allyle ($C_8H_5NO_2$) with a concentrated solution of caustic potassa until the oily layer entirely disappears, a complete decomposition ensues, and the residue contains only a mixture of carbonate of potassium with an excess of the alkaline hydrate; the volatile products condensed in hydrochloric acid furnish a saline mass which may be obtained crystalline on evaporation. This substance is always a mixture, the constituents of which vary both in nature and properties. The action of potassa on it not only separates a base, which is readily soluble in water, but also insoluble basic oils, the boiling-point of which rises to 180° C. Simple distillation of the liberated bases appears to give rise to decompositions, and thus to induce further complication. We have not succeeded in ascertaining with perfect precision the nature of this mixture, but we have found that it invariably contains a considerable quantity of a base which bears the same relation to allylic alcohol that is observed between ethylamine and ordinary alcohol. The formation of this substance, which we propose to designate allylamine, C_6H_7N, is perfectly analogous to the production of ethylamine by means of cyanate of ethyle.

Diallylamine, $C_{12}H_{11}N$.—Impure allylamine, obtained by the action of potassa on the cyanate, when digested with a second quantity of iodide of allyl is rapidly changed into a mass of hydriodates of new compounds. This crystalline mass evidently contains a considerable quantity of the hydriodate of diallylamine, but to succeed in separating it from this very complicated mixture would have required a more considerable quantity of substance than we had at our disposal.

Triallylamine, $C_{18} H_{15} N$.—The oxide of tetrallylammonium, to which we shall presently allude, submitted to the action of heat, is decomposed, with liberation of a basic oil. The perfect analogy of the oxide of tetrallylammonium with the corresponding ethyl-compound, leaves no doubt respecting the nature of this basic substance. It is obviously triallylamine. When saturated with hydrochloric acid and mixed with bichloride of platinum, it deposits a yellow platinum-salt, the analysis of which leads to the formula

$$\left.\begin{array}{l} C_6 H_5 \\ C_6 H_5 \\ C_6 H_5 \end{array}\right\} N, H Cl, Pt Cl_2.$$

Oxide of Tetrallylammonium, $C_{24} H_{20} NO, HO$.—The chief product of the action of ammonia upon iodide of allyl is a magnificent crystalline compound, which is deposited from the solution resulting from the reaction. Iodide of allyl is rapidly attacked even by an aqueous solution of ammonia at the common temperature. By a few days' contact, a large quantity of the iodide dissolves, and the solution becomes a solid mass. If no deposition of crystals take place from the solution, it is only necessary to add a concentrated solution of caustic potassa, which causes the separation of an oily layer that speedily solidifies. The crystals thus formed are the iodide of tetrallylammonium, which, like the corresponding compound of the ethyl-series, is but slightly soluble in solution of potassa. It is obtained pure by exposing it to the air until the potassa is converted into carbonate, and then recrystallizing it from absolute alcohol.

The iodide, by treatment with oxide of silver, is immediately transformed into the oxide. This forms an alkaline solution, which possesses all the properties of the oxide of tetrethylammonium.

The iodide contains

$$\left.\begin{array}{l} C_6 H_5 \\ C_6 H_5 \\ C_6 H_5 \\ C_6 H_5 \end{array}\right\} N I.$$

On taking a retropective glance at the results obtained in this inquiry, it is obvious that propylene, a homologue of olefiant gas, is susceptible of furnishing a mono-acid alcohol, which bears to it the same relation that is observed between methyl-alcohol and marsh-gas, or between benzoic alcohol and toluol, the analogue of marsh-gas among the derivatives of the aromatic acids. The hydrocarbons homologous and analogous to marsh-gas, the homologues of olefiant gas, and indeed probably a very considerable number of other hydrocarbons, may be regarded as starting-points for the production of mono-acid alcohols and of all their derivatives. All these alcohols are formed by the fixation of two equivalents of oxygen, which oxidation, however, cannot be effected directly, but is accomplished by a series of substitution-processes.

$$\underbrace{C_2\,H_4}_{\text{Marsh-gas.}} + O_2 = \underbrace{C_2\,H_4\,O_2}_{\text{Methylic alcohol.}},$$

$$\underbrace{C_{14}\,H_8}_{\text{Toluol.}} + O_2 = \underbrace{C_{14}\,H_8\,O_2}_{\text{Benzoylic alcohol.}},$$

$$\underbrace{C_6\,H_6}_{\text{Propylene.}} + O_2 = \underbrace{C_6\,H_6\,O_2}_{\text{Allylic alcohol.}},$$

But olefiant gas and its homologues are capable of furnishing mono-acid alcohols by another reaction, which has not yet been applied to the series of bodies homologous to marsh-gas. In fact, we have learnt by the beautiful researches of M. Berthelot that olefiant gas and its homologues are capable of being transformed into alcohols by the absorption of two equivalents of water; olefiant gas furnishes by this reaction ordinary alcohol, and propylene, propylic alcohol; thus—

$$\underbrace{C_4\,H_4}_{\text{Olefiant gas.}} + 2HO = \underbrace{C_4\,H_6\,O_2}_{\text{Ethylic alcohol.}},$$

$$\underbrace{C_6\,H_6}_{\text{Propylene.}} + 2HO = \underbrace{C_6\,H_8\,O_2}_{\text{Propylic alcohol.}}.$$

Olefiant gas, then, and all its homologues are capable of furnishing *two* mono-acid alcohols, exhibiting slightly different features, but presenting in all their principal characters the most obvious analogy. The alcohols formed by fixation of water, such as ethylic alcohol, have long been well known; in fact, the detailed study which has been made during the last quarter of a century of most of their derivatives, has exercised the most beneficial influence upon the development of organic chemistry.

The same remark does not apply to the alcohols produced from these same hydrocarbons by fixation of oxygen. The history of allylic alcohol, which we have endeavoured to trace in the preceding pages, and to which of late important contributions have been furnished, moreover, by MM. Berthelot and De Luca and by M. Zinin, begins to throw light also upon this second group of alcohols. For this reason the discovery of allylic alcohol appears to claim the attention of chemists, even if it had not assisted in fixing, in the system of organic compounds, the exact position of several very interesting natural products, viz. the sulphuretted oils furnished by the bulbs of the garlic (*Allium sativum*) and the seeds of the black mustard (*Sinapis nigra*), which it had not been possible up to the present time to group around a similar centre.

"Photochemical Researches." By Prof. Bunsen and Henry E. Roscoe, B.A., Ph.D. 3rd Communication. "The Optical and Chemical Extinction of the Chemical Rays."

In order to determine whether the act of photochemical combination necessitates the production of a certain amount of mechanical effect, for which an equivalent quantity of light is expended, or whether this phenomenon is dependent upon a restoration of equilibrium effected without any corresponding equivalent loss of light, we must now study the phenomena occurring at the bounding surfaces, and in the interior of a medium exposed to the chemically active rays.

If I_0 represents the amount of light entering a medium, and I the amount issuing from the medium, we have $\alpha I_0 = I$, when α represents the fraction of the original amount of light which passes through the medium, on the supposition that the light extinguished is proportional to the original intensity of the light. The first series of experiments was made with the view of determining this point. The intensity of the chemical rays proceeding from a constant source of light was measured before and after passage through a cylinder with plate-glass ends, filled with dry chlorine. The amount of transmitted light, I, was determined for various intensities of incident light, I_0, and the fraction $\frac{I}{I_0}$ was found to remain constant, proving that the absorption of the chemical rays varies directly as the intensity of the light. From this result, the general law of the extinction of the optical and chemical rays in transparent media may be deduced. For, as it has been shown that the amount of light transmitted through a medium of finite thickness is proportional to the intensity of the incident light, it may be assumed that this same relation will hold good for an infinitely thin medium. According to this supposition, the relation between the transmitted light, I, and the thickness of the medium, is represented by the equation $I = I_0 . 10^{-h\alpha}$ and $\alpha = \frac{1}{h} \log \left(\frac{I_0}{I}\right)$, in which I_0 represents the light before transmission, I, that after transmission through a layer of h thickness, and $\frac{1}{\alpha}$ the thickness of absorbing medium by passing through which the amount of light has diminished to $\frac{1}{10}$th.

The difference between the incident and transmitted light, *i. e.* that lost in passing through the medium, is made up (1) of a portion reflected, and (2) of a portion absorbed or extinguished. We have experimentally determined the values of the coefficient of reflexion ζ, and the coefficient of extinction α, for the glass plates used in our cylinders. We found that 4·86 per cent. of the chemical rays, from a flame of coal-gas, which fall perpendicularly on a surface of crown glass, are lost by the first reflexion; and that the amount of light absorbed in our plates was so small as to fall within the limits of observational errors. The value of ρ for the plates of glass employed was found to be 0·0509. When the coefficient of reflexion for glass ρ is known, the amount of light α transmitted by n plates is found from the formula $\frac{1-\rho}{1+(2n-1)\rho}=a$. Hence the amount of light transmitted by two plates is 0·823. We have confirmed the accuracy of the calculated result by direct experiment, and obtained a value 0·800, or a mean of 0·811 as the coefficient of transmission of our plates.

If all the transparent media have not the same coefficient of reflexion, the order in which the media are placed will affect the amount of transmitted light. We have given an example of the mode in which the calculation must in this case be made, in the determination of the coefficient of extinction of water. We found that the amount of light absorbed by a column of water 80 millimetres thick was inappreciable. According to the method here adopted, it is possible to determine the coefficient of reflexion of all transparent fluids for the chemical rays. We have only determined the coefficient of reflexion for American mica; for the chemical rays of a coal-gas flame ρ was found to be $=0·1017$. From the coefficient of reflexion, the refractive index (i) can be calculated from the equation $\rho=\left(\frac{1-i}{1+i}\right)^2$ or $i=\frac{1-\sqrt{\rho}}{1+\sqrt{\rho}}$. The refractive index for crown glass thus calculated from $\rho=0·0509$ is found to be $i=1·583$; the refractive index for Fraunhofer's line H has been optically determined to be between 1·5466 and 1·5794 (Buff's Physik).

Another important element in the investigation of photochemical extinction is the law according to which the optical coefficient of extinction varies with the density of the absorbing medium. A series of experiments proved that the amount of chemical rays transmitted through a medium varies proportionally with the density of the absorbing medium.

We may now proceed to the investigation of the original question proposed, viz.—in the combination of chlorine and hydrogen effected by the light, are the chemical rays expended in a relation proportional to the quantity of hydrochloric acid formed? The first point to be determined, in order to answer this question, is the coefficient of extinction of pure chlorine for the chemical rays of a coal-gas flame. The amount of light was measured before and after transmission through cylinders filled with chlorine. The loss of light by reflexion $a=0·811$ must be deducted from the incident light, and then the coefficient of extinction for chlorine is calculated. From a series of determinations, the value of $\frac{1}{a}$, *i. e.* the depth of chlorine at 0° C. and 0·76 pressure, through which the light must pass in order to be reduced to $\frac{1}{10}$, is found to be, as a mean of five experiments, 171·7 mm. Another series of determinations were made with chlorine diluted with air, in order to prove experimentally that the absorbed light varies in the case of chlorine directly as the density. The quantity of chlorine contained was determined in each instance by a volumetric analysis. An average of six experiments gave a value for $\frac{1}{a}=174·3$ mm. As a mean of these two series of experiments we have a value of 173·3.

If the light is not consumed in the act of photochemical change, the coefficient just found must remain unaltered when the chlorine and hydrogen mixture is employed; if on the contrary light is not only lost by the optical extinction, but an amount of light vanishes proportional to the chemical action, experiment must give a larger value for the coefficient.

In order to determine this important question we employed an apparatus (fully described in the Paper), by means of which we could expose columns of the sensitive gas of various lengths, to a constant source of light. By determining the amount of action effected in these columns of varying length, we are able to obtain the value of the coefficient of extinction for the sensitive mixture. A series of experiments showed that when the light had passed through 234 millimetres of the sensitive mixture of chlorine and hydrogen at 0° and 0·76, it was reduced to $\frac{1}{10}$ of its original intensity. If, instead of hydrogen, we had diluted the chlorine with some other transparent but chemically inactive gas, the depth to which the rays must penetrate in order to be reduced to $\frac{1}{10}$ is according to the experiments with pure chlorine, 346 millimetres. Hence it is seen that for a given amount of chemical action effected in the mixture of chlorine and hydrogen, an equivalent quantity of light is absorbed. For we find that in the case of the standard chlorine and hydrogen mixture, where, together with the optical absorption, a chemical action of the light occurs, the value of the coefficient of extinction is 0·00427; whereas in the chlorine mixture of similar dilution, where the chemical action was absent, the coefficient is found to be 0·00289, or very much smaller.

It appeared of great interest to repeat these experiments with rays from other luminous sources. For this purpose we employed the diffuse light of morning reflected from the zenith of a cloudless sky. The experiments gave a value of $\frac{1}{\beta}$ of 45·6 mm. for chlorine. That is, diffuse morning light was reduced to $\frac{1}{10}$ by passing through 45·6 millimetres of chlorine. A series of experiments made with our apparatus and the standard mixture gave a depth of 73·5 millimetres of chlorine and hydrogen before the light was reduced to $\frac{1}{10}$. From this it is seen that morning zenith light is much more easily absorbed by chlorine than lamp-light. Hence it was conjectured that the diffuse solar light might differ in its properties in this respect with the time of day or year. Experiment fully confirmed this supposition. Observations made with evening light proved that a depth of 19·7 mm. of chlorine was sufficient to reduce the light to $\frac{1}{10}$ of its original amount, and that 57·4 mm. of the standard mixture was required to effect the same end.

The conclusion which we draw from all our observations is, that the coefficients of extinction of pure chlorine for chemical rays from various sources of light are very different. The depth to which such light must penetrate chlorine at 0° and 0·76, in order to be reduced to $\frac{1}{10}$ of its original intensity, is—

(1.)	For a flame of coal-gas	173·3 mm.
(2.)	Reflected zenith light, morning	45·6 mm.
(3.)	Reflected zenith light, evening	19·7 mm.

Hence it is seen that the chemical rays reflected at different times and hours not only possess quantitative but also qualitative differences, similar to the various coloured rays of the visible spectrum. Had nature endowed us with the power of discriminating the chemical rays, as we do the visible ones, by impressions of varying colour, we should see the rosy tints of morning pass in the course of the day through all the gradations of colour until the warm evening ones at length succeed.

A long and continued series of observations must be made before we are able to appreciate the influence which these qualitative differences in the chemical rays exert upon the photochemical phenomena of vegetation. That this influence must be of the greatest importance is evident from the varying effects produced in other photochemical processes by differences in the solar light. We need only mention in proof of this assertion, the fact, well known to all photographers, that the amount of light, photometrically speaking, gives no measure for the time in which a given photochemical effect is produced, and that a less intense morning light is always preferred for the preparation of pictures to a bright evening light.

GEOLOGICAL SOCIETY.

[Continued from p. 156.]

January 20, 1858.—Major-Gen. Portlock, President, in the Chair.

The following communications were read :—

1. " On the Evolution of Ammonia from Volcanos." By Charles Daubeny, M.D., F.R.S., F.G.S., &c.

The author referred to the existence of a chemical compound of titanium with nitrogen, known from the researches of Wöhler and Rose ; and pointed out in this paper its bearing on one part of the theory of Volcanos, namely, the evolution of ammonia, and the consequent presence of ammoniacal salts amongst the products of their operations. Dr. Daubeny first commented on the hypotheses already suggested by Bischoff and Bunsen, to account for the volcanic production of ammonia: viz. 1st, the decomposition of carbonaceous or other organic substances ; 2ndly, the conversion, by the hot lava overflowing the herbage, of the nitrogenized matter present in the latter into ammonia, and the combination of this with the muriatic acid in the lava giving rise to the sublimation of sal-ammoniac. To both of these hypotheses the author pointed out serious objections. He had himself proposed to account for the presence of ammonia in volcanic outbursts by assuming that the gaseous hydrogen, although incapable of combining with nitrogen under ordinary pressures, might unite with it under that exercised upon it in the interior of the earth ; and he still believes this idea to be worthy of consideration, though perhaps it is impracticable to secure by experiment the conditions necessary for the chemical union of these two gases.

The affinity, however, which certain metals possess for nitrogen seems to afford more solid grounds on which to build a theory respecting the production of ammonia. Titanium has been found, by MM. Wöhler and St. Claire Deville, to absorb nitrogen from the air: and the union of heated titanic acid with nitrogen (forming a nitride of the metal) takes place, indeed, with so much energy as to generate light and heat; and thus constitutes a genuine case of combustion, in which nitrogen, and not oxygen, acts as the supporter.

Although titanium is evidently present to some extent in most volcanos, the author is not disposed to think that it abounds sufficiently to account for the large quantities of sal ammoniac that are known to occur; but, rather, he argues by analogy that probably not only titanium but other metals, such as iron, and probably even hydrogen, may combine directly with nitrogen in the interior of the globe, under conditions of great pressure and other circumstances likely to modify the nature of those reactions which take place under our eyes.

In a postscript the author adverted to the recently discovered fact, that boron, like titanium, has the property of combining directly with the nitrogen of the air, and that the compound which it forms with it also possesses the property of evolving ammonia under the influence of the alkaline hydrates.

2. "On the Granites of Ireland: Part II. The Granite of the North-east of Ireland." By the Rev. Prof. S. Haughton, F.G.S.

This communication was a continuation of the details of the author's researches in the granites of Ireland. Part I. was published in the 12th volume of the Journal. In this paper Mr. Haughton first treated of the potash-granites; giving their analyses and atomic quotients; and, secondly, described the soda-granites in like manner; drawing the following conclusions from the examination:—1st, that both in Leinster and the County Down, the potash-granites are more constant in composition, both mineralogical and chemical, than the corresponding soda-granites; 2ndly, that the potash-granites appear to be the standard type of granite, from which other granites and crystalline rocks are formed (as more or less overlying and superficial coatings) by the addition of bases; for example, the anorthite-syenite of Carlingford, the soda-granites of Newry, and in Leinster the outlying patches of granite between the main chain and the sea; 3rdly, that the potash-granite of Leinster is more persistent in external character than the potash-granites of Newry, although the latter are equally constant in chemical composition.

3. "On the Classification of the Palæozoic Strata of the State of New York." By Dr. J. J. Bigsby, F.G.S.

In the synoptical view of the strata and fossils of the palæozoic basin of New York, read before the Society Nov. 18, 1857, the author desired to arrange the vast stores of information contained in the Official Reports of the State Geologists of New York in a methodical and accessible form; and in the present communication he treats succinctly of the stratigraphical arrangements hitherto used, and the classification now adopted by himself. This was printed in the abstract of the author's former paper (Phil. Mag. Jan. 1858, p. 76): it is but little modified from that proposed by De Verneuil; and is mainly characterized by the union of certain sections of the series into natural groups, A, B, C, &c., and by the establishment of a distinct middle Silurian stage, and an equally distant middle Devonian stage. The lithological and palæontological characters of the several groups of strata are then treated of in succession; their resemblances and differences, in these respects, being carefully noted. From the consideration of the stratigraphical details contained in this and the preceding paper, Dr. Bigsby deduces two main conclusions, namely, 1. That from the Potsdam sandstone to the summit of the Carboniferous rocks, these strata were laid down in comparative quiet; subject to occasional, vertical, variable, secular oscillations, which led to considerable superficial changes. 2. That their elevation, foldings, fractures, and metamorphism were effected after the deposition of the whole, and in a single prolonged transaction, principally in a N.E. and N.W. direction, along the present Appalachian ridges and their continuation from Labrador to near the Gulf of Mexico. The evidences on which these two propositions rest were next detailed; and the views of the Professors Rogers on these points, and the author's objections, were stated in full.

XXX. *Intelligence and Miscellaneous Articles.*

ON THE INFLUENCE OF MUSICAL SOUNDS ON THE FLAME OF A JET OF COAL-GAS. BY JOHN LECONTE, M.D.

A SHORT time after reading Prof. John Tyndall's excellent article "On the Sounds produced by the Combustion of Gases in Tubes*," I happened to be one of a party of eight persons assembled after tea for the purpose of enjoying a private musical entertainment. Three instruments were employed in the performance of several of the grand trios of Beethoven, namely, the piano, violin, and violoncello. Two '*fish-tail*' gas-burners projected from the brick wall near the piano. Both of them burnt with remarkable steadiness, the windows being closed and the air of the room being very calm. Nevertheless it was evident that *one* of them was under a pressure nearly sufficient to make it *flare*.

Soon after the music commenced, I observed that the flame of the last-mentioned burner exhibited pulsations in height which were *exactly synchronous* with the audible beats. This phænomenon was very striking to every one in the room, and especially so when the strong notes of the violoncello came in. It was exceedingly interesting to observe how perfectly even the *trills* of this instrument were reflected on the sheet of flame. *A deaf man might have seen the harmony*. As the evening advanced, and the diminished consumption of gas in the ctiy *increased the pressure*, the phænomenon became more conspicuous. The *jumping* of the flame gradually increased, became somewhat irregular, and finally it began to flare continuously, emitting the characteristic sound indicating the escape of a greater amount of gas than could be properly consumed. I then ascertained by experiment, that the phænomenon *did not* take place

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* Phil. Mag. S. 4. vol. xiii. p. 473, 1857.

unless the discharge of gas was so regulated that the flame approximated the condition of *flaring*. I likewise determined by experiment, that the effects *were not* produced by jarring or shaking the floor and walls of the room by means of repeated concussions. Hence it is obvious that the pulsations of the flame *were not* owing to *indirect* vibrations propagated through the medium of the walls of the room to the burning apparatus, but must have been produced by the *direct* influence of the aërial sonorous pulses on the burning jet.

In the experiments of M. Schaffgotsch and Prof. J. Tyndall, it is evident that "the shaking of the singing flame within the glass tube," produced by the voice or the syrene, was a phænomenon perfectly analogous to what took place under my observation *without the intervention of a tube*. In my case, the discharge of gas was so regulated that there was a tendency in the flame to flare, or to emit a "*singing sound*." Under these circumstances, strong aërial pulsations occurring at *regular intervals* were sufficient to develope synchronous fluctuations in the height of the flame. It is probable that the effects would be more striking when the tones of the musical instrument are *nearly* in unison with the sounds which would be produced by the flame under the slight increase in the rapidity of discharge of gas required to manifest the phænomenon of flaring. This point might be submitted to an experimental test.

As in Prof. Tyndall's experiments on the jet of gas burning within a tube, clapping of the hands, shouting, &c. were ineffectual in converting the "silent" into the "singing flame," so in the case under consideration, *irregular* sounds did not produce any perceptible influence. It seems to be necessary that the impulses should *accumulate* in order to exercise an appreciable effect.

With regard to the mode in which the sounds are produced by the combustion of gases in tubes, it is universally admitted that the explanation given by Prof. Faraday in 1818 is essentially correct. It is well known that he referred these sounds to the successive explosions produced by the periodic combination of the atmospheric oxygen with the issuing jet of gas. While reading Prof. J. Plateau's admirable researches (third series) on the "Theory of the modifications experienced by Jets of Liquid issuing from circular orifices when exposed to the influence of Vibratory Motions*," the idea flashed across my mind that the phænomenon which had fallen under my observation was nothing more than a *particular case* of the effects of sounds on *all kinds of fluid jets*. Subsequent reflection has only served to fortify this first impression.

The beautiful investigations of Felix Savart on the influence of sounds on jets of water, afford results presenting so many points of analogy with their effects on the jet of burning gas, that it may be well to inquire whether both of them may be referred to a common cause. In order to place this in a striking light, I shall subjoin some of the results of Savart's experiments. Vertically descending jets of water receive the following modifications under the influence of vibrations:—

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* Phil. Mag. S. 4. vol. xiv. p. 1 *et seq.*, July 1857.

1. The continuous portions become shortened; the vein resolves itself into separate drops nearer the orifice than when *not* under the influence of vibrations.

2. Each of the masses, as they detach themselves from the extremity of the continuous part, becomes flattened alternately in a vertical and horizontal direction, presenting to the eye, under the influence of their translatory motion, regularly disposed series of maxima and minima of thickness, or ventral segments and nodes.

3. The foregoing modifications become much more developed and regular when a note, in unison with that which would be produced by the shock of the discontinuous part of the jet against a stretched membrane, is sounded in its neighbourhood. The continuous part becomes considerably shortened, and the ventral segments are enlarged.

4. When the note of the instrument is *almost* in unison, the continuous part of the jet is alternately lengthened and shortened, and the beats which coincide with these variations in length *can be recognized by the ear.*

5. Other tones act with less energy on the jet, and some produce no sensible effect.

When a jet is made to ascend *obliquely*, so that the discontinuous part appears scattered into a kind of *sheaf* in the same vertical plane, M. Savart found,—

a. That under the influence of vibrations of a determinate period, this sheaf may form itself into *two* distinct jets, each possessing regularly disposed ventral segments and nodes; sometimes, with a different note, the sheaf becomes replaced by *three* jets.

b. The note which produces the greatest shortening of the continuous part, always reduces the whole to a *single* jet, presenting a perfectly regular system of ventral segments and nodes.

In the last memoir of M. Savart—a posthumous one, presented to the Academy of Sciences of Paris by M. Arago in 1853*,—several remarkable acoustic phænomena are noticed in relation to the musical tones produced by the efflux of liquids through short tubes. When certain precautions and conditions are observed (which are minutely detailed by this able experimentalist), the discharge of the liquid gives rise to a succession of musical tones of great intensity and of a peculiar quality, somewhat analogous to that of the human voice. That these notes were not produced by the descending drops of the liquid vein, was proved by permitting it to discharge itself into a vessel of water, while the orifice was below the surface of the latter. In this case the jet of liquid must have been *continuous*, but nevertheless the notes were produced. These unexpected results have been entirely confirmed by the more recent experiments of Prof. Tyndall†.

According to the researches of M. Plateau, all the phænomena of the influence of vibrations on jets of liquid are referable to the conflict between the vibrations and the *forces of figure* ("*forces*

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* *Comptes Rendus* for August 1853. Also Phil. Mag. S. 4. vol. vii. p. 186, 1854.

† Phil. Mag. S. 4. vol. viii. p. 74, 1854.

figuratrices"). If the physical fact is admitted—and it seems to be indisputable—that a liquid cylinder attains a *limit of stability* when the proportion between its length and its diameter is in the ratio of 22 to 7, it is almost a *physical necessity* that the jet should assume the constitution indicated by the observations of Savart. It likewise seems highly probable that a liquid jet, while in a transition stage to discontinuous drops, should be exceedingly sensitive to the influence of all kinds of vibrations. It must be confessed, however, that Plateau's beautiful and coherent theory does not appear to embrace Savart's last experiment, in which the musical tones were produced by a jet of water issuing under the surface of the same liquid. It is rather difficult to imagine what agency the "forces of figure" could have, under such circumstances, in the production of the phænomenon. This curious experiment tends to corroborate Savart's original idea, that the vibrations which produce the sounds must take place in the glass reservoir itself, and that the cause must be inherent in the phænomenon of the flow.

To apply the principles of Plateau's theory to gaseous jets, we are compelled to abandon the idea of the *non-existence of molecular cohesion in gases.* But is there not abundant evidence to show that cohesion *does exist* among the particles of gaseous masses? Does not the deviation from rigorous accuracy, both in the law of Mariotte and of Gay-Lussac,—especially in the case of condensable gases, as shown by the admirable experiments of M. Regnault,—clearly prove that the hypothesis of the non-existence of cohesion in aëriform bodies is fallacious? Do not the expanding rings which ascend when a bubble of phosphuretted hydrogen takes fire in the air, indicate the existence of some cohesive force in the gaseous product of combustion (aqueous vapour), whose outlines are marked by the opake phosphoric acid? In short, does not the very *form* of the flame of a "fish-tail" burner demonstrate that cohesion *must exist* among the particles of the issuing gas? It is well known, that in this burner the single jet which issues is formed by the union of *two oblique jets* immediately before the gas is emitted. The result is a perpendicular *sheet of flame.* How is such a result produced by the mutual action of two jets, unless the force of cohesion is brought into play? Is it not obvious that such a fan-like flame must be produced by the same causes as those varied and beautiful forms of aqueous sheets developed by the mutual action of jets of water, so strikingly exhibited in the experiments of Savart and of Magnus?

If it be granted that gases possess molecular cohesion, it seems to be physically certain that jets of gas must be subject to the same laws as those of liquid. Vibratory movements excited in the neighbourhood, ought therefore to produce modifications in them analogous to those recorded by M. Savart in relation to jets of water. Flame or incandescent gas presents gaseous matter in a *visible* form admirably adapted for experimental investigation; and *when produced by a jet,* should be amenable to the principles of Plateau's theory. According to this view, the pulsations or *beats* which I observed in the gas-flame when under the influence of musical sounds, are produced by the conflict between the aërial vibrations and the "forces of figure" (as Plateau calls them), giving origin to periodical fluctuations of intensity, depending on the sonorous pulses.

If this view is correct, will it not be necessary for us to modify our ideas in relation to the agency of tubes in developing musical sounds by means of burning jets of gas? Must we not look upon all burning jets,—as in the case of water-jets,—as *musically inclined*; and that the use of tubes merely places them in a condition favourable for developing the tones? It is well known that burning jets frequently emit a *singing sound* when they are perfectly *free*. Are these sounds produced by successive explosions analogous to those which take place in glass tubes? It is very certain, that under the influence of molecular forces, any cause which tends to elongate the flame, without affecting the velocity of discharge, must tend to render it discontinuous, and thus bring about that mixture of gas and air which is essential to the production of the explosions. The influence of tubes, as well as of aërial vibrations, in establishing this condition of things is sufficiently obvious. Was not the "beaded line" with its succession of "luminous stars," which Prof. Tyndall observed when a flame of olefiant gas burning in a tube was examined by means of a moving mirror, an indication that the flame became *discontinuous*, precisely as the continuous part of a jet of water becomes *shortened*, and resolved into isolated drops, under the influence of sonorous pulsations? But I forbear enlarging on this very interesting subject, inasmuch as the accomplished physicist last named has promised to examine it at a future period. In the hands of so sagacious a philosopher, we may anticipate a most searching investigation of the phænomena in all their relations. In the mean time, I wish to call the attention of men of science to the view presented in this article, in so far as it groups together several classes of phænomena under one head, and may be considered a partial generalization.—*From* Silliman's *American Journal for* January 1858.

ON A NEW SOURCE OF ELECTRICAL EXCITATION.

BY MRS. ELISHA FOOTE.

I have ascertained that the compression or the expansion of atmospheric air produces an electrical excitation. So far as I am aware, this has not been before observed, and it seems to me to have an important bearing on the explanation of several atmospheric and electrical phænomena.

The apparatus used was an ordinary air-pump of rather feeble power, and adapted either to compress or exhaust the air. Its receiver was a glass tube about 22 inches in height and 3 in diameter, with its ends closed by brass caps cemented to it. At the bottom was a stopcock and a screw by which it was attached to the air-pump. To the top were soldered two copper wires, one hanging down within the tube, terminating in one or more points, and extending to within about 6 inches of the bottom, the other extending from the upper side of the cap to an ordinary electrical condenser.

In experimenting after compressing or exhausting the air within the receiver, the wire reaching to the condenser was disconnected from it. The upper plate was lifted from its place by its glass handle, and its electrical condition tested by a gold-leaf electrometer. I have found it convenient first to compress the air and close the stopcock, when the condenser would be found to be charged with positive electricity; then after discharging all traces of it both from the condenser and the wire leading to it, the air was allowed to escape, and the condenser would become recharged to an equal extent.

My experiments with this apparatus have extended over about eight months, and I have found the action to bear a strong analogy to that of the electrical machine. In damp or warm weather little or no effect would be produced, whilst at other times, particularly in clear cold weather, the action would be so strong as to diverge the leaves of the electrometer to their utmost extent. In warm weather, when no action would be produced, I have attained the result by cooling the air artificially. A sudden expansion or contraction always increases the effect.

The results with oxygen gas were similar, but I was not successful with either hydrogen or carbonic acid gases.

It is believed that the results which have been obtained on a small scale in my experiments may be traced in the great operations of nature. The fluctuations of our atmosphere produce compressions and expansions sufficient to cause great electrical disturbances. Particularly should this be observed in the dry cold regions of our atmosphere above the effects of moisture and vapours; and it was established by the experiments of Becquerel as well as those of Gay-Lussac and Biot, that the electricity of the atmosphere increases in strength with the altitude.

A manifest relation, moreover, between the electricity of the atmosphere and the oscillations of the barometer has frequently been observed. Humboldt, treating upon the subject in his 'Cosmos,' remarks, among other things, that the electricity of the atmosphere, whether considered in the lower or the upper strata of the clouds in its silent problematical diurnal course, or in the explosion of the lightning and thunder of the tempest, appears to stand in a manifold relation to the pressure of the atmosphere and its disturbances.

The tidal movements of our atmosphere produce regular systematic compressions twice in twenty-four hours. These occur with so much regularity within the tropics, as observed by Humboldt, that the time of day is indicated within fifteen or twenty minutes by the state of the barometer. And Saussure observed a diurnal change in the electricity of the atmosphere corresponding with the diurnal changes of the barometer. The electricity of the atmosphere, he observes, has therefore a daily period like the sea, increasing and decreasing twice in twenty-four hours. It, generally speaking, reaches its maximum intensity a few hours after sunrise and sunset, and descends again to its minimum before the rising and setting of that luminary.

Other phænomena, which it is believed may be traced to the same cause, will be the subject of another communication.—*From* Silliman's *American Journal for* November 1857.

THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

APRIL 1858.

XXXI. *On the Forms of Clouds.*

By W. S. Jevons, *Assayer, Sydney Branch of the Royal Mint**.

[With a Plate.]

1. In the Philosophical Magazine for July 1857, will be found a paper "On the Cirrous Form of Cloud," in which I attempted to show, from a simple experiment, that cirri are produced by the *interfiltration of bodies of moist air,* merely differing in temperature. Simple differences of specific gravity, and motions thence resulting, might, however, be said to be the actual cause of the cirrus. Having since followed out the same mode of investigation into more complicated and perhaps more interesting cases, I have now to publish the results to which I have been led concerning the cumulus, the cumulostratus, and the nimbus or thunder-cloud. It will be well, however, first of all, to state distinctly the nature of the evidence and arguments which I shall adduce.

2. Now the means by which I take the first step to explain the form of a cloud, is to produce experimentally with liquids a *miniature representation* of it, under conditions in which the immediate causes can be certainly known. If it is found that given portions of liquids of ascertained specific gravities, when placed together or variously set in motion upon each other, produce peculiar appearances which exactly resemble in form some of the more distinct kinds of clouds, I assume with complete confidence that similar motions and differences of specific gravity have operated in the production of the atmospheric clouds in question.

It must be distinctly understood, that in this first step we are dealing only with dynamical causes, that is, with simple force

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* Communicated by the Author.

and motion. Liquids are in many respects very unlike gases, the latter being chiefly distinguished by the property of elasticity; but in the atmosphere this property cannot be *directly* productive of force or motion, or even the modification of force and motion, because the air, being only confined by the superincumbent air, is always at perfect freedom to assume the density and elastic force due to the pressure of that air. Elasticity is, as it were, always *self-adjusting*, and never called into play, so that free air will resemble in its motions a very rare liquid, and any part of the atmosphere will be subject to the same hydrodynamical laws as the interior of a body of liquid.

3. But assuming that we understand the mere motions which take place in the production of a cloud, it is quite another thing to show whence the forces which produced those motions are derived. In our miniature experiments such conditions are known beforehand, because we have prepared our liquids of different specific gravities by dissolving various weighed quantities of some heavy soluble substance in water; and we can project the solutions thus prepared into each other with any desirable velocity or direction by the aid of a simple apparatus to be presently described. But to explain, according to the known complex properties of the atmosphere, the origin of those differences of specific gravity and of those motions which we have established to exist in the formation of a cloud, is a distinct and more difficult part of the subject.

4. It is necessary, so to speak, to *translate* the conditions of our liquid experiments into the language of *air, water and heat*, which are the only *elements* actively concerned in the production of meteorological phænomena. In fact, every complicated change which heat, itself only a mode or disguised form of force, may occasion in air or water, and every disturbing effect which air and water,—the latter in no less than three distinct forms, the solid, liquid, and gaseous,—may mutually have upon each other, will have to be taken into account before we can lay down the *vera causa* of an atmospheric cloud as distinctly as we can announce the conditions of our miniature one.

My experimental arrangements and results may be briefly explained and easily understood, and my readers must judge for themselves whether the representation which they afford of the internal motions of clouds be satisfactory. But even if so, it will still be open to them to accept or dispute the suggestions, founded upon less simple principles, which I shall offer concerning the causes of those motions.

The Section-glass.

5. The apparatus which I employ may be conveniently termed

a *section-glass,* because it contains a thin section of liquid which is supposed to represent a section of the atmosphere. It consists of two sheets of plate-glass (about 18 inches by 14), which being let into two plain wooden frames, can be so screwed together, face to face, as to form a water-tight vessel with an internal space of the uniform width of about one-third of an inch.

This being filled with water, we have a thin layer or section of liquid, of which the minutest motions or changes can be conveniently detected and observed either by means of the small particles of sediment floating in it, or by the production of a precipitate of chloride of silver, of which the component parts, silver and chlorine, are contained in distinct portions of the liquid.

One or more glass tubes may be introduced in any desirable positions into the section-glass through grooves cut in the opposite faces of the wooden frame, and numerous currents of liquid, varying in velocity, volume, or quantity, direction, temperature, density, &c., may thus be introduced, so that their complex motions and reactions may be observed in the most convenient manner. It is very easy to render the whole water-tight, if a thin continued band of common glazier's putty be laid round between the level surfaces of the wooden frames before they are screwed together, the tubes required for the experiment in preparation being likewise imbedded in the same. One of the glass plates has been faintly engraved, by means of hydrofluoric acid, with two series of rectangular lines at the uniform distance of one inch; these afford in every part a fixed line of reference to detect the slightest movements of the enclosed liquid, as well as to measure the rate or extent of that movement if required.

The accessory apparatus, such as tubes, funnels, stopcocks, &c., will at once be understood from Plate II. fig. 1, in which the instrument is arranged as it is chiefly employed.

6. The liquids used consist of the following dilute solutions, viz.—

(1) Water containing 1 part in 5000 of common salt.
(2) Water containing 1 part in 5000 of nitrate of silver.
(3) An aqueous solution containing one-tenth of its weight of ordinary white sugar.

The first two have about the same specific gravity, but produce, when they mix, a white precipitate of chloride of silver, which, though perfectly distinct, is so thin and impalpable as to remain a long time suspended in the water, the motions of which its weight does not perceptibly influence. Known quantities of the third solution are, however, added to the two former, with the object of producing such differences of specific gravity as form the required conditions of the experiment.

7. A little reflection will show that the whole variety of phæ-

nomena which may be produced by the experimental arrangements described above, result from only two *essential variable* conditions.

1st. Original impressed *momentum.*

2nd. *Gravity,* which is called into play whenever we use two liquids differing, however slightly, in specific gravity, but which does not *in the least affect the internal motions of a perfectly homogeneous fluid.*

Though we employ certain chemical salts and also liquids differing in temperature, it will easily be seen, and must be borne in mind, that these are merely *devices* for rendering the motions of the liquids visible, or for altering their specific gravities.

8. In the use of the section-glass, many curious facts have been observed concerning the movements of jets and streams in the interior of a body of liquid, which this is not the place to describe, although the great variety of gracefully curved lines, and of curious and complicated forms which are presented, give a lively interest to the experiments. In a few of the more distinct cases, I hope to show that they lead us to the understanding of analogous atmospheric movements on a much grander and more important scale.

Cumulus.

9. The general nature and principles of the cumulus have been understood for some time past; but as the explanations which are given of it in meteorological works are, I believe, more or less erroneous, and always incomplete, I must here state what occurs to me concerning it, before proceeding to the more complex cases of the cumulostratus and nimbus.

10. The pure cumulus is a simple, roundish, or hemispherical heap of cloud matter, extending itself upwards from a horizontal base, and appearing mysteriously suspended quite apart from all other clouds at a moderate distance from the surface of the earth. In this pure form it occurs only during the day; and this fact leads to the supposition, well supported by the observation of its general form, and the curious internal motions which it often exhibits, that it is occasioned by upward currents of warm and moist air originating at the surface of the earth.

11. When a body of air rises in the atmosphere, the continual diminution of the superincumbent pressure allows it to expand, and to decrease in density at about the same rate as obtains in every other part of the atmosphere. Expansion occasions the increase of the capacity for heat, and there is consequently an absorption of sensible heat. The rising air must therefore sink in temperature as it swells in volume; but because a corresponding expansion of volume and diminution of density takes place

in every other part of the atmosphere, a uniform corresponding fall of temperature must also be everywhere found, supposing no extraneous disturbing influences to exist. Thus the walls of air which surround the ascending column, and were cold with regard to it at the surface, must suffer in every part of its course an equal diminution of temperature proportional to the height at which we test it. In fact, however high the column of air ascend, it will fall continuously in temperature, but still remain warm with respect to the surrounding air on the same level. Its motion, supposing it not to become broken up and dispersed, might thus be perpetuated up to the limiting surface of the atmosphere but for unforeseen causes, or perhaps for the interference of the contained aqueous vapour.

12. It is evident that the aqueous vapour, which is always *entangled* in greater or less quantity with the ascending air, must experience exactly corresponding changes of volume and density. In proportion as the air expands, therefore must the aqueous vapour diminish in *actual* elastic force. [The *actual elastic force* is a term used in distinction from the *possible elastic force,* by which is meant the greatest tension, according to the usual tables, that can be supported at any given temperature.] But since the temperature of a rising body of air falls at some uniform rate, or at all events proportionally to the expansion, while the *possible* elastic force of vapour decreases in some ratio much more rapid than the temperature, it follows that the possible will always, sooner or later, overtake the actual elastic force of the contained aqueous vapour. If the upward motion of the air be continued ever so little above the point at which this takes place, the actual elastic force can be no longer maintained, but condensation must ensue and a cloud be produced.

13. It now becomes interesting to inquire whether or not the ascending current will be continued to any considerable height above that "*plane of precipitation*" at which, as just explained, condensation of aqueous vapour commences. As yet we have found no reason (11) why the motion should not be continued up to the surface of the atmosphere, while it is commonly stated that the large amount of latent heat given out in condensation tends even to increase the buoyancy and "perpetuate" the upward movement (Graham's 'Elements,' p. 328).

Yet it is evident that the uprising column of air is actually terminated, for the reason that the *cumulus,* as continually occurring in the atmosphere, is always of a moderate elevation, and terminates in a sharply defined spherical head. To judge solely from the appearance of the cloud itself, we should at once say that just above that level where condensation of vapour commences, the rising column is suddenly checked, flows over in a fountain-like head, and then sinks on every side beneath the level of precipitation, all watery particles then evaporating.

14. The general form and internal motions of the cumulus may, indeed, be illustrated with some approach to accuracy by a simple experiment in the section-glass; by producing, in short, a fountain-like jet of liquid, of which the gravity is reduced to an exceedingly small amount relatively to the surrounding liquid.

Experiment 1. Fill the section-glass with water containing only a trace of common salt (1 part in 5000), and when it has come to rest, introduce from the centre of the lower side a slow vertical current of distilled water, of the same (ordinary) temperature, and containing a trace of nitrate of silver, together with 1 part in 5000 of its weight of sugar in solution.

The specific gravity of the injected liquid is very slightly greater than that of the remainder; and by the effect of gravity acting against momentum, is produced the regular and extremely graceful fountain-like form shown in fig. 2.

15. Either by considering the hemispherical form of the natural cumulus, or the analogy which this experiment presents, it is surely evident that a rising column of air, instead of being perpetuated, is completely checked a short height above the plane of precipitation. It is not necessary to the further progress of my theory, that I should point out the *whole* cause of this. I notice here only one point which seems to have been overlooked by meteorologists, viz. that the weight of the precipitated watery particles must increase the specific gravity of a cloud as a whole.

In condensation, a certain portion of gaseous vapour, which possesses only six-tenths the specific gravity of common air, is reduced to liquid water, which is 815 times as dense as air. It is true that this water is in the form of minute suspended particles or vesicles, which are not at rest but must always tend to subside; yet it does not follow that their weight must not be added to the weight of the air to obtain the total weight, and thence the specific gravity of the whole mass. Indeed it follows from the almost self-evident proposition, that the weight of a body will not be affected by motions merely internal and relative, that the suspended particles of water in a cloud must increase its total specific gravity.

16. While, therefore, the heat given out in the precipitation of watery particles tends to expand the air and diminish its density, a counter effect is to some extent produced by the weight of those particles. It is not possible to determine whether precipitation will really increase or decrease the density of the cloudy air, and accelerate or arrest its upward motion, except by exactly calculating out the amounts of the two opposite effects, and thus discovering in which direction the resultant tends. A

rough calculation which I have made leads to the conclusion that the effect of the latent heat considerably overbalances that of the heavy watery particles; but from the difficulty of obtaining satisfactory data as to latent and specific heats, I cannot here pretend to determine the point. It is sufficient if the following be distinctly understood and allowed*:—

1st. That in the formation of a cumulose cloud, *some cause* exists to check the rising column of air, and sooner or later to cause it to descend below the plane of precipitation.

2nd. That the weight of the precipitated watery particles must contribute in some degree to produce this effect.

Cumulostratus.

17. The cumulostratus, which I have seen aptly described as the *anvil-shaped cloud,* possesses a peculiar and distinctive form. It is a cumulus of which the higher parts or the summit extend horizontally into stratose projections, and is said to be the intermediate or transition form between the cumulus and the nimbus or storm-cloud.

18. The following are the conditions under which it was found that a miniature cloud, bearing a striking resemblance to the natural cumulostratus, could be produced in the section-glass (see figs. 3 and 4).

Exp. 2. Place in the section-glass a stratum of water containing only a trace of common salt, and then gradually introduce below it a second stratum containing the same trace of common salt, together with 2 parts in 1000 of sugar. The latter stratum, possessing the greatest specific gravity, will lie tranquilly below the first. Lastly, project up slowly from the lower side of the apparatus, a jet of water containing a trace of nitrate of silver, together with about 1 part in 1000 of sugar.

19. The last-mentioned liquid is of a specific gravity intermediate to the other two, and will ascend through the lower stratum and up to a certain elevation in the higher stratum. There its superior gravity will overcome its momentum, and it will

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* The following phænomenon which I noticed a few days since, presents such a rare and remarkable confirmation of what is advanced above, that I describe it here:—During a severe hot wind (December 24, 1857, temperature of air 104° to 108° F.), a large column of smoke (probably from a bush fire) was seen rising into the atmosphere in the neighbourhood of Sydney. After attaining a certain elevation, this was sharply terminated by a nearly level plane, beneath which the smoke seemed to spread out on all sides. But in several places small cumuli projected up above this plane, remaining in contact with it and appearing as if supported upon it. Here we have, in all probability, the plane of precipitation distinctly marked, and a proof that the ascending column of air could not permanently rise above it.

redescend, as in the cumulus, into the lower stratum. But here its downward motion will be again checked, so that in fact it cannot attain equilibrium except by spreading out in the form of a distinct layer or stratose extension intermediate to the other two strata. A cloud of chloride of silver is thus produced of which two of the varieties of form are shown in figs. 3 and 4, being drawn from photographs of the actual experiments.

20. Assuming that the resemblance of the miniature and real atmospheric cloud is sufficient ground, I can at once announce, from the conditions of the experiment, that the cumulostratus is a cumulus in which the tendency to redescend beneath the plane of precipitation does not exist. On the contrary, the air, after ascending and becoming cloudy, finds itself in equilibrium with the surrounding air, and spreads out in more or less elevated and horizontal projections. But I must remark, that the miniature cloud having once *suggested* the conditions of the real cloud, ceases to be the sole ground on which we accept these conditions as true. The mind having once received a clear and salient idea as to a probable cause of the cumulostratus, will apply it to every instance of that cloud which meets the eye in nature. This comparison is the test of the theory, and must be the main ground on which my readers may judge of the truth of my conclusions.

21. Having established the simple dynamical conditions of the cumulostratus, it yet remains, as before explained, to translate these conditions, and to show the ultimate physical causes of the cloud. One simple and apparently reasonable supposition is sufficient, viz. that a partial subsidence or separation of the watery cloud-particles takes place. The evident effect of this is to diminish the specific gravity of the remaining cloudy air. The latter was, in the first place, warmer than the surrounding air when it ascended, and became further warmed by the heat evolved in condensation. Its buoyancy, however, was destroyed, and its upward motion arrested above the plane of precipitation, by some causes to which the weight of the watery particles undoubtedly contributed in a greater or less degree. But when the watery particles fall away from the air, a part at least of this counteracting tendency is removed; the air becomes *permanently warmer* and *permanently lighter* than before, and will not have the same tendency to redescend beneath the plane of precipitation, as in the cumulus. Still retaining a part of the cloudy particles in suspension, it will seek a certain elevation at which it is in equilibrium with the surrounding air, and will there spread out in those stratiform projections which characterize the cumulostratus.

22. The separated watery particles may fall as rain; but this is not a necessary supposition, since many appearances indicate,

I believe, that subsidence may go on in the body of a cloud without their *aggregation* into rain-drops of sufficient size to descend rapidly through the air to the surface of the earth. Thus may be explained, I suspect, the cirrose appearance which Howard ('Climate of London,' vol. i. p. lxix) detected in the interior of quickly evaporating cumuli. I continually notice, too, that peculiar bag-like projections or "droplets" form at the under surface of a dense sheet of cirrostratus towards the latter part of the day, evidently caused by the weight of subsiding cloud-particles.

The Nimbus, Rain-cloud, or Thunder-cloud.

23. The nimbus or cumulo-cirrostratus is an organized combination of the simple forms, and is described by Howard as "a horizontal sheet, above which the cirrus spreads, while the cumulus enters it laterally and from beneath." The more distinct forms of thunder-cloud to which I now alone refer, more resemble a cumulostratus with a crest of cirrus spreading from the summit; and it occurred to me that it was only necessary to combine the conditions of the experiment (see Phil. Mag. for July 1857) by which the miniature cirrus was produced, with the conditions of the miniature cumulostratus, in order to produce an actual representation of the form of a thunder-cloud. The experiment is conducted as follows:—

24. *Exp.* 3. Pour into the section-glass a stratum of water which contains about 2 parts in 1000 of sugar, with a trace of common salt, and the temperature of which has been raised to about 100° F. Then carefully introduce beneath it a second stratum at the ordinary temperature (about 60°), and containing the same trace of common salt, together with about 2½ parts of sugar in 1000. Lastly, project upwards from the lower edge of the apparatus a jet of distilled water at the ordinary temperature, containing merely a trace of nitrate of silver in solution.

25. The jet of the last-named liquid forces its way rapidly up through the lowest cold and dense stratum, by virtue both of its momentum and its considerable buoyancy. The heated upper stratum, however, although containing sugar, is of less specific gravity than pure water at the temperature of 60°, and therefore, on entering this stratum, the upward progress of the jet is checked, and a cumulostratose form, similar to that of the last experiment, is the first result. But the heat of the uppermost stratum is rapidly communicated to the liquid with which it is in contact; and wherever there are two portions of liquid of the same temperature, and of which the upper contains sugar, while the lower is free from it, it is evident that these must change place from the mere effect of gravity.

Thus the injected liquid, instead of gradually subsiding into a longitudinal stratum, as its intermediate specific gravity causes it to do in the case of the cumulostratus, becomes by degrees of *least* specific gravity, and ascends through the superincumbent stratum in the form of small streams or threads. In my previous paper I attempted to show that these liquid threads are analogous to, and produced by the same simple cause as those atmospheric threads which are termed *cirri*. In the present experiment, then, we have succeeded in producing a cumulostratus of which the higher parts acquire a tendency to expand themselves upward as a *cirrose crest.*

The form of the miniature thunder-cloud thus obtained, under favourable circumstances, is shown in fig. 5, but for obvious reasons the cloud is always in a state of change, and does not endure for many minutes.

26. It will very probably be impossible for many of my readers to recognize in the peculiar result of the last experiment any resemblance whatever to the ordinary conception of a thunder-cloud. During the continuance of a storm, nothing of course is visible to a spectator beneath except one black unbroken mass of cloud. It is only to a very distant observer that the true shape and elevation of this cloud would be presented, and he probably sees nothing in it to merit his particular attention. Hence very few persons will be able, I think, to describe what the form of a thunder-cloud is. Moreover, under varying circumstances, a hundred thunder-clouds may be produced which will appear to a superficial observer to differ completely in form and nature, but in which closer examination may detect, in greater or less degree, all the essential characteristics of the perfect or typical thunder-cloud. To obtain, indeed, the type or single clear conception of a thunder-cloud, it is necessary for the mind to carry on a process of *abstraction* upon all thunder-clouds which meet the eye, and the result of such will be, I think, a form resembling in all essential particulars that produced in the third experiment.

The essential parts of a thunder-cloud appear to be three, viz. 1st, the cumulus, which is the centre and source of all; 2nd, stratus, or portions of cloud which extend themselves outwards and lie in equilibrium with the surrounding air; and 3rd, cirrus, which consists only of small quantities of moist air and cloud forcing their way upwards, by virtue of their buoyancy to higher elevations.

These, it will be seen, are exactly represented by the motions and forms of the injected liquid in the experiment just described.

27. There is one thing that it is so essential to understand with regard to this *miniature thunder-cloud,* that I repeat here what I before stated (7). It is produced by the simplest dyna-

mical causes, namely, the momentum which we communicate in the first place to the injected liquid, and the motions which afterwards arise from differences of specific gravity purposely occasioned. Supposing the analogy of this experimental cloud to the real atmospheric cloud to hold good, it follows that the latter is a subject to be treated on the simplest dynamical principles, while the conditions of our experiment actually indicating the proximate causes of the atmospheric phænomenon, afford us a new and safe guide to its ultimate causes. Leaving others, however, to subject the *miniature thunder-cloud,* or rather (see 20) the idea which is evolved in this, to a comparison with the actual phænomenon, and assuming on my own part that it will be favourable, I proceed to offer a suggestion as to the conversion of the experimental conditions into those of the atmosphere.

28. Having shown (in 20) how the subsidence or aggregation and rapid fall of watery particles within a cloud may cause the remaining air (still partially cloudy) to become *permanently* warmer and specifically lighter, so as to seek a definite elevation above the plane of precipitation, and there to spread out gradually in horizontal stratiform projections, let us suppose this action to have gone on to a certain extent within a cloud so as to produce the cumulostratus. The latter will consist of two parts, the active cumulose centre, and the tranquil bodies of cloud which are derived from and surround it. But if we now suppose subsidence and the formation of rain to continue throughout the cloud, it is evident that the specific gravity of the *stratose* or tranquil parts of the cloud will be still further decreased, but *gradually* decreased. Having assumed a given elevation, because there alone their specific gravity was equal to that of the surrounding air, the equilibrium of these stratose parts can now no longer be maintained. As they gradually decrease in specific gravity, the uppermost layers of air, which are most free from watery particles, must tend to ascend to greater elevations, and here we meet precisely the conditions in which, as shown in my previous paper, an upward *cirrose filtration* will take place. Small streams and threads of warm and moist air will ascend from the upper part of the cloud, and forcing their way into the superior strata, also moist, but of less temperature, renewed precipitation will undoubtedly take place according to the mixture theory of Dr. Hutton, and the characteristic crest of cirrus will be produced.

29. It is not possible within the narrow limits of this paper to answer any probable or apparent objections to this theory, nor to offer the many supporting arguments and instances which may occur to me. I cannot resist noticing, however, that the meteorologist Howard, in speaking of the nimbus or cumulo-cirro-

stratus, 'Climate of London,' i. p. xlix, says, "The cirri *vegetate*, as it were, in proportion to the quantity of rain falling." It would be impossible to express more accurately the relation of the subsiding cloud-particles and falling rain to the ascending cirri, than to speak of the latter as "vegetating."

I leave it for the reader to consider how various winds and many different meteorological conditions will alter the general shape and proportions of the thunder-cloud, and how the cirrose crest will generally be *swept away* by a lofty current of air so as to assume the form of a thin sheet or *cirrose veil*, often of an arched form, and preceding the main body of the storm-cloud.

Lastly, in proposing, as I think, a fresh and original theory of the thunder-cloud, I am not unaware of its crudeness. Whether by others or myself, a *quantitative* estimation must be made of the forces and velocities upon which it so entirely depends; having suggested that the form of the thunder-cloud is a simple mechanical phænomenon, it will remain a mere suggestion until it is presented as a rigorous mathematical theorem. The simplicity, however, of the principles involved, naturally inspires much confidence that this may be achieved.

Electrical Theory of the Thunder-cloud.

30. It may have caused some surprise that I have offered a theory of the thunder-cloud in which the principle, *electricity*, is not even referred to.

Thunder and lightning are indeed the most striking phænomena which the thunder-cloud presents, and partly on account of this, and partly from a propensity to form electrical theories which seems almost universal, it has always been usual to consider that electricity must be, in some way or other, the *cause of storms*. It is treated, in short, as the fourth element of meteorology, of which the first three are air, water, and the agent heat.

31. If, without assuming that the theories above propounded are exactly true, we only admit that they lie in the right direction, and appear to rest upon some sound basis, we can pretty plainly draw the inference that this supposed electric agency is a mere illusion. All the more distinct forms of clouds, cirrus, stratus, cumulus, cumulostratus, and thunder-cloud, have long been known, or have been shown according to my own theory, to be the simple mechanical effects of differences of specific gravity occasioned in air and water by the action of heat. Electricity is a force of quite a different nature, and incapable of cooperating with these in producing the class of mechanical effects in question. To introduce it is gratuitous and unnecessary. Though it is always usual in meteorological works to refer all difficult points concerning clouds to that delusive agent, no one

ventures definitely to state how electricity can produce or modify them.

When we have a prospect of assigning other simpler forces as the true causes of the forms of clouds, it is unphilosophical even to mention electricity in conjunction with these.

32. But, surely, meteorologists have strangely mistaken the true functions of electricity in ever proposing it as a *cause*. Sir J. Herschel expresses himself to this effect (Essays, &c., p. 245) :—" Here also we have to deal with electricity as a third element, but we strongly incline to the opinion, that its agency as a meteorological *cause*, is exceedingly limited, indeed, that it may be altogether left out of the account as productive of any meteorological effects of importance on the great scale."

Electricity has no place among the *principles* of meteorology; it is but an accidental or *secondary effect*, by which I mean one, as it were, of a different order from its cause, and which does not produce any appreciable reaction.

One example will illustrate this position. It is always stated in electrical treatises, that when the back of a common domestic cat is smartly rubbed, crackling sparks of electricity will be emitted. Friction is undoubtedly the cause, in this case, of the electricity; but would any one be foolish enough to suppose that electrical attraction was in any way the cause of your hand being brought near the cat's back and rubbed upon it?

The sparks are a secondary effect of the force expended in rubbing; and although they may be said to contain a minute amount of force under a very peculiar form, they can have no appreciable reaction. Similarly, the atmospheric sparks are a secondary effect of the friction of currents of air, but the mechanical force manifested in the whole discharges of a large thundercloud must be almost infinitely small compared with the force required to raise thousands of tons of water to the elevation of the clouds, and to stir up the winds till their pressure amounts, perhaps, to 10 lbs. on each square foot of opposing surface.

33. Yet it may be well if I can offer any suggestion to reconcile the *secondary effect* in question, with the simple and fundamental causes whch I believe to operate in clouds. Possessing, as I assume, a clear idea of the motions and changes which constitute a cumulus, and therefore the thunder-cloud, which is essentially a cumulus with a derivative and, as it were, organized system of cirrus and stratus, I see no difficulty in offering an explanation which is at least plausible. My only fear is that it is not original.

Atmospheric air almost always possesses a faint charge of electricity diffused through it, as the electric excitation is diffused over the surface of the glass cylinder of the electrical machine.

It is so perfectly insulated by the non-conducting property of gases, that only a single particle can be discharged at once. Now when this air rises into the body of a cumulose cloud, the precipitated watery particles being good conductors, will rapidly collect upon their surfaces all the electricity of the air in which they are immersed.

34. But it is characteristic of a rain- or storm-cloud that the watery particles become separated from the air; they sink, subside and coalesce, while the air, freed from their weight, rises into higher strata. Surely this air will also be deprived of its whole electric charge, which will collect upon the cloud as the electric fluid of the glass plate upon the metallic conductor. Now as the particles of the cloud gradually coalesce into large spherical drops, the total superficial conducting surface is vastly diminished, and the tension of the electricity is raised to such a pitch, that it must finally overcome the resistance of the non-conducting atmosphere and discharge itself with the earth which is in the opposite electric condition. And so long as a current of moist electrified air enters the cloud from beneath and passes out at the summit deprived of moisture and electricity, must the continually renewed tension of the electricity be relieved by repeated flashes of lightning.

35. It does not escape me that many apparent objections may at once be urged against this theory; for instance, the fact that rain is not always accompanied by lightning, and that in a thunder-storm capricious changes in the kind or sign of electricity often occur. With regard to these I will only remark, that though this theory does not *include* them, it does not, as far as I can see, *exclude* them: there is nothing apparently irreconcileable in them. I have attempted to show how the *cumulose precipitation* of rain may be accompanied by the concentration of electric force, but into the modifying conditions of the latter I do not now enter.

36. I conclude by subjoining, in as brief terms as possible, a statement of the conclusions arrived at in this paper, or the previous one of July 1857.

1. The cirrus is produced by the *interfiltration* of two moist bodies of air, of which the lowest possesses the highest temperature and the least specific gravity.

2. In the acknowledged mode of production of the cumulus by an ascending current of warm moist air, it has been usual to overlook some cause which operates to check the upward motion soon after precipitation has commenced; of this effect the weight of the precipitated watery particles forms at least partially the cause.

3. When partial subsidence or separation of the watery cloud-

particles takes place, the ascending body of air will not be so much checked, and having lost its tendency to redescend, as in the cumulus, will spread out in stratiform projections, and produce the cumulostratose form.

4. If subsidence and aggregation of the cloud-particles into rain continue and increase, the air throughout the cloud will be gradually relieved from the weight of the water, yet retaining the latent heat evolved in its condensation, will decrease in specific gravity. It will, therefore, commencing with the upmost parts, tend to ascend through the superincumbent strata, producing (1) the cirrose crest which is characteristic of the nimbus.

5. The electricity of a thunder-cloud is continuously derived from the weak charge previously existing in the ascending current which supplies the moisture. The precipitated watery particles collect this electricity, which, as they gradually aggregate into drops of rain, becomes intensified to an indefinite extent.

EXPLANATION OF PLATE II.

Fig. 1. Section-glass.
Fig. 2. Cumulus.
Figs. 3 and 4. Cumulostratus.
Fig. 5. Thunder-cloud.

The letters in the figures denote:

L. Liquid of *less* or *least* specific gravity compared with the other portions of liquid with which it is in contact.
G. Liquid of *greater* or *greatest* specific gravity.
I. Liquid *intermediate* in specific gravity to two other portions of liquid.
Incr. Liquid of which the specific gravity is *increasing*.
Decr. Liquid of which the specific gravity is *decreasing*.

N.B. Figs. 2, 3, 4, and 5 are engraved from photographs of the actual experiments.

Royal Mint, Sydney, New South Wales,
January 9, 1858.

XXXII. *A Description of a Contact Breaker superior to any hitherto made, and of certain Effects of a Condenser on the Action of various kinds of Contact Breakers. By the* Rev. N. J. Callan, *D.D., Professor of Natural Philosophy in Maynooth College**.

IN 1837 I constructed a contact breaker in which the contact was made and broken by a copper wire which was caused to vibrate very rapidly, so that its opposite ends which were amalgamated dipped alternately into mercury. A description of this instrument was published in Sturgeon's 'Annals of Electricity,' &c., in April 1837. It was the first contact breaker ever made; it was not self-acting. When the mercury is covered with oil, it is superior to all other non-self-acting contact breakers.

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* Communicated by the Author.

About two years ago M. Foucault described in the *Comptes Rendus* a contact breaker, in which the contact was made and broken by a platina point caused to dip into mercury and rise from it by means of an electro-magnet; the mercury was covered with alcohol. Soon after I had seen the description of this instrument, I made one which acted very well as long as I used only one voltaic cell; but when three cells were employed, the alcohol took fire. I mentioned this circumstance to M. Foucault at the meeting of the British Association in Dublin. He said that he used thirty cells without inflaming the alcohol; that in his contact breaker the alcohol over the mercury was about 2 inches deep, and that in mine the depth of the alcohol must have been too small. It was only about half an inch.

In an instrument of this kind, I have lately used platina, copper, amalgamated copper, silvered copper, tin, and iron for making and breaking contact by dipping into mercury and rising from it. The mercury was covered with oil, alcohol, or turpentine. The same battery and electro-magnet were employed to make these various metals vibrate rapidly. The amalgamated copper acted with far greater effect than platina or any of the other metals, but the contact was not made and broken so rapidly as when any of the other metals was used, because a small portion of mercury adhered to the amalgamated copper after it rose above the surface of the mercury. Hence, in order to break contact, it was necessary to make the amalgamated copper and the armature of the electro-magnet vibrate through a greater distance than that through which the other metals required to move. When the amalgamated copper was used, it was necessary to allow the armature to recede nearly $\frac{1}{8}$th of an inch from the poles of the electro-magnet. Hence the contact was not made and broken with great rapidity. This defect I have remedied by the use of a lever, the two arms of which are at right angles to each other, and one of which is about half an inch and the other 6 or 7 inches long. The short arm is attached to the armature by means of a double-jointed rod of brass or iron; and the long arm, which must be horizontal, to the amalgamated piece of copper. The electro-magnet is fixed to a frame in a horizontal position; the armature is screwed to a board moveable on a hinge, and standing nearly at right angles to the horizon. It is brought by means of a screw to the distance from the poles of the electro-magnet which is found to answer best. It is also pressed by a spring, or two springs, with a force just sufficient to prevent the attraction of the electro-magnet from drawing it to the magnet, and thus breaking connexion with the battery before the magnet and core of the induction coil receive the full magnetic power which the battery is capable of giving to them. If the armature be drawn to the poles of the electro-magnet through the $\frac{1}{96}$th of an inch, the amalgamated piece of copper, being at 6 inches from the centre of motion, will ascend $\frac{1}{8}$th of an inch, and will be raised out of the mercury even when the armature does not vibrate through a distance greater than the $\frac{1}{96}$th of an inch. The vibrations through so small a distance are very rapid, and consequently the connexion of the coil of the magnet, and of the primary wire of the induction coil, with the battery is made and broken with great rapidity.

I have used for the long arm of the lever a piece of thick sheet brass about 6 or 7 inches long and an inch broad. A piece $\frac{1}{4}$th of an inch broad and about 4 inches long may be cut out of the middle of the long arm, so that a thin plate of amalgamated copper, of platina, or of any other metal may be attached to the lever by a small screw and nut, at any distance we wish from the centre of motion. I have attached the amalgamated copper at distances varying from 3 to 10 inches from the centre of motion, and have found that it acts as well at 3 or 4 as at 10 inches from the centre. It is useless to make the length of the long arm greater than 6 or 7 inches. I have found that the use of the lever just described is a great improvement to the contact breaker, not only when amalgamated copper and mercury are used, but also when the contact is made by platina or any other metal and mercury, or by the pressure of two solid metals against each other.

In the mercurial contact breaker, I have used oil, alcohol and turpentine. I think the oil acts as well as either of the others, and has one advantage over both of them, that it does not evaporate. The others require to be renewed every day.

The magnetic power of the electro-magnet in the mercurial contact breaker, and the shock given by the secondary current of its helix, are much greater when amalgamated copper is used than when platina is employed, or than when the connexion with the battery is made and broken by two solid metals. Hence the contact breaker I have described, in which contact is made and broken by means of mercury and amalgamated copper, is superior to all other contact breakers. The reason is that amalgamated copper makes better contact with mercury than platina does, or than any two solid metals make with each other.

A piece of amalgamated tin plate acts as well as amalgamated copper. The part of the tin or copper which dips into the mercury need not exceed $\frac{1}{8}$th of an inch in breadth.

The mercurial contact breaker requires much less magnetic power to work it than other contact breakers; because, in order to make perfect contact between mercury and amalgamated copper, pressure is not necessary. I have found that for working this instrument, the magnetism of one pole of the core of an induction coil is abundantly sufficient. When only one pole of the core is presented to the armature, a piece of soft iron should be fixed near the end of the armature opposite to that on which the core acts.

It will be useful to divide the induction coil and its core into two or more parts, and to fix one end of each part of the core to a frame, so as to be about $\frac{1}{30}$th or $\frac{1}{25}$th of an inch from the armature. The ends of the core presented to the armature should be alternately north and south, and then the opposite ends of the parts of the core will be also alternately north and south, and may be connected with each other by a plate of iron, so that all the parts will act on the armature as one electro-magnet. The various parts of the core should project at least an inch beyond each extremity of the parts of the primary coil which surround them. Both ends of each part of the core should be filed flat. By connecting the ends of the parts of the core which are not presented to the armature by a plate of iron, the magnetic power of the core and its attraction for the armature, and consequently the action of the contact breaker, is increased; but the sudden magnetization and demagnetization of the core is rendered more difficult. I have used with the mercurial contact breaker, a small coil divided into two equal parts, and have found that the connexion of the ends with each other by an iron plate, produced scarcely any change in the length of the sparks given by the secondary coil. The sparks from the coil were longer when the contact breaker was used, than any I was able to get by making and breaking contact with the hand. The division of the induction coil and its core into two or more parts will be useful in long coils, because it diminishes the length of the wires in the core. The parts into which the core is divided need not be equal to each other in length.

When the ends of the coil of an electro-magnet by which a contact breaker is worked, are connected with a condenser, the armature is, if the battery be small, instantly made to vibrate more rapidly, and to strike against the poles of the magnet with increased force, unless the contact be made and broken by mercury and amalgamated copper, or some other amalgamated metal. Such are the effects which I have found to be produced by the condenser on the action of contact breakers in which the contact was made and broken by means of mercury and platina, or any unamalgamated metal, or by the pressure of two solid metals against each other. But these effects were sensible only when the battery employed was small. When a powerful battery was used, the condenser gave no sensible increase of power to the electro-magnet; nor did it, even when the battery was small, produce any sensible improvement in the action of the contact breaker if the connexion with the battery was made and broken by mercury and an amalgamated metal. This is an additional proof of the superiority of the contact breaker in which mercury and amalgamated copper are employed for making and breaking contact. For, the only reason we can conceive why the condenser should produce a sensible improvement in all contact breakers except the one in which mercury and an amalgamated metal are used, is, that in this, the amalgamated metal makes more perfect contact with mercury than platina does, or than two solid metals make with each other, and therefore produces greater magnetic power in the electro-magnet.

The increased action produced by the condenser in various contact breakers is a confirmation of the truth of the explanation of the action of the condenser which is given in the paper I read at the last meeting of the British Association, and which was published in the Philosophical Magazine for last November. In that paper I showed that the effects of the condenser are,—1st, to accelerate the motion of the electricity flowing in the primary coil at the moment its connexion with the battery is broken, and thereby increase the magnetic power of the core; and 2ndly, to destroy more suddenly the magnetism of the core by the electrical current which rushes from the positive to the negative plate of the condenser, as soon as the former receives all the electricity flowing in the primary coil at the moment its connexion with the battery is broken. These are precisely the effects which I have found the condenser to produce on the electro-magnet of the contact breaker.

I may mention here, that I have found by experiment that iron wire coiled round the primary wire of an induction coil resists the force with which electricity is attracted to the positive plate of the condenser after the connexion between the primary wire and battery is broken, and consequently that the immense quantity of iron wire wound round the conducting wire of the Atlantic cable must give great resistance to the electric current by which messages are to be sent across the Atlantic. Hence, if any other means can be employed to protect and strengthen the conducting wire, it should be adopted in the part of the cable which is not yet made, and the use of iron wire should be abandoned.

Maynooth College,
Feb. 27, 1858.

XXXIII. *On the Lakes and Rivers of Damascus.*

By Francis M. Jennings *of Cork, M.R.I.A., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE following remarks will, I trust, be considered as suggestive, and as offering to travellers what is most required in making preparations for a journey to foreign lands,—a statement of the points of interest in distant countries in connexion with questions of physical geography, geology, natural history, &c., showing what are still undecided, and what most require further research and examination.

The Rev. J. L. Porter, author of 'Five Years in Damascus,' was the first to give an accurate map of the vicinity of that famous city, and the rivers flowing through it,—the Barada, ancient Abana, and the Awaj, ancient Pharpar,—showing that they enter into two distinct and separate lakes, flooding a considerable district in winter, and diminishing again in the dry season. Now these lakes appear to have no outlet for the excess of waters, or if there be an outlet or outlets, they have escaped observation, and yet their waters are *fresh.* Further examination may possibly prove that a subterranean passage carries off a portion of the waters, or that they escape by watercourses into the Hauran. If, however, such means of escape for the excess of water do not exist, these lakes must be considered as exceptional, and an analysis of the water remote from the streams flowing into them would be of much interest.

The city of Damascus is about 2200 feet above the level of the sea, which gives facility for an escape of water not afforded by the Caspian, Sea of Aral, or Dead Sea.

The fauna of the rivers is also worthy of notice, as in last August and September I saw in the Barada, below Damascus, large numbers of crabs, exactly similar in appearance to the shore crabs of Great Britain, and resembling them in colour; the bodies of many are about 6 inches long; the species are no doubt different from those inhabiting our coasts.

In the river Litany, which runs in the valley between the two Lebanons, similar Crustacea are also abundant, and univalve shells of a marine type.

The banks of the two rivers running through Damascus, have for many thousand years maintained an immense population, which, probably, as at present, has received its principal supply of salt from the Mediterranean; this in the aggregate must be considerable, and should not be overlooked in an examination of the waters of the lakes before alluded to.

I am aware that the Crustacea and Mollusca from Syria have not escaped the notice and observations of former travellers, but from the number of the Crustacea, I think it has more importance in a geological bearing than as yet it has received.

I am, Gentlemen,
Yours truly,
FRANCIS M. JENNINGS.

XXXIV. *Experiments on some Sonorous Flames.*
By Prof. W. B. ROGERS, *Boston, U.S.*

To the Editors of the Philosophical Magazine and Journal.

Clapham Common,
March 11, 1858.

GENTLEMEN,

I ENCLOSE an extract from a letter I have this day received from Professor W. B. Rogers of Boston, U.S., describing some interesting experiments made by him on sonorous flames. You may probably consider them of sufficient interest for insertion in the Philosophical Magazine.

Believe me,
Yours truly,
JOHN P. GASSIOT.

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1 Temple Place, Boston,
February 15, 1858.

MY DEAR SIR,

In my former hurried note I alluded to some experiments I was making on *sonorous flames.* I have since ascertained that the presence of a *wick* does not prevent the musical vibration. In a tapering jet-pipe, one-tenth of an inch in diameter, I inserted a wick of loose cotton twine or of asbestos, so as to project some distance into the flame of common illuminating gas burned at the extremity, and I found it easy, by a proper adjustment of the flame and enclosing glass tube, to obtain a clear musical note.

After various attempts to secure the same result with alcohol and other combustible liquids burned directly from wicks, I have found that by using *hollow circular wicks,* and tubes but slightly exceeding them in diameter, we can readily produce the musical effect with the flames of sulphuric æther, alcohol, and the mixture of the latter with spirits of turpentine, which in this country is known as *burning fluid.*

As the effect in these experiments depends on the access to the flame of a current of air of definite amount and velocity and in proper directions, it is necessary to adjust the diameter of the wick and size of the flame to the dimensions of the tube employed, and to hold the tube with its lower edge a little lower than that of the wick. The flame will then be seen to contract and to lose much of its brilliancy, and will give forth a tone which, with a little care, can be rendered quite smooth and continuous.

These results are readily obtained with the flame of a small circular-wicked lamp, now used in the United States for burning the mixture of alcohol and turpentine. In this lamp the wick-tubes rise about 2 inches above the reservoir, and an external moveable tube is provided, which, being raised or lowered, serves to vary the depth of the wick and to adjust the flame with great nicety. The body of the lamp should be removed from its pedestal and placed on a ring-support, to secure a free current of air upward through the wick-tube.

These results add but little to the beautiful experiments by which Faraday, forty years ago, demonstrated the cause of these musical vibrations; but as they present some of the effects in a more satisfactory form, they may give additional force to the conclusion, that *under proper conditions flames of every kind are capable of exciting sonorous vibrations.*

I may here add, that my attention has lately been called to a very striking effect of this kind, observed by Mr. W. F. Shaw, an ingenious gas-fitter of Boston, while experimenting with the wire-gauze burners which he uses in his gas-stoves. On inverting one of these burners over the jet so as to bring the perforated cylinder below, and the continuous metal tube above, the diaphragm of gauze (as seen in fig. 3), he found that the flame, when duly adjusted, gave rise to a very powerful continuous musical sound, which could be changed in pitch and loudness by the addition of other pipes above.

In this arrangement the gas becomes mingled throughout with so large a proportion of air as probably to form a thoroughly explosive mixture on the top of the wire-gauze, thereby favouring the fullest development of the sonorous effect.

In all these experiments the conditions are just such as are suited to produce the succession of explosions which Professor Faraday has shown to be the immediate cause of these musical sounds. As regards the mechanical agency which renders the explosions *successive and not continuous,* I think it is to be traced to those vibrations which Savart has shown to belong to *jets of air,* as well as of water and other liquids.

That you may see at a glance the conditions under which my experiments have been made, I enclose a figure of my apparatus for the æther-flame (fig. 1), one of the circular wick-lamps above referred to (fig. 2), and also a figure of Shaw's burner in the position in which it gives the sonorous effect (fig. 3).

The two former have been already described. The æther-candle is formed of a glass tube about 8 inches long and $\frac{1}{4}$ of an inch in diameter, open below and drawn to a small pointed aperture at top. Some loose cotton-twine or asbestos is introduced so as to reach very near to the opening; the tube is then half-filled with sulphuric æther, and the larger end closed with a cork, after which we may place it vertically in the centre of a wooden block. On applying a light to the apex, the vapour burns in a steady bluish jet, which with proper tubes enables us to repeat all the experiments on the singing and the silent flame. This simple apparatus acts freely at ordinary temperatures, and may be used from time to time for several days without replenishing.

Fig. 1.

Fig. 3.

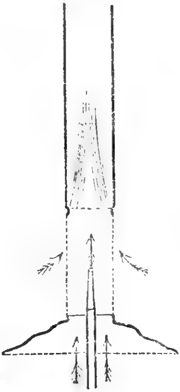
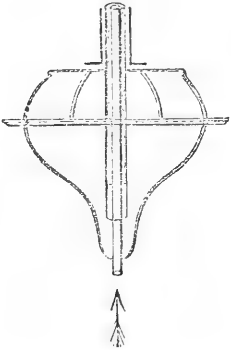


Fig. 2.



With best regards,
Yours truly,
William B. Rogers.

XXXV. *On the Percussion of Bodies.* By M. Poinsot.

[Continued from p. 180.]

On the points by means of which the body is capable of a given reflexion.

37. If we seek the points by means of which the body, on striking an obstacle, may be reflected with a given velocity V, we have merely to make

$$u' = -\mathrm{V}.$$

We thus obtain the quadratic equation

$$(u+\mathrm{V})x^2 - \mathrm{K}^2\theta x + \mathrm{VK}^2 = 0,$$

by means of which the unknown distances of these centres are determined. The two roots of this equation will be real, provided

$$K^2\theta^2 > 4(V^2+Vu);$$

and, as may be easily seen, both roots are positive, and less than h or $\frac{K^2}{a}$.

Hence, provided V satisfies the inequality

$$4V^2+4Va\theta < K^2\theta^2,$$

there are two centres in the body corresponding to the given reflexion V. On examining this inequality we find it to be equivalent to

$$V < \frac{(\lambda-a)\theta}{2},$$

and consequently that it expresses the self-evident condition that the given value V of u' must not exceed the *maximum* of u'.

38. If we represent V by $n u$, n being any number whatever, and subsequently replace u by $a\theta$, the foregoing inequality becomes

$$a < \frac{K}{2\sqrt{n^2+n}},$$

hence the theorem :—*If the movement of the body be such that the distance* a *between its centre of gravity and the spontaneous centre of rotation is less than* $\frac{K}{2\sqrt{n^2+n}}$, *then between the centres of gravity and percussion there will always be two points or centres, such that if an obstacle be presented to the one or the other, the body will be reflected with a velocity* V, n *times greater than that which it possesses.*

39. If, as a particular case,

$$K^2\theta^2=4(V^2+Vu),$$

the two roots of the preceding equation will be equal, and the two centres in question will coincide with the point whose distance from the centre of gravity is

$$x=\frac{K^2\theta}{2(V+u)}.$$

But, solving the corresponding equation

$$4V^2+4Vu=K^2\theta$$

for V, we find

$$V=\frac{(\lambda-a)\theta}{2},$$

that is to say, the given value of V is in this case equal to the *maximum* of u'. Hence the value $\frac{K^2\theta}{2(V+u)}$ of x ought now to resolve itself into the distance $\lambda-a$ of the centre of *maximum* reflexion. It is easy to verify that it does so.

40. If

$$K^2\theta^2 < 4(V^2+Vu),$$

both roots will be imaginary, and no centre corresponding to the given reflexion V can be found.

41. We may remark that when $\theta=0$, this condition for imaginary roots is always fulfilled, whatever value V may have: hence, *when the body is simply translated through space, there is no point by means of which it can be reflected*: wherever the obstacle may be presented, the centre of gravity, after the shock, will continue to move in the same direction as before, and that always with diminished velocity.

42. When $u=0$, that is to say, when the body merely possesses a rotation θ around its own centre of gravity, there are always two centres corresponding to a given reflexion V, provided V is less than $\frac{K\theta}{2}$. This accords with the result in art. 36, since $\frac{K\theta}{2}$ is the *maximum* velocity that can be given to the centre of gravity of a rotating body by presenting to the latter an obstacle or fixed point so as to oppose its rotation.

43. If we replace V by $-V$ throughout the preceding analysis, we shall have the case of a *negative* reflexion, in other words, of a *progression* of the centre of gravity in the same direction as before. Hence to determine these centres of a given progression, or these points by means of which the body, after striking an obstacle, continues to move in the same direction as before with a given velocity, we shall have the equation

$$(u-V)x^2-K^2\theta x-VK^2=0,$$

the roots of which are real, equal, or imaginary, according as

$$K^2\theta-4(V^2-Vu)>0,\ =0,\ \text{or} <0,\ \&c.$$

The discussion of this equation would be analogous to the foregoing.

COROLLARY.

ON THE CENTRES OF PERFECT REFLEXION.

44. If we wished to consider in particular those points or centres by means of which the body would be reflected with the same velocity as it possesses, and thus deport itself as a perfectly elastic body, we should merely have to make

$$u'=-u,$$

or to set $V=u$ in the formulæ of art. 37. By so doing we should have the equation

$$2ux^2-K^2\theta x+K^2u=0;$$

or putting $a\theta$ and h in place of u and $\frac{K^2}{a}$, respectively, the simpler equation

$$x^2-\frac{h}{2}x+\frac{K^2}{2}=0,$$

whence we deduce the roots

$$x=\frac{h}{4}\pm\frac{\sqrt{h^2-8K^2}}{4},$$

which are always real, positive, and less than h, when

$$h^2-8K^2>0,$$

or, putting $\frac{K^2}{a}$ in place of h, when

$$a<.\frac{K}{2\sqrt{2}}.$$

It follows, therefore, that *if the movement of the body is such that the distance* a, *from its spontaneous centre* O *to its centre of gravity* G, *is less than the line* $\frac{K}{2\sqrt{2}}$, *there are always two points or centres of perfect reflexion; that is to say, two points such that if the body strike an obstacle with either, its centre of gravity will be reflected with a velocity exactly equal to that which it possesses.*

Examples.

45. Suppose, by way of example, that $a=\frac{1}{3}K$: the condition $a \quad \frac{K}{2\sqrt{2}}$ is evidently fulfilled, and we have for x the two positive values

$$x_{\prime}=K \text{ and } x_{\prime\prime}=\tfrac{1}{2}K.$$

The two points on the line CG, joining the centres of gravity and percussion, which correspond to these distances, are therefore centres of perfect reflexion; so that by presenting an obstacle to either of them, the centre of gravity of the body would be reflected as if the latter were perfectly elastic.

Although the centre of gravity will move in the same direction and with the same velocity, no matter whether the body strikes with the first or with the second of these points, it will easily be seen that the rotation θ' which the body preserves after the shock is not the same in both cases. In fact, substituting, successively, these values of x in the expression for θ' (art. 31), we find that the rotation θ' becomes $\frac{\theta}{3}$ in the first case, and $\frac{2\theta}{3}$ in the second.

46. If we suppose
$$a=\frac{K}{2\sqrt{2}},$$
the two roots of the equation will be equal and have the value
$$x=\tfrac{1}{4}h.$$
In this particular case of the movement of the body, therefore, there is only one point which can be a centre of perfect reflexion, and its distance from the centre of gravity is one-fourth of that of the latter from the centre of percussion.

CENTRES OF NO REFLEXION.

47. To find the points by means of which the body suffers no reflexion after the shock, we have only to set
$$u'=0,$$
and therefore
$$ux^2-K^2\theta x=0;$$
or, substituting $a\theta$ for u, and h for $\frac{K^2}{a}$,
$$x^2-hx=0,$$
whence we deduce
$$x=0 \text{ and } x=h,$$
two values which correspond respectively to the centre of gravity and centre of percussion. It is evident, in fact, that if an obstacle be presented at either of these centres, the velocity of translation will be destroyed. The only difference between the two cases is, that in the first the velocity of translation u is alone destroyed, without that of rotation θ being altered, whereas in the second case both u and θ are destroyed.

48. In order to find the point where an obstacle must be presented in order that the velocity of translation may suffer no alteration, we should merely have to make $u'=u$, which would lead us to the value
$$x=-a,$$
corresponding to the spontaneous centre O of rotation. In fact since this point of the body is at rest each instant, it is clear that it cannot strike an obstacle there presented, and on that account the motion of the body can suffer no alteration.

General Remark.

49. We may here remark that the theory of the centres of reflexion is, in reality, the same as that of the centres of percussion; for the relation
$$Q+p=P,$$
which exists between the two components Q and p, and the original force P, shows at once that the same point which is the

centre of a given percussion Q, *i. e.* if we please, of a velocity $\frac{Q}{M}$ lost, is also the centre of a given reflexion or velocity $\frac{p}{M}$. These two centres, therefore, coincide with one and the same point, which is considered from two different points of view, but determined in the same manner. Nevertheless, the consideration of these centres with reference to the reflexion which the body suffers when an obstacle is there presented merited the space we have devoted to the same, as well on account of the new dynamical questions thus presented, as on account of the curious similarity thereby established between hard and elastic bodies. In fact, it is well worthy of notice that a perfectly hard body, in virtue purely of the motion it possesses, may be gifted with a certain kind of elasticity at its several points, so that by collision with an obstacle the centre of gravity of the body may be reflected in the rear of its former motion, or precipitated forward with a new velocity, just as if some elastic spring had been interposed at the point of impact. It is also not less remarkable, that this velocity of reflexion may not only be equal to the incident velocity of the centre of gravity, as in the case of perfectly elastic bodies, but may even surpass the same, and become as great as we please, provided the rotation of the body on itself be sufficiently rapid.

50. This increase of velocity, which the centre of gravity of a body may acquire by the mere presence of a fixed point which the body encounters, appears somewhat paradoxical. It seems as if the quantity of movement which exists in a body, and which is always estimated by the product of the mass into the velocity of the centre of gravity, could never be augmented otherwise than by the accession of some new active force applied to the body. But here we see only a fixed point capable of destroying, but not of producing motion, and still it is a fact that on encountering this fixed point, a moving body, far from losing any portion of its velocity, may suddenly become endued with a velocity at once greater than, and in the same direction as, that of its original projection. Here, therefore, in apparent contradiction to the general principles of dynamics, we have, in some measure, a *creation*, instead of a pure loss of motion as one would have anticipated.

We must observe, however, that in nature there is no such thing as a *fixed point*; that a point considered by us as fixed, is in reality a *free* point, which we assume to be charged with a mass extremely great, infinite indeed, in comparison to the mass of the body under consideration; consequently, that if, by the action of a finite force applied to it, this material point receives merely an infinitely small velocity, and thus appears to us to remain motionless, we must nevertheless assume that it has received a finite quantity of motion, which has been entirely transferred there; and hence that the force applied has not been destroyed, but still subsists without the least alteration.

Whenever, therefore, on encountering some fixed point, a body is seen to become animated with a velocity greater than it formerly possessed, we must, if we wish to know the quantity of motion which exists after the shock, not only consider the quantity which the body possesses, but also that which it has imparted, in a contrary direction, to the fixed point in question. And if we take the *difference* between these two quantities of motion, as we ought to do in this case, we shall find precisely the same quantity as existed before the shock; so that the general principle of the *conservation of force* remains intact. It may easily be shown, too, that the principle of the conservation of *areas* is likewise observed, provided the movement of the fixed point—or rather of the free point, with an infinitely great mass—be taken into consideration. Thus, as soon as we take cognizance, not of the moveable body alone, but of the system composed of that body and of the material point in question, all paradox disappears.

51. But to return to the peculiar motion of the body irrespective of that of the fixed point, which is insensible, it may be well to make, or rather to recall, one more important remark, although the reader will probably have already done so. This capability of being reflected on encountering an obstacle, or of being projected forwards with a greater velocity than before, proceeds, essentially, from the *rotation* of the body around its own centre of gravity. For if the body be endued with a simple movement of translation alone, it will neither be capable of any reflexion by means of any of its points, nor of any progression with a velocity superior to its own. By the presentation of an obstacle, the motion of the centre of gravity of the body must, necessarily, be retarded, and it will be altogether arrested if the obstacle be presented in a direction passing through the centre of gravity. But when the body turns on itself, it becomes, so to speak, endued with a kind of elasticity, such as we have already described, and it may then present those curious effects which we observe whenever a body in motion encounters an obstacle, or a series of obstacles successively presented to it.

But to complete our theory, we must now consider the points of the body from a second point of view, relative to the *conversion* or new rotation which it assumes on encountering a fixed point.

ON THE CENTRE OF *MAXIMUM* CONVERSION.

52. We have seen (art. 31) that when the obstacle or fixed point is presented to the movement of the body in a direction

passing at the distance x from its centre of gravity, the rotation θ of the body is changed to another θ', such that

$$\theta' = \frac{K^2\theta - ux}{x^2 + K^2}.$$

Hence to find the point which corresponds to a *maximum* of θ', that is to say, the centre of *maximum* conversion, we have merely to make

$$\frac{d\theta'}{dx} = 0;$$

we thus obtain the quadratic equation

$$ux^2 - 2K^2\theta x - uK^2 = 0,$$

whence, on replacing u by its value $a\theta$ or $\frac{K^2}{h}\theta$, we deduce

$$x = h \pm \sqrt{h^2 + K^2};$$

both these roots are real, one being positive and the other negative.

Thus there are always two centres of *maximum* conversion; the one is situated to the right of the centre of gravity, on the same side as the centre of percussion; the other lies to the left, on the same side as the spontaneous centre.

If we wish to consider the distance δ between the centre of percussion C and the points in question, then, since this distance is evidently $x - h$, the foregoing expression gives

$$\delta = \sqrt{h^2 + K^2} = \sqrt{hl};$$

that is to say, *the distance from the centre of percussion* C *to either of the two centres of* maximum *conversion is the geometric mean between the distance* h *of the centre of gravity, and the distance* l *of the spontaneous centre from the same point* C.

This theorem is precisely similar to that respecting the centres of maximum percussion and reflexion (art. 19). Whence we see that, in all bodies, the centres of conversion are the points which would become centres of reflexion, if the motion of the body were changed so that the centre of percussion became the spontaneous centre, and *vice versâ*.

53. If, in the expression for θ', we substitute for x the first positive root

$$x = h + \delta,$$

we find

$$\theta' = -\theta \frac{K^2}{2h(h+\delta)};$$

thus θ' has a negative value, in other words, its sign is unlike that of θ. The first centre of *maximum* conversion, therefore, is a point by means of which we impart to the body a *maximum* rotation in an *opposite direction* to its original rotation θ.

If for x we substitute the second value

$$x=h-\delta,$$

which is negative, we find

$$\theta'=\theta\cdot\frac{K^2}{2h(\delta-h)};$$

and since $\delta>h$, θ' has a positive value like θ. This second centre of conversion, therefore, is a point by means of which a *maximum* rotation, in the *same direction* as the original rotation θ, is given to the body.

PARTICULAR CASES OF THE MOTION OF THE BODY.

54. When the motion of translation u is zero, we find

$$x=0 \text{ and } \theta'=\theta,$$

that is to say, the centre of *maximum* conversion then coincides with the centre of gravity, and the original rotation of the body is not altered.

55. When the rotatory motion θ is zero, we have

$$x=\pm K \text{ and } \theta'=\mp\frac{u}{2K}.$$

COROLLARY I.

ON THE POINTS OR CENTRES OF A GIVEN CONVERSION.

56. To find the points or centres of a given conversion Θ, we must make

$$\theta'=-\Theta;$$

when, to determine the distances x of these points, we shall have the quadratic equation

$$\Theta x^2-ux+K^2(\theta+\Theta)=0,$$

the roots of which will be real, provided

$$u^2 \text{ or } a^2\theta^2>4K^2(\Theta^2+\theta\Theta).$$

This condition being fulfilled, therefore, there are always two centres corresponding to the given conversion Θ. On examining the condition, it will be found equivalent to

$$\Theta<\theta\cdot\frac{\delta-h}{2h} \text{ or } \frac{K^2}{2h(\delta+h)},$$

that is to say, to the self-evident condition that Θ must not have a greater value than that which corresponds to the *maximum* of θ'.

57. Let $\Theta=n\theta$, n being any given number; the foregoing equation becomes

$$nx^2-ax+(n+1)K^2=0,$$

whence

$$x = \frac{a \pm \sqrt{a^2 - 4K^2(n^2+n)}}{2n};$$

and both roots are real and positive, provided

$$a^2 - 4K^2(n^2+n) > 0.$$

Hence *when the spontaneous centre of rotation is sufficiently distant from the centre of gravity to make*

$$a > 2K\sqrt{n^2+n},$$

there are always two points of the body, on the same side of its centre of gravity as the centre of percussion, such that if either becomes the point of impact the body will suddenly commence rotating in an opposite direction with a velocity n *times greater than that which it possessed before the shock.*

When $a = 2K\sqrt{n^2+n}$, both these centres coincide with one and the same point situated at the distance $\frac{a}{2n}$ from the centre of gravity.

58. If we now suppose n to be negative, it is evident that *when the inequality*

$$a > 2K\sqrt{n^2-n}$$

is satisfied, there are two points such that, if the body strikes an obstacle with either, it will also commence a rotation n *times greater than it had before the shock, but in the same direction as the latter.*

If, further, $a = 2K\sqrt{n^2-n}$, these two centres will coincide with a point at the distance $-\frac{a}{2n}$ from the centre of gravity.

COROLLARY II.

CENTRES OF PERFECT CONVERSION.

59. To find the points by means of which the body might be made to assume a rotation equal and opposite to that which it possesses, we have merely to make

$$\theta' = -\theta \text{ or } n = 1$$

in the expressions of art. 57. In this manner we find

$$x = \frac{a \pm \sqrt{a^2 - 8K^2}}{2},$$

both of which values are real and positive, provided

$$a^2 - 8K^2 > 0.$$

60. If we were to make

$$\theta' = \theta \text{ or } n = -1,$$

we should find $x^2 + ax = 0,$

whence

$$x=0 \text{ or } x=-a;$$

it is, in fact, evident that if we were to present the obstacle either at the centre of gravity or at the spontaneous centre of rotation, the new rotation which the body would assume would be the same as that which it possesses, in other words, the original rotation of the body would suffer no alteration, &c.

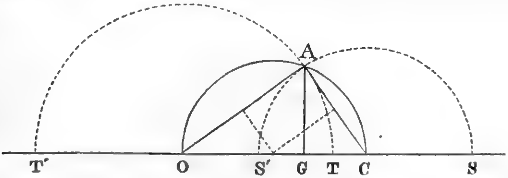
Remark.

61. It is useless, however, to continue the further enumeration of these centres of a given conversion; their research is analogous to that of the centres of reflexion, and presents no difficulty whatever. We must remark, however, that all points, such as the above, which have reference to certain given arbitrary quantities, are not veritable *centres* such as those first considered; in other words, they are not unique points of the body determined solely by the movement under consideration. Their number is, in fact, infinite: for example, the points by means of which a body in motion is capable of striking with a given force, are not restricted to the two above determined; on the contrary, there are innumerable others which possess the same property, and, as we shall afterwards see, all such points of *equal percussion* are situated on the circumference of a certain ellipse within the body. But the points of *maximum* percussion, *maximum* reflexion, &c. are unique; they are, properly speaking, the only ones to which the term *centre* is applicable, and which, as such, deserve to be particularly noticed.

The position of, and mutual dependence between these several centres in any body may be rendered clear and, as it were, visible by means of an extremely simple geometrical figure; in this manner, too, the theorems may be very easily remembered.

GEOMETRICAL EXPRESSION OF THE PRINCIPAL RESULTS OF THE FOREGOING MEMOIR.

62. Let G be the centre of gravity, and C the centre of percussion of the body, that is to say, the centre of the original impulse which it has received. Join C and G, and at G erect a perpendicular to C G, making its length G A equal to K the *arm*



of inertia of the body. If, upon C A as chord, a circle be described so that its centre is in the direction of C G, this line produced will cut the circumference in a point O, which will be the spontaneous centre of rotation.

And, conversely, starting from the point O, as given, we may find the corresponding *centre of percussion* C by joining O and A, and upon O A as chord describing a circle, whose diameter shall coincide with O G; O G produced will cut the circumference of this circle in the point C required.

The two *reciprocal* centres C and O, therefore, between which the centre of gravity is situated, may be considered as the two extremities of the diameter of a circle whose ordinate G A, corresponding to the point G, represents the line K which determines the moment of inertia of the body.

63. If now from O as centre, and with radius O A, a circle be drawn cutting the direction of the diameter in the points T and T′, we shall obtain the two centres of greatest percussion; that is to say, T will be the centre of greatest percussion in the direction of the translation of the body, and T′ that of greatest percussion in the opposite direction, in other words, when the body strikes in the rear of its motion through space.

64. These points T and T′ will be at the same time the two centres of *greatest* reflexion; the first T will be that of a veritable reflexion of the centre of gravity, whilst the second T′ will be that of a *negative* reflexion, in other words, of a progression of the centre of gravity in the same direction as it at present moves.

65. In the second place, the circle described around C as centre, with the radius C A, cuts the diameter in two points S and S′, which will be the two centres of *greatest conversion*: the first S, which is in the production of the diameter, is the centre of *maximum positive* conversion; that is to say, the point by which, if an obstacle be there presented, the body will be made to rotate, after the shock, in a direction contrary to that of its present rotation, and with the greatest possible velocity; the other centre S′ is that of *maximum negative* conversion, or the point by means of which, after the shock, the greatest possible rotation will be given to the body in the *same direction* as it at present turns.

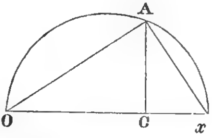
66. Thus the simple and well-known figure of a right-angled triangle, with the perpendicular let fall from the right angle upon the hypothenuse, supplies everything with respect to the position of, and mutual dependence between the several centres we have considered in a body which turns around one of its principal axes at the same time that its centre of gravity is translated through space in a direction perpendicular to this axis.

The figure clearly shows, too, what takes place when the body is endued with only one of these two motions. For:

First. If the impulse which the body has received is due, solely, to a simple force passing through the centre of gravity, the centre C of percussion will coincide with the point G, the side CA with the perpendicular GA=K, and the other side AO of the right-angled triangle with the line through A parallel to the hypothenuse. The centre T of greatest *positive* percussion, therefore, now coincides—as it clearly should do—with the point G, and the other centre T′ of greatest *negative* percussion is at an *infinite* distance; lastly, the two centres S and S′ of greatest conversion are situated at the distance CA=GA=±K from the same centre G.

Secondly. If the body has received the impulse of a couple merely, the spontaneous centre O will coincide with the centre of gravity, the side OA with the perpendicular GA, and the side AC with the parallel through A to the hypothenuse. The two centres T and T′ of *maximum* percussion are now at the distance OA=±K from the centre G; and of the two centres S and S′ of greatest conversion, the first S is *infinitely* distant, and the second S′ coincides with the same centre G.

67. Lastly, in order to have a clear idea of the force or action at any point x of the body taken arbitrarily on the hypothenuse CO, we have only to join x with the vertex A of the right angle of the triangle and determine the fraction $\frac{AG^2}{\overline{Ax}^2}$ of the whole mass M of the body. We may then say that the point x acts as would a free point charged with this fraction of the whole mass M. We see, too, that the point O reciprocal to x would act as a free point charged with the remainder of this mass; so that these two reciprocal points x and O divide the mass of the body into two parts reciprocally proportional to $\overline{xA}^2$ and $\overline{OA}^2$, or—what amounts to the same thing—into two parts inversely proportional to their distances OG and xG from the centre of gravity G of the body.



68. It appears to us that truths so evident, and so easy of expression as the above, are as new elements added to science, and cannot fail to contribute to its advancement. For we must admit that the human understanding advances but little, otherwise than by the aid of such simpler ideas or more convenient instruments which it invents and, so to speak, handles with greater facility. These new questions, therefore, appear to us not unworthy of the attention of geometricians, and their novelty

as well as their possible use in mechanics justify, we think, all the details and all the developments we have given in the foregoing memoir.

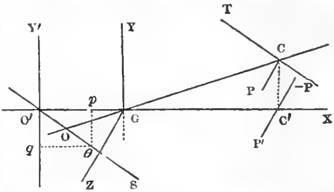
But these elegant propositions are themselves merely corollaries of other more general ones, as we shall see in the following chapter.

CHAPTER II.

§ I.

1. Hitherto we have supposed the direction of the actual impulse animating the body to be in the plane of its two principal axes G X and G Y, so that spontaneous rotation took place around an axis parallel to the third principal axis G Z.

We will now consider the case where the impulse P is given in a direction perpendicular to the plane of the two axes G X and G Y, and at any point C in this plane, which point we shall call the centre of *impulsion.* Such an impulse gives rise, as we shall show, to a spontaneous axis in the plane containing these two principal axes; the point O, where the line C G produced meets this spontaneous axis O S, will be referred to as the *spontaneous* centre corresponding to the centre C. As will be seen, these two centres are always reciprocal; that is to say, if the impulse were given at O, a spontaneous centre would be formed at C, and the spontaneous axis C T would be parallel to the first O S.



The question to be first solved therefore is the following:—

PROBLEM I.

Given, in the plane of two principal axes of a body, the centre C *of an impulse* P *perpendicular to this plane, to determine the corresponding spontaneous axis* O S.

Solution.—Let x and y be the coordinates of the point C, where the impulse is applied, with respect to the two principal axes G X and G Y under consideration; let m be the mass of the body, and $m\alpha^2$, $m\beta^2$ its moments of inertia around the axes G X and G Y respectively.

If P be transported parallel to itself from C to C′ on the axis G X, C′ being the foot of the ordinate y of the point C (see foregoing figure), we shall have in the first place a couple, with the moment Py, tending to produce rotation around the axis G X with an angular velocity

$$p=\frac{\mathrm{P}y}{m\alpha^2},$$

and in the next place, we shall have a force equal and parallel to the given force P, but applied at C′, whose distance from the centre of gravity G is x. We have seen that this force causes a spontaneous rotation around a certain axis O′ Y′ parallel to G Y, and situated on the other side of the centre G at a distance

$$x'=\frac{\beta^2}{x};$$

the angular velocity around this axis is

$$q=\frac{\mathrm{P}x}{m\beta^2}.$$

In virtue of the impulse given at C, therefore, the body tends at one and the same time to turn around O′ X with the angular velocity

$$p=\frac{\mathrm{P}y}{m\alpha^2},$$

and around O′ Y′ with the angular velocity

$$q=\frac{\mathrm{P}x}{m\beta^2}.$$

Hence, according to the principle of the composition of rotations*, the body tends to turn around the diagonal of the rectangle constructed on the two lines O′p and O′q which represent the rotations p and q. The given force P, therefore, gives rise to a spontaneous axis of rotation which cuts the axis of x at the distance

$$x'=-\frac{\beta^2}{x}$$

from the origin G, and is inclined to this axis at an angle whose tangent is

$$-\frac{q}{p}=-\frac{\alpha^2x}{\beta^2y};$$

so that, t and u being the current coordinates of this spontaneous axis O′S, its equation will be

$$u=-\frac{\alpha^2x}{\beta^2y}\left(t+\frac{\beta^2}{x}\right);$$

or, more simply,

$$\alpha^2xt+\beta^2yu+\alpha^2\beta^2=0.$$

Such, then, is the equation, between the coordinates t and u,

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* *Théorie nouvelle de la rotation des corps,* 1re partie, art. 5.

of the spontaneous axis corresponding to a centre of impulsion whose coordinates are x and y. Other equally simple solutions might be given, but it is not necessary to do so here.

2. Considering t and u now as the coordinates of the *spontaneous centre* O, which is in the line C G produced, we have the relation

$$t : u = x : y,$$

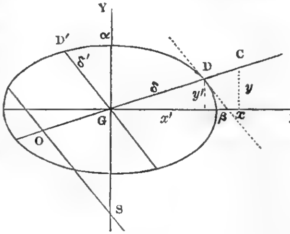
which, combined with the preceding equation, gives

$$t = -\frac{\alpha^2\beta^2 x}{\alpha^2x^2+\beta^2y^2},$$

$$u = -\frac{\alpha^2\beta^2 y}{\alpha^2x^2+\beta^2y^2}.$$

The coordinates of the spontaneous centre O are thus expressed by means of those of the centre of impulsion C, or *vice versâ*; for it will be observed that in these two expressions, as in the symmetrical equations whence they are deduced, we may interchange t and x, and u and y; in other words, the two centres C and O are always *reciprocal.*

3. If H be the distance $\sqrt{x^2+y^2}$ of the centre of impulsion C from the centre of gravity G, and A the distance $\sqrt{t^2+u^2}$ of the spontaneous centre O from the same point G, we shall have, by putting for t and u their values in x and y, and forming the product AH of the distances in question,



$$\text{AH} = \alpha^2\beta^2\frac{x^2+y^2}{\alpha^2x^2+\beta^2y^2}.$$

Now, since the numerator and denominator here are each homogeneous functions of the second degree in x and y, we may replace the latter by any other two lines proportional to them. If, therefore, we agree that x and y shall now represent the coordinates of the point D where the line C G cuts the ellipse whose equation is

$$\alpha^2x^2+\beta^2y^2=\alpha^2\beta^2,$$

and which we will call the *central* ellipse, the second term of our preceding equation will be reduced to x^2+y^2 simply; but x^2+y^2 now represents the square δ^2 of the semi-diameter GD determined in the central ellipse by the given direction of CG; hence between the distances A and H of the two reciprocal centres O and C from the centre of gravity G, we have the remarkable equation

$$AH = \delta^2,$$

δ being the length of that semi-diameter of the central ellipse whose direction passes through the two points in question.

The diameter 2δ is inclined to the axis of x at an angle whose tangent is $\frac{y}{x}$; the direction of OS is inclined to the same axis at an angle whose tangent is $-\frac{\alpha^2 x}{\beta^2 y}$ (art. 1), and the product of these two tangents is $-\frac{\alpha^2}{\beta^2}$: consequently, considering the central ellipse whose equation is

$$\alpha^2 x^2 + \beta^2 y^2 = \alpha^2 \beta^2,$$

we may say that the spontaneous axis OS is always *parallel* to the diameter $2\delta'$, which is *conjugate* to the diameter 2δ, whose direction passes through the centre C of the impulse given to the body*.

4. The form of the equation of our ellipse shows that β is the length of its semi-axis in the direction of the axis of x, and α that of its semi-axis in the direction of the coordinate axis of y; α and β being the *arms of inertia* of the body with respect to its principal axes GX and GY. But by forming the rectangle

$$\alpha\beta = \text{const.} = R^2,$$

we have, evidently,

$$\alpha = \frac{R^2}{\beta} \text{ and } \beta = \frac{R^2}{\alpha},$$

by which it will be seen that the two axes of the central ellipse are not directly, but inversely proportional to the arms of inertia of the body around the same axes. It is further manifest, from the well-known formula which gives the moment of inertia around any axis whatever, that this property of the two axes of the central ellipse extends to all diameters of the same; that is to say, the arm of inertia of the body around any diameter is *inversely* proportional to the length of this diameter†.

5. If, therefore, K be the arm of inertia with respect to the

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* Salmon's 'Conic Sections,' art. 174.

† By the formula here alluded to (see *Théorie nouvelle de la rotation des corps*, 1$^{\text{re}}$ partie, art. 74), the arm of inertia with respect to any line GD making an angle ψ with the axes of x is $\sqrt{\alpha^2\cos^2\psi + \beta^2\sin^2\psi}$. But, δ being the semi-diameter of the central ellipse whose direction coincides with GD, $\delta\cos\psi$ and $\delta\sin\psi$ are the coordinates of its extremity, and as such fulfil the equation of the curve. Hence $\delta^2(\alpha^2\cos^2\psi + \beta^2\sin^2\psi) = \alpha^2\beta^2 = R^4$, and the arm of inertia in question has the value $\frac{R^2}{\delta}$.

diameter δ' which is parallel to the spontaneous axis OS, we shall have

$$K=\frac{R^2}{\delta'}=\frac{\alpha\beta}{\delta},$$

but in the ellipse

$$\alpha\beta=\delta\delta' \sin \phi,$$

where ϕ is the angle between the conjugate diameters δ and δ'; hence

$$K=\delta \sin \phi$$

is the value of the arm of inertia around the diameter conjugate to δ, and it is also the distance of the extremity of the latter diameter from its conjugate δ'.

The above equation,

$$AH=\delta^2,$$

therefore, becomes

$$A \sin \phi. \quad H \sin \phi=K^2,$$

and shows that the distances $A \sin \phi$ and $H \sin \phi$ of the reciprocal centres C and O from the diameter GD′, parallel to OS, are such that their product is equal to the square of the arm of inertia of the body around this diameter GD′. This proposition is exactly similar to the one demonstrated in the first chapter (art. 5); it is, however, more general than the latter, and contains it as a particular case.

Corollary I.

6. We have just seen that if x and y are the coordinates of any point C, considered as a *centre of impulsion*, the equation of the *spontaneous* axis corresponding to this centre C will be

$$\alpha^2xt+\beta^2yu+\alpha^2\beta^2=0,$$

where t and u are the current coordinates. It is manifest that if x and y vary, in other words, if we suppose C to change its place, the line OS will change its position also. It might be asked, therefore, what relation ought to be established between x and y in order that the spontaneous axis OS may always pass through one and the same point O, having the coordinates t' and u'. To find this relation it is evidently sufficient to regard the preceding equation as being always satisfied for the coordinates $t=t', u=u'$ of the given point O; the required relation between x and y, therefore, is

$$\alpha^2t'x+\beta^2u'y+\alpha^2\beta^2=0;$$

which we at once recognize to be the equation of a spontaneous axis CT corresponding to the point O considered as a centre of impulsion.

Thus, in order that the several centres of impulsion C, C′, C″,

&c. may give rise to different spontaneous axes intersecting in one and the same point O, the former must be situated upon the line CT which corresponds, as spontaneous axis, to the given point O considered as a centre of impulsion or percussion.

Remark.

7. Since, during the first instant, all the points of a spontaneous axis OS remain motionless, we see that a shock at the centre C causes no percussion either at the reciprocal centre O or at any other point of the spontaneous axis OS.

Similarly, by striking at O no percussion is caused at any point of the parallel axis CT.

COROLLARY II.

8. From this remark and the preceding corollary we may deduce this remarkable theorem:—a shock at any point C′ whatever in the line CT causes no percussion at the spontaneous centre O, for the spontaneous axis O′S′, which corresponds to C′, always passes though the point O.

Similarly, at whatever point of the spontaneous axis OS the body may be struck, no percussion will be caused at the centre C.

COROLLARY III.

9. With respect to these centres and their corresponding axes, various other questions, analogous to the foregoing one, may be proposed and solved with equal facility.

For instance, upon what curve ought the centres of percussion to be situated in order that the corresponding spontaneous axes may all touch a given curve?

Let

$$u=f(t)$$

be the equation of the given curve, and

$$\alpha^2xt+\beta^2yu+\alpha^2\beta^2=0$$

that of the spontaneous axis corresponding to the centre C, whose coordinates are x and y.

The contact of this curve and right line requires that the two differential coefficients $\frac{du}{dt}$, deduced from the above equations, shall have the same value at the point of contact; hence we deduce the equation

$$\alpha^2x+\beta^2y\,.f'(t)=0.$$

If, by means of two of these equations, we eliminate t and u from the third, we shall obtain an equation between x and y which will give the required locus of the centres C.

As a particular case, let us suppose the given curve $u=f(t)$ to be a parabola having its vertex at G, GY as axis, and a parameter equal to A. Our three equations will then be

$$\mathrm{A}u = t^2,$$
$$\alpha^2 xt + \beta^2 yu + \alpha^2\beta^2 = 0,$$
$$\mathrm{A}\alpha^2 x + \beta^2 y \,.\, 2t = 0;$$

whence, eliminating t and u, we find that the centres C must be situated on the parabola

$$4\beta^2 \,.\, y = \mathrm{A}\alpha^2 \,.\, x^2,$$

having the same axis and vertex as the given parabola, but a parameter $\mathrm{A}' = \frac{4\beta}{\mathrm{A}\alpha^2}$.

Hence, when the centres of percussion are situated on this parabola, all the corresponding spontaneous axes will be tangents to the other given parabola.

Provided A were equal to $\frac{2\beta^2}{\alpha}$, then A′ and A would be equal to one another, and the two parabolæ would coincide; so that the centres being taken upon one branch, all the spontaneous axes would be tangents to the other branch of the same parabola.

Corollary IV.

10. The same analysis would lead us to the solution of this inverse problem: given the curve which forms the locus of the centres of percussion, to find the curve formed by the successive intersections of the spontaneous axes, in other words, the curve to which all such axes are tangents.

For, x and y being still the coordinates of any one of the centres C, the corresponding spontaneous axis has the equation

$$\alpha^2 xt + \beta^2 yu + \alpha^2\beta^2 = 0. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now if an infinitesimal variation be given to x and y, t and u being regarded as constant, we shall have

$$\alpha^2 t + \beta^2 u \,.\, \frac{dy}{dx} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and these two equations between u and t are satisfied only at the point of intersection of the two spontaneous axes corresponding to the two centres infinitely close to each other. But, by hypothesis, the locus of these centres is given, so that between x and y we shall have a given equation

$$y = f(x),$$

whence we may deduce

$$\frac{dy}{dx} = f'(x).$$

These two values being substituted in the preceding equations (1) and (2) and x subsequently eliminated, there will result, between t and u, an equation

$$u=\psi(t),$$

which will give the required curve formed by the successive intersections of the spontaneous axes, and to which these axes are tangents.

11. For example, if the given locus of the centres be a parabola

$$\mathrm{A}y=x^2,$$

the required curve will be found to be

$$\frac{4\beta^4}{\mathrm{A}\alpha^2}\cdot u=t^2,$$

which is also a parabola whose parameter is inversely proportional to the parameter A of the first; so that if A were given equal to $2\frac{\beta^2}{\alpha}$, the two parabolæ would form but one,—a result in perfect accordance with what we have seen in the preceding corollary.

12. If the centres were taken along the contour of the central ellipse whose equation is

$$\alpha^2x^2+\beta^2y^2=\alpha^2\beta^2,$$

the curve enveloped by the corresponding spontaneous axes would be found to be

$$\alpha^2t^2+\beta^2u^2=\alpha^2\beta^2,$$

or the same ellipse; in fact since, by hypothesis, each centre is at the extremity of a diameter, the corresponding spontaneous axis is nothing more than the tangent at the other extremity.

13. If the locus of the centres were the circumference of a circle given by the equation

$$x^2+y^2=\mathrm{R}^2,$$

the equation of the required curve would be found to be

$$\frac{t^2}{\beta^4}+\frac{u^2}{\alpha^4}=\frac{1}{\mathrm{R}^2},$$

which is that of an ellipse having the semi-axes $\frac{\beta^2}{\mathrm{R}}, \frac{\alpha^2}{\mathrm{R}}$ directed, respectively, along the axes β, α of the central ellipse, but proportional in length to the squares of these axes.

Corollary V.

14. Questions relating to the mutual positions of the centres C and O of percussion and spontaneous rotation are still simpler, and more easily solved than the preceding ones; for x

and y being, as usual, the coordinates of the point C, and t and u those of the reciprocal centre O, we have, between the coordinates of these two points, the equations

$$x=-\frac{\alpha^2\beta^2 t}{\alpha^2t^2+\beta^2u^2},$$

$$y=-\frac{\alpha^2\beta^2 u}{\alpha^2t^2+\beta^2u^2}.$$

If, therefore, the centres C being taken on any curve whatever given by the equation $y=f(x)$,

the locus of the corresponding centres O be required, it is only necessary to substitute, for x and y, the preceding values in order to obtain immediately, between t and u, the equation of the required locus of the centres O.

And conversely, given the locus of the spontaneous centres, that of the centres C would be found by means of the expressions reciprocal to the foregoing.

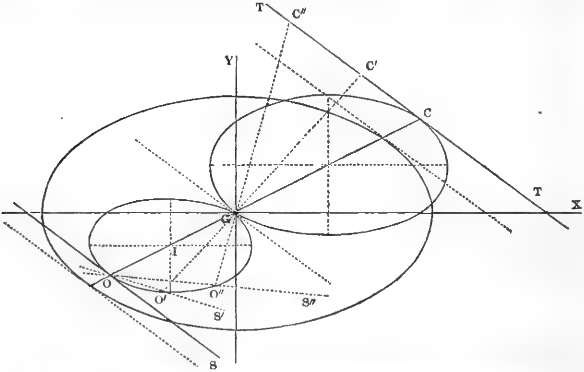
15. Let the centres C be situated in any given right line; the latter may always be represented by an equation of the form

$$\alpha^2ax+\beta^2by+\alpha^2\beta^2=0,$$

provided we give suitable values to the two arbitrary coefficients a and b, and if in this equation we express x and y in terms of t and u, we shall have, for the locus of the corresponding spontaneous centres O, the equation

$$\alpha^2t^2+\beta^2u^2-\alpha^2at-\beta^2bu=0.$$

This is an ellipse similar and similarly placed to the *central* ellipse; its centre, however, is at the point I, whose coordinates



are $\frac{a}{2}$ and $\frac{b}{2}$, and its circumference passes through the origin G.

The right line C T, having been represented by the equation

$$\alpha^2 ax+\beta^2 by+\alpha^2\beta^2=0,$$

may, manifestly, be regarded as the spontaneous axis corresponding to a centre O whose coordinates are a and b. The centre I of our ellipse, therefore, is the middle point of the line G O; and the ellipse itself is constructed upon G O as diameter homologous to the like-directed diameter in the central ellipse.

16. Hence, *regarding the given line* C T *as a spontaneous axis corresponding to a centre* O, *we may say that if the several points of this line be taken as centres of percussion, the corresponding spontaneous centres will be all situated on an ellipse similar to the central ellipse, and described upon the line* G O *as diameter homologous to that which the direction of* G O *determines in the central ellipse.* And conversely, *if the points of this ellipse were taken as centres of percussion, the spontaneous centres would be all situated along the line* C T.

On account of the reciprocity between the centres C and O, it is evident that if the centres of percussion were the points of the line O S, parallel to C T, the corresponding spontaneous centres would be on an ellipse similar to the first, but described on C G as diameter homologous to O G.

17. As a second example, let the centres C be taken on the parabola represented by the equation

$$y^2=\mathrm{A}x;$$

putting in place of x and y their values in t and u, we find the equation

$$\mathrm{A}\alpha^2t^3+\mathrm{A}\beta^2u^2t+\alpha^2\beta^2u^2=0,$$

which gives the required locus of the spontaneous centres O corresponding to the centres C.

Solved with respect to the ordinate u, this equation gives

$$u=\pm\frac{\alpha}{\beta}\sqrt{\frac{-\mathrm{A}t^3}{\alpha^2+\mathrm{A}t}};$$

whence, since α, β, and A are *positive*, we see that this curve of the third order has only real ordinates u on the side of the *negative* abscissæ t; that, like the parabola ($y^2=\mathrm{A}x$), the curve has two equal branches which stretch to infinity above and below the abscissa axis; but that these two branches cannot recede from the ordinate axis to a distance greater than

$$t'=-\frac{\alpha^2}{\mathrm{A}},$$

so that a line drawn parallel to the ordinate axis, and at a distance from the same equal to t', is an asymptote to both branches of the curve in question.

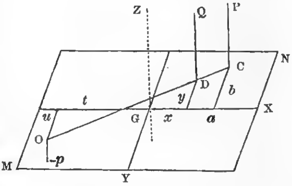
Many other questions of the same kind might be proposed; these examples, however, will suffice, and we will proceed to a more important problem from which numerous consequences may be deduced.

§ II.

PROBLEM II.

18. *Let* P, *as before, be the force of impulsion given to the body in a direction perpendicular to the plane of two of its principal axes* G X *and* G Y; *required the magnitude* Q *of the percussion which the body would produce against an obstacle or fixed point suddenly presented at any point* D *in the plane of these same axes.*

Solution.—Let C be the point in this plane where the impulsion P is applied: join C D, and upon the production of this line suppose a point O to be so chosen that, by striking at O, no percussion would be caused at the point D. It is evident that if the force P be resolved into two other parallel forces, one Q applied at D, and the other p applied at O, the required percussion at the point D will be exactly represented by the component Q immediately applied thereto; for, by hypothesis, the other component p which strikes at O causes no percussion upon the point D. Hence, in virtue solely of the principle of the composition of parallel forces, the required percussion Q at the point D in question will be expressed by



$$Q = P \cdot \frac{CO}{DO}.$$

To solve the problem, therefore, we have merely to determine the position of the point O by means of the two points C and D.

Now, in the first place, if a and b be the coordinates of the point C, x and y those of the point D, and t and u those of the point O, then, since O is on the line which passes through C and D, we have the equation

$$u - y = \frac{b-y}{a-x}(t-x).$$

In the second place, in order that D may suffer no percussion when the body is struck at O, the spontaneous axis corresponding to the latter point must pass through the former: hence this second equation,

$$\alpha^2 tx + \beta^2 uy + \alpha^2\beta^2 = 0.$$

Combining these two equations with a view of obtaining the values of t and u as functions of x, y, a and b, we find

$$t=\beta^2\frac{\alpha^2(x-a)+y(bx-ay)}{\alpha^2ax+\beta^2by-\alpha^2x^2-\beta^2y^2};$$

in the same manner, without calculation, and by simply interchanging x and y, a and b, α and β, we have

$$u=\alpha^2\frac{\beta^2(y-b)+x(ay-bx)}{\alpha^2ax+\beta^2by-\alpha^2x^2-\beta^2y^2}.$$

Either of these two expressions, however, will here suffice; for since

$$\mathrm{Q}=\mathrm{P}\cdot\frac{\mathrm{CO}}{\mathrm{DO}},$$

and the ratio of the two lines C O and D O is the same as that of their projections upon either of the two coordinate axes, we have, by projecting upon the abscissa axis, the simple expression

$$\mathrm{Q}=\mathrm{P}\cdot\frac{a-t}{x-t}.$$

Substituting for t its value in x and y, as above given, we find, after necessary reductions, the elegant formula

$$\mathrm{Q}=\mathrm{P}\cdot\frac{\alpha^2ax+\beta^2by+\alpha^2\beta^2}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2}. \quad . \quad . \quad . \quad . \quad . \qquad \text{(A)}$$

This, then, is the expression for the force Q with which a body, animated by an impulse P perpendicular to one of its principal planes, is able to strike an obstacle presented at any point D in the same plane.

Corollary I.

19. In the first place, it will be seen that this percussion Q is zero at all points for which

$$\alpha^2ax+\beta^2by+\alpha^2\beta^2=0,$$

that is to say, at all points of the spontaneous axis O S corresponding to the centre of impulsion C.

In the next place, it is clear that this percussion Q will be equal to the force P itself when

$$\alpha^2x^2+\beta^2y^2=\alpha^2ax+\beta^2by;$$

whence it follows that the body strikes with the same force P, not only at its centre of impulsion C, and at its centre of gravity G, but also at all points of the circumference of the ellipse described upon the line C G, with two axes parallel and proportional to the axes α and β of the central ellipse of the body.

Corollary II.

On the centre of *maximum* percussion.

20. To find the coordinates x and y of the point D where the percussion Q is a *maximum*, we have merely to form the two equations

$$\frac{dQ}{dx}=0, \quad \frac{dQ}{dy}=0,$$

which give

$$\alpha^2 a(\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2)-2\alpha^2x(\alpha^2ax+\beta^2by+\alpha^2\beta^2)=0,$$
$$\beta^2 b(\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2)-2\beta^2y(\alpha^2ax+\beta^2by+\alpha^2\beta^2)=0,$$

whence we immediately deduce the proportion

$$x:y=a:b.$$

This shows, in the first place, that the required point D lies in the direction of the line C G joining the centres of impulsion and gravity. Substituting, therefore, in either of the preceding equations, for y its value $\frac{b}{a}x$, and subsequently for x its value $\frac{a}{b}y$, we shall have, for determining x or y separately, the quadratic equations

$$(\alpha^2a^2+\beta^2b^2)x^2+2\alpha^2\beta^2ax-\alpha^2\beta^2a^2=0,$$
$$(\alpha^2a^2+\beta^2b^2)y^2+2\alpha^2\beta^2by-\alpha^2\beta^2b^2=0;$$

these equations are exactly similar; the first resolves itself into the second by simply changing x and a into y and b, and *vice versâ.*

21. But, since the required point D is on the given line C G, let us make the distance $v=\text{DG}$ between the required point and the centre of gravity G our unknown term; by making

$$\text{CG}=\sqrt{a^2+b^2}=\text{H},$$

we shall have the proportions

$$v:\text{H}=x:a=y:b,$$

whence

$$x=\frac{a}{\text{H}}v, \text{ and } y=\frac{b}{\text{H}}v.$$

Substituting these values in the preceding equations, we have, to determine v, the equation

$$v^2+2\text{H}\frac{\alpha^2\beta^2}{\alpha^2a^2+\beta^2b^2}\cdot v-\text{H}^2\frac{\alpha^2\beta^2}{\alpha^2a^2+\beta^2b^2}=0;$$

but, A being the distance between the spontaneous centre O and the centre of gravity G, we have

$$\frac{\alpha^2\beta^2}{\alpha^2a^2+\beta^2b^2}=\frac{A}{H},$$

for, by art. 2, the first term expresses the ratio of the abscissæ or ordinates of the two reciprocal points C and O, and this ratio is evidently the same as that of the two lines H and A.

Employing this simpler expression, the equation for determining v becomes

$$v^2+2Av-AH=0,$$

and gives

$$v=-A\pm\sqrt{A^2+AH}.$$

There are, therefore, two centres of *maximum* percussion, one to the right, and the other to the left of the centre of gravity G.

In order to refer these two points to the *spontaneous* centre of rotation O, we have only to make

$$DO=\lambda=v+A$$

in order at once to deduce

$$\lambda=\pm\sqrt{AL},$$

where for brevity L represents the line OC = A + H. We are thus led to the following theorem:—

The centre of maximum *percussion is upon the line passing through the body's centres of impulsion and gravity, and its distance* λ *from the* spontaneous *centre of rotation is the geometric mean between the distances of the centre of gravity and of the centre of impulsion from the same* spontaneous *centre.* This theorem is precisely similar to the one before found in the particular case where spontaneous rotation takes place around an axis parallel to one of the principal axes of the body.

22. As to the value (Q) of the *maximum* percussion, it is expressed by

$$(Q)=P\,.\,\frac{1\pm\sqrt{1+\frac{H}{A}}}{2},$$

as will be easily found from the general expression for Q by there substituting, in the first place, for x and y their values in v, and afterwards for v the value

$$v=-A\pm\sqrt{A^2+AH},$$

which corresponds to the *maximum.*

The first value (Q), which is positive, corresponds to the centre D situated between C and G; it gives a percussion in the *same direction* as, and always *greater* than the impulsion P.

The second value (Q), which is negative, corresponds to the second centre D′ of *maximum* percussion; it is situated on the other side of, and at the same distance from, the spontaneous centre O: it gives a percussion *opposite* in direction to the impulsion P which animates the body, and always *less* than the same.

[To be continued.]

XXXVI. *On the Difference in the Amount of Electricity developed by equal surfaces of Cylinder and Plate Electrical Machines.*

By JONATHAN N. HEARDER, *Mechanician, Plymouth*[*].

IT is a fact worthy of notice, that, whilst there has been a continual succession of important improvements in the modes of developing electricity by chemical or voltaic action, as well as in the apparatus for applying the electricity thus obtained to practical purposes, the machines for exciting Franklinian or frictional electricity have remained, so far as the principles of their construction are concerned, just in *statu quo* during the last half century. It is true that machines have been constructed of greater magnitude than formerly, but no attempt has been made to examine the conditions upon which electrical excitation depends, or the results arising out of modifications of these conditions. The electrical machines of the present day do not develope more electricity from the friction of a given amount of surface than those constructed fifty years ago; and indeed, from occasional allusions made to the power of certain electrical machines in the various valuable scientific papers which I have lately met with, I question much if the electrical machines of the present day are at all equal to some constructed by Nairne, Cuthbertson, Adams, and others. The rationale of the action of the amalgam still remains unexplained; and indeed so little appears to have been done with regard to the investigation of the action of the electrical machine, that I never remember to have heard any notice taken of the extraordinary difference in the amount of electricity obtained from the friction of equal surfaces on cylinder and plate electrical machines, the superiority of the former being greater than as 4 to 1. I noticed this extraordinary difference about twenty-six years since, when I had frequent opportunities of contrasting one of my cylinder machines of $10\frac{1}{2}$ inches in diameter with an excellent 3-feet plate machine contrived by Sir W. S. Harris, and described in the Electrical Number of Weale's Series, Transactions of the Plymouth Institution, and elsewhere. Although I did not then enter into any very accurate examination of their relative powers, I was never-

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[*] Communicated by the Author; having been read at the Plymouth Institution and Devon and Cornwall Natural History Society, March 1, 1858.

theless satisfied of the fact, and having subsequently constructed many electrical machines of various kinds and sizes, the superiority of cylinder over plate machines was continually forcing itself upon my attention. About sixteen years since, I fitted up a small cylinder machine for the purpose of examining some of the phænomena of electrical excitation. As the experiments were undertaken principally for my own practical guidance, I did not publish them, but during some recent investigations with my new induction coils, my attention has been frequently again directed to the subject, and I have thought that some account of the experiments might not be altogether unacceptable to electricians.

My object in the present paper is to point out some of the phænomena attendant on the action of the electrical machine, together with some circumstances by which they are modified. To ensure accuracy, I have lately repeated the experiments with several machines, and amongst them the 10½-inch cylinder before alluded to, and with corroborative results.

The standard which I have employed for comparing these results, has been a discharge of a given length from a Leyden jar, using the same jar in all experiments that were compared with each other. I once attempted to establish a common unit-measure which would be accessible to all, and which would afford a means by which electricians could compare their own results with those of others, but did not succeed to my satisfaction. Leyden jars, although they might contain a given amount of coated surface, did not seem adapted to the purpose, as their variable thickness would modify considerably the quantity of electricity requisite to produce a discharge of a given length; thus two jars of equal size, in the hands of two different individuals, would give very different results. I therefore thought of employing coated plates of glass, whose thickness might be always ascertained by their weight per square foot, but found, to my disappointment, that their retentiveness of residual charge was so considerable, that it was perfectly impossible to ensure accuracy of result, even in a consecutive series of experiments with the same plate. As much as 10 or even 20 per cent. of the original charge sometimes remained as a residue, which would furnish ten or a dozen minute discharges over a period of more than half an hour. This disadvantage, which appears peculiar to crown and plate-glass, since I find it to exist in Leyden jars made of the same material, induced me to abandon the use of the plates, and content myself with using the Leyden jar and a graduated discharging electrometer. The number of turns of the machine requisite to produce a discharge of a given length being multiplied by the area of glass rubbed at a single revolution, gave the amount of surface required to be submitted to friction to produce a given effect.

Great care was necessary to avoid discrepancies which would otherwise arise from variations in the quality of the amalgam; and when different amalgams were used, their relative value was always previously ascertained by having recourse to a given standard of measurement, and all subsequent experiments were reduced to this standard. The discharging interval was always kept as small as was consistent with the character of the experiment, since, with high charges, the conductor, partaking of the intensity of the jar, takes up less and less of the electricity from the cylinder as the charge increases, and consequently much of the electricity thus excited passes round upon the surface of the cylinder to the rubber again, and the results are therefore inaccurate.

Although my experiments will serve to show the superiority of cylinders over plates, with regard to the quantity of electricity excited by equal rubbed surfaces, I do not pretend to offer a correct solution of the problem, as I am not at all sure of the real cause of the extraordinary difference. The experiments were undertaken more with a view of analysing the actions of each class of machines under different modifications, than of comparing them with each other.

The first experiments were made with a view to ascertain the connexion between the length of the rubber and the quantity of electricity developed. A cylinder machine was therefore fitted with four separate rubbers, whose lengths were respectively 1 inch, 2 inches, 4 inches, and 8 inches. Each rubber was furnished with a silk flap, which reached half round the cylinder to the conductor on the other side, and of a width corresponding to the rubber to which it belonged, the smallest being 1 inch wide, and the largest 8 inches. The collecting points of the conductor were also made moveable, so that just so many might be used as would be suited to the width of the end of the silk flap near to which they were applied. The discharging interval of the coated glass was varied in different sets of experiments, but in all cases it was observed that the quantity of electricity developed was in, as near as possible, an exact ratio with the length of rubber. The length of rubber being multiplied by the circumference of cylinder, and this again by the number of turns, gave the actual amount of surface submitted to friction; and it thus appeared that the length of the silk flap being the same, the quantity of electricity developed was just in proportion to the length of the rubber or the surface rubbed.

The next point which it appeared interesting to examine was the part which the silk flap plays in the excitation of the machine. Much more depends upon this than is usually supposed, and accordingly the same cylinder exhibits very different results with silk flaps of different kinds. Without going into detail of experiments with flaps of various materials, including single and double silk, plain and varnished with shell-lac, oil, caoutchouc, and other materials, it will be sufficient to state the kind which gives the greatest effect. After more than thirty years' experience with some scores of machines, I have found that the thin yellow oiled silk makes the best flap for an electrical machine, when properly prepared for the purpose. This material is best varnished on one side only, the unvarnished side being applied to the glass, and requiring no preparation. If the oiled silk be varnished on both sides, it adheres so closely to the glass as to render it impossible to turn the machine, and it therefore requires to be prepared by giving it two or three coats of shell-lac varnish on the side to be applied to the glass, which will effectually prevent the too strong adhesion. A new flap of this kind improves much in action after the machine has been excited three or four times, evidently showing the gradual attainment of qualities not possessed at first, and moreover proving that the flap itself is a necessary element in the excitation.

The arrangement of the rubber also is a matter of no small importance. It is absolutely necessary that there shall be a perfect contact of the cylinder with the rubber and silk flap, from the face upon which the amalgam is spread to the end where the prime conductor receives the electricity; hence the superiority of oiled silk, the thinness of which enables it to adapt itself to the irregularities of the cylinder.

As a proof of the foregoing remarks, it is merely necessary to try the following experiment.

When the machine is in tolerable action, pass under the silk flap a piece of silk cord, in a direction parallel to the rubber, and at an inch above it, and hold it by the ends so as to touch the cylinder. The silk flap will thus be kept from touching the cylinder in the part where the silk cord is strained across, and this slight separation will be found considerably to impair the action of the machine. The stouter the cord, and the nearer it is held to the rubber, the greater is its influence, the loss of power amounting to one-half or three-fourths of the whole; hence the practice of sewing the silk flap on to the upper edge of the rubber is most objectionable.

The best plan is to attach it to the lower edge of the rubber, and allow it to pass up between the rubber and cylinder. In order to prevent the injury which would accrue to the flap from repeatedly spreading the amalgam upon the surface which covers the rubber, a second piece of silk, which is better not oiled, is attached to the lower edge of the rubber, and turned up over its face, so as to receive the amalgam. When torn, it can be replaced by another piece.

Although any separation of the silk flap from the cylinder in a direction across its length and parallel to the rubber is injurious, yet the folds which form in the silk in the direction of its length, from the rubber to the conductor, do not, unless they are very large, materially interfere with the action of the machine; in order to avoid these, however, the silk flap may be slit in one or more places, longitudinally through its whole length if necessary, thus forming it into several long and narrow flaps, whose edges will occasionally overlap; the flap will thus lie much smoother, and will improve the action of the machine. If, however, the irregularity of the cylinder be such as to cause these edges to recede from each other during its rotation, it would be better to put the silk flap on in separate strips of 3 or 4 inches wide, allowing their sides to overlap each other half an inch or more. The great desideratum is close proximity of the flap with the cylinder.

To ascertain the influence of the flap over the excitation, a cylinder was fitted, having a flap so contrived as to allow of its being shortened to any extent without interfering with the foregoing conditions. The conductor was mounted with a sliding curved arm, which carried the collecting points, and allowed of their being placed just in advance of the termination of the silk flap. The Leyden jar and discharger were attached to the conductor, and the striking interval was adjusted to the desired length; the circumference of the cylinder was 23·5 inches, and the length of the rubber $7\frac{1}{2}$ inches, the surface rubbed at each revolution thus being 176·25 square inches. The effects of the different lengths of silk flap were as follow:—

Length of silk flap from upper edge of rubber.	Number of turns requisite to produce the discharge.	Amount of surface passing against rubber.
inch.		square inches.
1	60	10575
2	34	5992·5
3	24	4230
4	21	3801·25
5	21	3801·25
6	20	3525
7	20	3525
8	20	3525
9	20	3525
10	20	3525

It will thus be observed that very little increase of effect was obtained with more than 4 inches of silk flap, which quantity, with the width of the rubber, altogether about $5\frac{1}{2}$ inches, embraced about a quarter of the circumference of the cylinder. It, however, showed that the flap had something to do with the excitation of the electricity, either by increasing the distance of the collecting points from the rubber, and thereby preventing the too ready return of electricity from the conductor in sparks to the rubber, or by becoming itself an agent in the actual excitation.

Further experiments showed that the silk flap had its own peculiar function, for when the machine was in good action, the rubber could be withdrawn altogether from contact with the cylinder, and the excitation would only gradually diminish after a considerable number of turns; hence it would appear that the rubber commences an excitation which the silk flap prolongs.

The fact that there is no advantage gained by making the silk flap extend over more than one-fourth of the circumference of the cylinder, has enabled me to contrive a machine of double power; for if the ordinary silk flap be cut so as to terminate on the top of the cylinder, a second rubber may be introduced in the place usually occupied by the prime conductor, having its silk flap passing under the lower quadrant of the cylinder, and terminating opposite to the edge of the upper flap. The prime conductor in this case must be branched and placed opposite the end of the cylinder with its two arms in a vertical direction, extending one above, and the other below the cylinder; the framework of the stand which supports that end of the cylinder being made of a transverse bar and two uprights, placed at a sufficient distance from each other to be beyond the influence of the lower branch of the conductor. If, however, the insulation of the additional rubber be not required, the arrangement is more simple, all that is necessary being to connect the rubber with the machine by a curved spring reaching from one of the uprights round the front of the cylinder to the other; in which case, as the pillar, otherwise required to support the rubber, is dispensed with, the conductor may still stand in front of the machine, with its vertical branches embracing the cylinder, one above and the other below. In this case the ends of the branches must be fitted with transverse portions, of a length sufficient to take the collecting points. A machine thus fitted will excite twice as much electricity at each turn as one fitted with a single rubber, the only disadvantage being, that the long zigzag sparks will be rather shorter than with the single rubber, in consequence of the greater proximity of the rubbers to the conductor, and the greater facility of discharge thus afforded from the latter to the former; but even this disadvantage is much diminished when very large cylinders are used.

With cylinders above 12 inches diameter, the advantage of the foregoing arrangement is very great; for with the single rubber, the useless length of silk flap increases the adhesion and friction to such a degree, as not only to render it exceedingly laborious to turn the machine, but also to hazard the fracture of the glass necks of the cylinder, all of which is avoided by the short silk flaps.

The experiments detailed in the present paper have been confined to machines with only one rubber.

Having determined the conditions necessary to produce the strongest electrical excitation in the cylinder machine before mentioned, I was desirous of comparing cylinders of different sizes with each other, and I accordingly adopted the same standard of comparison, viz. a uniform length of discharge from the same Leyden jar. In this way I have compared the action of machines varying from 6 inches to 12 inches diameter, and have found that when they are all constructed with the same care, and the same conditions are as nearly as practicable maintained in all, they afford, within moderate limits, the same quantity of electricity for equal surfaces rubbed; but any considerable inequality in the surface of the glass which shall prevent the rubber from fitting nicely, or the silk flap from lying smoothly, will produce a considerable deterioration in power, amounting to 10 or even 20 per cent., as compared with true and smooth cylinders; thus I have a 12-inch cylinder which has an irregular surface, which excites very little more electricity than a $9\frac{1}{2}$ inch cylinder the surface of which is remarkably true and smooth, although either cylinder is fitted to work in the same frame, and with the same rubber and flap, and both are mounted with equal care.

In experiments of this kind, it is absolutely necessary that the same amalgam should be used, and that those experiments which are to be compared with each other should be performed within moderately short intervals, as the quality of amalgam undergoes remarkable changes in the course of a few months. I have frequently set by a sample as weak and useless, which in the course of a few months has become remarkably energetic, and *vice versâ,* a choice sample has often during the same period become almost useless; indeed it often happens that in making amalgam, two samples will vary as much as 50 per cent. in their qualities, although made at the same time and with every possible provision to make them exactly alike; hence experiments made at different times, and with different samples of amalgam, cannot fairly be compared with each other.

The amalgam which I usually employ, consists of 9 parts of mercury, 5 of zinc, and 3 of tin, the two latter being fused in an iron ladle, and the mercury then added and well stirred. This mixture is then put into a wooden box and shaken for half an hour, or until reduced to a fine black powder, and preserved for use in this form. When used, it is ground up very fine with a small quantity of fresh lard, and laid upon the rubber as smoothly as possible. With this amalgam, a well-constructed machine, having a cylinder of 32 inches in circumference, and a rubber 9 inches in length, with a prime conductor 4 or 5 inches diameter and 20 inches long, should, without the necessity of warming the cylinder, give from four to six straight dense sparks of 3 to $3\frac{1}{2}$ inches in length, for every revolution of the cylinder.

A 2-inch ball inserted into the conductor should throw off spontaneously rapid brushes of electricity into the air, and furnish zigzag sparks of 9 inches or more in length. It should also charge a Leyden jar of ordinary thickness containing 4 square feet of internal surface, so as to discharge at 0·5 of an inch with 48 or 50 turns.

Whatever the size of the machine, excepting perhaps those of very small dimensions, it may be generally assumed that the friction of about 24 square feet should be sufficient to charge a square foot of coated glass to the intensity of 0·5 of an inch.

On comparing these effects with those obtained from a plate machine, we observe an extraordinary difference in relation to the quantity of surface submitted to friction, since the plate machine scarcely yields with four rubbers as much electricity as it might be expected to develope with one. I have been at some pains to analyse the action of the plate machine, and I find, that, with an exceedingly small allowance for the effect of a silk flap without a rubber, the power of the machine is, as nearly as possible, equal to the sum of the effects of the rubbers singly, and that it is immaterial, when both surfaces of the plate are rubbed, whether the conductor collect the electricity from both surfaces or only from one. When a single rubber, however, is applied, there is a slight advantage in collecting the electricity from the excited side of the plate in the proportion of 19 to 18, though the quantity developed from a single rubber is only about one-fourth of that produced by the friction of an equal surface on a cylinder. That the conductor should continue to collect electricity from the surface of the plate which is not rubbed, without accumulating a charge on the rubbed surface, is an anomaly which I am unable to explain, except on the assumption that electrified surfaces conduct electricity, and that it passes round from one side to the other.

By a comparison which I have made of the power of plate electrical machines from the diameter of 18 inches, to that of the large plate machine at the Polytechnic Institution, I find that, when in good excitation, they are pretty nearly alike as to the quantity of electricity developed by the friction of equal surfaces, and whatever their construction, their effects are equivalent to the number of rubbers employed. In proof of this I may quote the following. An excellent double 24-inch plate machine with eight rubbers, each $5\frac{1}{2}$ inches long, and rubbing at each revolution rather less than $18\frac{1}{4}$ feet, requires, when in good excitation, five turns, or 91 square feet to produce a given discharge.

An excellent 3-feet plate machine, the property of Sir W. S. Harris, fitted with four rubbers, each 10 inches long, rubs about 24 square feet at each revolution, and when in good condition requires the friction of 90 square feet to produce the same effect.

I have at various times compared these machines with each other, and with smaller machines, all excited with the same amalgam, and find a remarkable uniformity of result.

It now remains to compare plates with cylinder machines. On one occasion I compared a $7\frac{1}{2}$-inch cylinder, rubbing 176·25 square inches at each revolution, with Sir W. S. Harris's 3-feet plate machine; both were in excellent condition, but not excited with the same amalgam. A standard jar was employed with a Lane's discharger, and a striking interval of 0·5 of an inch. The quantity of surface rubbed by the cylinder to produce the discharge was about fifteen turns, or 18·33 square feet, whilst the plate machine required $3\frac{3}{4}$ turns to produce the same effect. As a single rubber rubbed about 6 square feet, the whole surface would be $6\times4\times3\frac{3}{4}=90$ square feet, or about five times the surface rubbed by the cylinder.

Again, the same cylinder was compared with a double 24-inch plate machine in my own possession, every care being taken to use the same amalgam and maintain the same conditions. In this case the quantity of surface rubbed upon the plate was rather more than four times as much as that on the cylinder.

Thirdly, a $9\frac{3}{4}$-inch cylinder with an 8-inch rubber, rubbing 244 square inches at each turn, produced a given discharge with a friction of 41·36 feet, whilst the double plate machine used at the same time and excited with the same amalgam, required 165 feet to produce the same effect.

It thus appears that cylinder machines have a great superiority in their exciting power over plate machines, and for ordinary experimental purposes are much more convenient. A cylinder of 12 inches diameter, having a single rubber of 9 inches in length, will be just equal to a 24-inch plate machine having four rubbers, each $5\frac{1}{2}$ inches long; or if the cylinder be fitted with two rubbers as before described, it will be equal to a double plate machine which possesses the disadvantage of having eight rubbers to keep in order. In very large plate machines, where a single pair of rubbers only is used, the sacrifice of power in proportion to the size of the machine must be very considerable; and the only advantage which they appear to possess as a compensation for this loss, is that of affording very long sparks from a large conductor.

The cause of this difference in the exciting power of plates and cylinders is not to me perfectly clear. It is possible that the difference in the chemical composition of plate- and flint-glass, as well as the difference in the mechanical character of their surfaces, the latter being a surface of fusion and the former a ground and polished surface, may have some influence over their electric capabilities. There is also another peculiarity to which I have before alluded in connexion with plate- and crown-glass, viz. their retentiveness of the electric charge, which may operate as prejudicially to the free action of electricity excited on its surface, as it does to the free action of a charge communicated by metal coatings when used in the form of a Leyden jar or coated plate. I am more inclined, however, to believe that the cylinder owes its superiority to the fact that the electrical excitation of the interior consequent on the friction of the exterior, is incapable of exercising any controlling inductive action internally, every portion of this inner surface being diametrically opposed to another portion similarly electrified.

The whole of the action is thus determined to the external surface, as is the case with an electrified hollow metal cylinder or sphere, which exhibits scarcely any electrical effects in the interior, though its outer surface appears highly charged. This is not the case with the plate, both of whose surfaces are freely induced upon by surrounding objects.

Any amount of electrical excitation therefore on one side of the plate will require, for the full development of its free action, an equal amount of excitation of the same kind on the other side, in order to compensate for the controlling action of vicinal inducing bodies. Since, however, this will only account for the loss of one-half of the power, there remains still another source of equal loss to be discovered; for, admitting upon this principle of compensation that two rubbers are necessary in order to obtain the full effect of one, we still by no means account for the fact that the quantity actually developed by one pair of rubbers, is only half of what it should be as compared with a cylinder.

XXXVII. *Chemical Notices from Foreign Journals.*

By E. Atkinson, *Ph.D.*

[Continued from p. 117.]

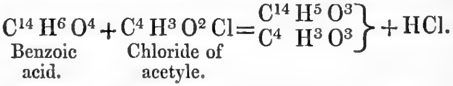
COUMARIC acid, $C^{18}H^8O^6$, stands to hydride of salicyle, $C^{14}H^6O^4$, in the same relation as does cinnamic acid, $C^{18}H^8O^4$, to hydride of benzoyle, $C^{14}H^6O^2$. Both acids experience an analogous decomposition under the influence of fused potash. By the action of chloride of acetyle on hydride of benzoyle, Bertagnini[*] obtained cinnamic acid. Expecting to obtain coumaric acid by a similar decomposition, Cahours[†] tried the action of chloride of acetyle on hydride of salicyle,

$$\underset{\substack{\text{Hydride of}\\ \text{salicyle.}}}{C^{14}H^{6}O^{4}} + \underset{\substack{\text{Chloride of}\\ \text{acetyle.}}}{C^{4}H^{3}O^{2}Cl} = \underset{\substack{\text{Coumaric}\\ \text{acid.}}}{C^{18}H^{8}O^{6}} + HCl.$$

When brought together, these substances act very energetically, hydrochloric acid is evolved, and there is obtained a beautifully crystallized substance, which is, however, not coumaric acid, for it is perfectly neutral. It does not dissolve in dilute potash or ammonia, and solid potash and baryta are without action upon it; it may even be passed over red-hot baryta without undergoing any change. It is insoluble in water, soluble in boiling alcohol, from which it crystallizes on cooling in long needles. Chlorine, bromine, and iodine act upon it, forming crystalline substitution products.

It is isomeric with coumaric acid, and has the formula $C^{18}H^{8}O^{6}$. Cahours names it *Acetosalicyle.*

Benzoic acid yields, when treated with chloride of acetyle, another isomeric body—the anhydrous aceto-benzoic acid,



$$\underset{\substack{\text{Benzoic}\\ \text{acid.}}}{C^{14}H^{6}O^{4}} + \underset{\substack{\text{Chloride of}\\ \text{acetyle.}}}{C^{4}H^{3}O^{2}Cl} = \left.\begin{matrix} C^{14}\,H^{5}\,O^{3} \\ C^{4}\;\;H^{3}\,O^{3} \end{matrix}\right\} + HCl.$$

Cahours had some time ago shown, that by the action of chloride of benzoyle on hydride of salicyle a similar crystallizable compound is obtained. It was known as parasalicyle. Chlorides of cumyle, anisyle, and succinyle act similarly on hydride of salicyle, forming bodies which Cahours names *Cumosalicyle, Anisosalicyle,* and *Succinosalicyle.*

In the same communication, Cahours has described certain iodides of organic radicals of the fatty acid series. They are formed by distilling iodide of phosphorus with carefully dried acetate, butyrate, or valerianate of potash. Strongly fuming liquids of a brownish colour are obtained, which are decolorized by distillation over mercury.

Iodide of acetyle boils between 104° and 105°, is heavier than water, which decomposes it into acetic and hydriodic acids. With alcohol it forms acetic æther. *Iodide of butyryle* and *iodide of valeryle* are perfectly similar to iodide of acetyle: the former boils at 146° to 148° C., the latter at 168° C.

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[*] Phil. Mag. vol. xiii. p. 183.

[†] *Comptes Rendus,* vol. xliv. p. 1252.

Independently of Cahours, but led by the same considerations, Schüler* attempted the formation of coumaric acid by this reaction, but arrived at results differing from those of Cahours. He found on mixing hydride of salicyle with chloride of acetyle, that a brisk action was set up, the mixture became heated, a large quantity of hydrochloric acid was disengaged, and the contents of the tube on cooling solidified to a crystalline mass, which on recrystallization was obtained in brilliant white crystals. These crystals gave on analysis numbers corresponding to the formula $C^{18} H^7 O^4$.

The formation of such a body is inexplicable on the supposition of a simple action between the two substances. Schüler thought the substance not quite pure, and that its true formula would be $C^{18} H^6 O^4$,

$$C^{14} H^6 O^4 + C^4 H^3 O^2, Cl = HCl + 2HO + C^{18} H^6 O^4;$$

but the substance on being carefully purified gave still the same results to analysis.

Schüler endeavoured to prepare a larger quantity of the substance with the view of studying its products of decomposition, and took great care to purify the substances employed in the reaction. But he found that pure chloride of acetyle was entirely without action on hydride of salicyle in the cold. The substances were then heated together in a sealed tube in the water-bath. On opening the tube a stream of hydrochloric acid was given off, and the contents were found to be changed into a brown viscous mass from which nothing crystalline could be extracted. However the experiment with the pure materials was varied, the result was the same.

On adding to a mixture of pure hydride of salicyle and chloride of acetyle, a few drops of terchloride of phosphorus, a very energetic action was set up, hydrochloric acid was given off, and the crystalline substance obtained as in the first experiments. Phosphorous acid acts in a similar manner to terchloride of phosphorus, except that the action is far slower. The crystals obtained were analysed; the mean of several closely-agreeing analyses gave results which are expressed by the formula $C^{18}H^7O^4$, or $C^{36} H^{14} O^8$. The action may take place according to this equation:—

$$2(C^{14} H^6 O^4 + C^4 H^3 O^2 Cl) + PCl^3 = PCl^3 O^2 + 2HCl$$
$$+ 2HO + C^{36} H^{14} O^8.$$

Schüler names the body *acetosalicyle* to denote its origin, and from his description of it there can be no doubt that he and Cahours have obtained the same body.

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* *Journal für Prakt. Chemie,* vol. lxxii. p. 258.

It is only attacked with great difficulty by reagents. Alcoholic potash dissolves it, but it is again precipitated on the addition of water: sulphuric acid dissolves it, the odour of hydride of salicyle becoming perceptible; chloride of zinc has the same action. By nitric and by chromic acids it is converted into picric acid, and by hydrochloric acid and chlorate of potash into a yellow substance resembling chlorinated chinone.

The author was proceeding with an investigation of the action of terchloride of phosphorus on hydride of salicyle and on chloride of acetyle, when he became acquainted with Cahours' results. He proposes to follow the subject in this direction.

By the action of water on trinitroacetonitrile*, Schischkoff obtained† a new crystallizable body which has the formula $C^2(NO^4)^3, NH^4$:—

$$C^4(NO^4)^3N+4HO=2CO^2+C^2(NO^4)^3NH^4.$$

This body is trinitromethylammonium, ór it may be viewed as a compound of trinitromethyle hydrogen with ammonia,

$$C^2(NO^4)^3H, NH^3.$$

When this body is treated with potash, copious evolution of ammònia takes place; treated with concentrated sulphuric acid, an acid substance is liberated, which, when brought in contact with ammonia, regenerates the original substance. This new body has the formula $C^2(NO^4)^3H$; it is *trinitromethyle hydrogen* or *nitroform*. It belongs to the type chloroform, C^2Cl^3H, and is in fact marsh-gas, C^2H^4, in which three atoms of hydrogen are replaced by $3(NO^4)$.

Nitroform is a colourless body, solid below 15° C., crystallizing in cubes, easily soluble in water, to which it imparts a dark yellow colour. It is very combustible and explosive, has an unpleasant smell and a bitter taste. It cannot be distilled at the ordinary pressure: it decomposes at 100° C. It forms crystalline salts of a beautiful yellow colour, which are explosive, and which sometimes decompose spontaneously.

Chloroform is decomposed by potash into formiate of potash and chloride of potassium, and it has been generally assumed that iodoform experiences a similar change by the same treatment. Brüning‡ has shown that this is not the case. Alcoholic solution of potash and iodoform are boiled together for some time in a flask, so connected with a condenser that the vapours condense and flow back into the flask. On afterwards distilling

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[*] Phil. Mag. August 1857.

[†] Liebig's *Annalen*, February and September 1857.

[‡] Ibid. November 1857.

off about one-half, a distillate is obtained which becomes milky on the addition of water. On standing some time, a reddish-coloured oily layer settles to the bottom of the flask, which was washed with water, dried over chloride of calcium and analysed. Numbers were obtained which led to the formula $C^2 I^2 OH$: it is iodoform, $C^2 I^3 H$, in which one equivalent of iodine is replaced by oxygen, $C^2 (I^2 O) H$. Its boiling-point is 181° to 182°, and it crystallizes at $-6°$ C. Its specific gravity is 3·345, the highest of known organic bodies. Like mercury, it does not adhere to glass. When freshly prepared it is colourless, but soon becomes brown from separation of iodine. It volatilizes with the vapour of water. Heated in a sealed tube with potash, it is resolved into iodide of potassium and formiate of potash.

XXXVIII. *On the Elasticity of Carbonic Acid Gas. By* W. J. MACQUORN RANKINE, *LL.D., F.R.SS.L. & E., Regius Professor of Civil Engineering and Mechanics in the University of Glasgow.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN a paper by Mr. Waterston in the Philosophical Magazine for this month (March 1858), page 221, I find a formula of mine for the elasticity of carbonic acid characterized as "*palpably erroneous.*"

That formula is one of a class of formulæ for the elasticity of gases, first proposed by me in a paper which was presented to the Royal Society of Edinburgh in December 1849, read in February 1850, and published partly in the twentieth volume of their Transactions, and partly in the Philosophical Magazine for December 1851. Having from time to time revised the empirical constants contained in those formulæ, so as to adapt them to the advance of experimental knowledge, and having especially altered my estimate of the absolute temperature of melting ice from 274°·6 C. to 274° C., I communicated a revised formula for carbonic acid gas to the Royal Society of Edinburgh in a paper which was read in February 1855. An abstract of that paper, containing the formula with its constants, has appeared in the 'Proceedings' of that Society: but the paper containing the detailed comparison of the formula with experiment is still in the possession of the Society, unpublished; and I therefore beg leave to send you, annexed, the formula itself, extracted from the Proceedings of the Royal Society of Edinburgh for 1854–55, pages 289 and 290, together with the comparison between some of its results and those of M. Regnault's experiments, taken from some papers which are still in my hands. With respect to the employment of my formula by Dr. Joule and Dr. Thomson in their paper "On the Thermal Effects of Currents of Elastic Fluids" (Phil. Trans. 1854), I have to state, that in May 1854, while my own paper was unfinished, I communicated to those gentlemen a formula for carbonic acid gas in which the value of a certain coefficient, denoted by P_0V_0, was stated at about $\frac{1}{120}$th part less than that which I afterwards found to be its correct value; but that difference is of no practically appreciable importance in computing the results of such experiments as those to which Dr. Joule and Dr. Thomson applied the formula.

I have the honour to be, Gentlemen,
Your most obedient Servant,
Glasgow, March 1858. W. J. Macquorn Rankine.

General Formula and Constants for Carbonic Acid Gas.

$$\frac{PV}{P_0V_0} = \frac{\tau}{\tau_0} - \frac{a}{\tau} \cdot \frac{V_0}{V}.$$

P, pressure in lb. per square foot } at the absolute
V, volume of one lb. in cubic feet } temperature τ.
($\tau =$ degrees above melting ice $+ 274°$ C.
$\tau_0 = 274°$ C.).
$P_0 =$ one atmosphere $= 2116{\cdot}4$ lbs. per square foot.
$V_0 = 8{\cdot}15725$ cubic feet (volume which one lb. of carbonic acid would occupy at one atmosphere and 0° C. if it were a perfect gas. The actual volume is 8·101 cubic feet).
$P_0V_0 = 17264$ foot-pounds.
(PV for 0° and one atmosphere, actually = 17145 foot-pounds).
$a = 1{\cdot}9$ for the Centigrade scale.

Comparison with M. Regnault's *experiments on the increase of pressure at constant volume between* 0° *and* 100° *Centigrade.*

Let $P' =$ pressure at 100° C.
Let $P =$ pressure at 0° C.

$\frac{V_0}{V}$.	$\frac{P'-P}{P}$. Calculated.	$\frac{P'-P}{P}$. Observed.	Differences.
1·0045	0·36941	0·36856	+·00085
1·1949	0·37024	0·36943	+·00081
2·3279	0·37521	0·37523	−·00002
4·8748	0·38638	0·38598	−·00040

Comparison with M. Regnault's *experiments on expansion at constant pressure between* 0° *and* 100° C.

Pressures in atmospheres.	Expansion.		Differences.
	Calculated.	Observed.	
1	0·37078	0·37099	−·00021
1	0·37078	0·37190	−·00112
3·316	0·38449	0·38455	−·00006

Comparison with M. Regnault's *experiments on compressibility at constant temperature.*

Let P_1 = initial pressure, V_1 = initial volume;
Let P_2 = final pressure, V_2 = final volume.

P_1. In millimetres of mercury.	$\frac{P_1V_1}{P_2V_2}$. Calculated.	$\frac{P_1V_1}{P_2V_2}$. Observed.	Differences.
1000	1·0089	1·0086	+·0003
2000	1·0179	1·0177	+·0002
4000	1·0365	1·0365	·0
8000	1·0757	1·0799	−·0042

XXXIX. *On the Intensity of Light during the recent Solar Eclipse.*

By J. P. Joule, *LL.D., F.R.S. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Being desirous to obtain an image of the annulus in the late solar eclipse, I took a camera to the Werrington Junction on the Great Northern Railway. A few minutes before the central eclipse, however, it became evident that the sky would remain obscured with cloud, I therefore employed the camera simply to obtain, if possible, a measure of the intensity of the light. The country about the Werrington Junction is an extensive plain. I placed the camera on the ground, directing it to the south-east horizon. The sensitive plate was exposed during the five minutes which preceded the central effect, and the image was developed immediately afterwards. The next day, the weather appearing almost exactly similar, possibly of the two a little more cloudy, I exposed near Manchester plates prepared with the same collodion and nitrate of silver, and developed them with the same solution of sulphate of iron; the direction in which the camera was placed, the time of the day and every other circumstance being as nearly as possible the same as before. In this latter case, a picture, judged by Mr. Dancer, a gentleman of great experience in photography, and myself to be of considerably greater intensity than that procured during the eclipse, was obtained in two seconds.

The ratio of luminous influence, as measured by the camera, was therefore at the greatest 1 to 150. But the average exposed area during the five minutes preceding the central effect, compared with that of the entire disc, will be nearly as 1 to 24. I therefore infer that the circumference of the sun's disc gives out a very weak luminous radiation (at least as measured by a sensitive plate) in comparison with the central part. This observation is quite in accordance with the experience of Mr. Dancer, who finds that in the photography of the sun, the central part is always much more rapidly depicted than the circumference; so much so, that he finds it impossible to obtain in the same image a satisfactory delineation of both parts.

On the eclipse becoming central, the darkness became suddenly increased, remained for a few seconds apparently constant, and then as, or even more, suddenly cleared up. The light at the darkest was evidently enormously greater than that of the full moon, but from my experience in photography, I have no doubt that with it, two hours at the very least would have been necessary to produce an effect equal to that attainable by one second's exposure had the sun been uneclipsed.

I remain, Gentlemen,
Yours respectfully,
James P. Joule.

XL. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 233.]

June 18, 1857.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On a Class of Dynamical Problems." By Arthur Cayley, Esq., F.R.S.

There are a class of dynamical problems which, so far as I am aware, have not been considered in a general manner. The problems referred to (which might be designated as continuous-impact problems) are those in which the system is continually taking into connexion with itself particles of infinitesimal mass (*i. e.* of a mass containing the increment of time *dt* as a factor), so as not itself to undergo any abrupt change of velocity, but to subject to abrupt changes of velocity the particles so taken into connexion. For instance, a problem of the sort arises when a portion of a heavy chain hangs over the edge of a table, the remainder of the chain being coiled or heaped up close to the edge of the table; the part hanging over constitutes the moving system, and in each element of time *dt*, the system takes into connexion with itself, and sets in motion with a finite velocity an infinitesimal length ds of the chain; in fact, if v be the velocity of the part which hangs over, then the length vdt is set in motion with the finite velocity v. The general equation of dynamics applied to the case in hand will be

$$\Sigma\left\{\left(\frac{d^2x}{dt^2}-\mathrm{X}\right)\delta x+\left(\frac{d^2y}{dt^2}-\mathrm{Y}\right)\delta y+\left(\frac{d^2z}{dt^2}-\mathrm{Z}\right)\delta z\right\}dm$$

$$+\Sigma\,(\Delta u\delta\xi+\Delta v\delta\eta+\Delta w\delta\zeta\,\frac{1}{dt}\,d\mu=0,$$

where the first line requires no explanation, in the second line ξ, η, ζ are the coordinates at the time t of the particle $d\mu$ which then comes into connexion with the system; Δu, Δv, Δw are the finite increments of velocity (or, if the particle is originally at rest, then the finite velocities) of the particle $d\mu$ the instant that it has come into connexion with the system; $\delta\xi$, $\delta\eta$, $\delta\zeta$ are the virtual velocities of the same particle $d\mu$ considered as having come into connexion with and forming part of the system. The summation extends to the several particles or to the system of particles $d\mu$ which come into connexion with the system at the time t; of course, if there is only a single particle $d\mu$, the summatory sign Σ is to be omitted. The values of Δu, Δv, Δw are $\frac{d\xi}{dt}-u$, $\frac{d\eta}{dt}-v$, $\frac{d\zeta}{dt}-w$, if by $\frac{d\xi}{dt}$, $\frac{d\eta}{dt}$, $\frac{d\zeta}{dt}$ we understand the velocities of $d\mu$ parallel to the axes, after it has come into connexion with the system; but it is to be observed, that considering ξ, η, ζ as the coordinates of the particle $d\mu$, which is continually coming into connexion with the system, then if the problem were solved and ξ, η, ζ given as functions of t (and, when there is more than one particle $d\mu$, of the constant parameters which determine the particular particle), $\frac{d\xi}{dt}$, &c., in the sense just explained, cannot be obtained by simple differentiation from such values of ξ, &c.: in fact, ξ, η, ζ so given as functions of t, belong at the time t to one particle, and at the time $t+dt$ to the next particle, but what is wanted is the increment in the interval dt of the coordinates ξ, η, ζ of one and the same particle.

Suppose as usual that x, y, z, and in like manner that ξ, η, ζ are functions of a certain number of independent variables θ, ϕ, &c., and of the constant parameters which determine the particular particle dm or $d\mu$, of which x, y, z, or ξ, η, ζ are the coordinates, parameters, that is, which vary from one particle to another, but which are constant during the motion for one and the same particle. The summations are in fact of the nature of definite integrations in regard to these constant parameters, which therefore disappear altogether from the final results. The first line,

$$\Sigma\left\{\left(\frac{d^2x}{dt^2}-\mathrm{X}\right)\delta x+\left(\frac{d^2y}{dt^2}-\mathrm{Y}\right)\delta y+\left(\frac{d^2z}{dt^2}-\mathrm{Z}\right)\delta z\right\}dm,$$

may be reduced in the usual manner to the form

$$\Theta\delta\theta+\Phi\delta\phi+\ldots.$$

where, writing as usual θ', ϕ', &c. for $\frac{d\theta}{dt}, \frac{d\phi}{dt}$, &c.,

we have

$$\Theta = \frac{d}{dt}\frac{d\mathrm{T}}{d\theta'} - \frac{d\mathrm{T}}{d\theta} + \frac{d\mathrm{V}}{d\theta},$$

$$\Phi = \frac{d}{dt}\frac{d\mathrm{T}}{d\phi'} - \frac{d\mathrm{T}}{d\phi} + \frac{d\mathrm{V}}{d\phi}, \text{ \&c.,}$$

(this supposes that $\mathrm{X}dx + \mathrm{Y}dy + \mathrm{Z}dz$ is an exact differential); only it is to be observed that in the problems in hand, the mass of the system is variable, or what is the same thing, the variables θ, ϕ, &c. are introduced into T and V through the limiting conditions of the summation or definite integration, besides entering directly into T and V in the ordinary manner. And in forming the differential coefficients $\frac{d}{dt}, \frac{d\mathrm{T}}{d\theta'}, \frac{d\mathrm{T}}{d\theta}, \frac{d\mathrm{V}}{d\theta}$, &c., it is necessary to consider the variables θ, ϕ, &c., in so far as they enter through the limiting conditions *as exempt from differentiation*, so that the expressions just given for Θ, Φ, &c., are, in the case in hand, rather conventional representations than actual analytical values; this will be made clearer in the sequel by the consideration of the before-mentioned particular problem.

Considering next the second line, or

$$\Sigma\left\{\left(\frac{d\xi}{dt} - u\right)\delta\xi + \left(\frac{d\eta}{dt} - v\right)\delta\eta + \left(\frac{d\zeta}{dt} - w\right)\delta\zeta\right\}\frac{1}{dt}d\mu,$$

we have here

$$\begin{aligned}\delta\xi &= a\ \delta\theta + b\,\delta\phi + \ldots\\ \delta\eta &= a'\,\delta\theta + b'\delta\phi + \ldots\\ \delta\zeta &= a''\delta\theta + b'\delta\phi + \ldots,\end{aligned}$$

where a, b, a', &c. are functions of the variables θ, ϕ, &c., and of the constant parameters which determine the particular particle $d\mu$. The virtual velocities or increments $\delta\theta$, $\delta\phi$, &c., are absolutely arbitrary, and if we replace them by $d\theta$, $d\phi$, &c., the actual increments of θ, ϕ, &c., in the interval dt during the motion, then $\delta\xi$, $\delta\eta$, $\delta\zeta$ will become $\frac{d\xi}{dt}dt$, $\frac{d\eta}{dt}dt$, $\frac{d\zeta}{dt}dt$, in the sense before attributed to $\frac{d\xi}{dt}, \frac{d\eta}{dt}, \frac{d\zeta}{dt}$.

The particle $d\mu$ will contain dt as a factor, and the other factor will contain the differentials, or as the case may be, products of differentials of the constant parameters which determine the particular particle $d\mu$. We have thus the means of expressing the second line in the proper form; and if we write

$$\begin{aligned}\Sigma\,(a^2 + a'^2 + a''^2)\,d\mu &= \mathrm{A}dt\\ \Sigma\,(b^2 + b'^2 + b''^2)\,d\mu &= \mathrm{B}dt\\ &\vdots\\ \Sigma\,(ab + a'b' + a''b'')\,d\mu &= \mathrm{H}dt\\ \Sigma\,(au + a'v + a''w)\,d\mu &= -\mathrm{P}dt\\ \Sigma\,(bu + b'v + b''w)\,d\mu &= -\mathrm{Q}dt,\\ &\vdots\end{aligned}$$

then the required expression of the second line will be

$$(\mathrm{A}\theta'+\mathrm{H}\phi' \,..\, +\mathrm{P})\,\delta\theta+(\mathrm{H}\theta'+\mathrm{B}\phi' \,..\, +\mathrm{Q})\delta\phi+\ldots$$

which, if we put

$$\mathrm{K}=\tfrac{1}{2}\,(\mathrm{A}\theta'^2+\mathrm{B}\phi'^2+\,..\,+2\mathrm{H}\theta'\phi'+\,..\,+2\mathrm{P}\theta'+2\mathrm{Q}\phi'+\,..\,),$$
$$=\tfrac{1}{2}\,(\mathrm{A},\ \mathrm{B},\ ..\ \mathrm{H},\ ..\ \mathrm{P},\ \mathrm{Q},\ ..\ \big)\!\!\big(\theta',\ \phi',\ \ldots,\ 1)^2,$$

may be more simply represented by

$$\frac{d\mathrm{K}}{d\theta'}\,\delta\theta+\frac{d\mathrm{K}}{d\phi'}\,\delta\phi+\ldots$$

Only it is to be remarked that A, B, .. H, .. P, Q, .. will in general contain not only θ, ϕ, .., but also the differential coefficients θ' ϕ', .., and that in forming the differential coefficients $\frac{d\mathrm{K}}{d\theta'}$, $\frac{d\mathrm{K}}{d\phi'}$, &c., the quantities θ', ϕ', .., in so far as they enter into K, not explicitly, but through the coefficients A, &c., must be considered *as exempt from differentiation*, so that the preceding expression for the second line by means of the function K is rather a conventional representation than an actual analytical value.

Uniting the two lines, and equating to zero, the coefficients of $\delta\theta$, $\delta\phi$, &c., we obtain finally the equations of motion in the form

$$\frac{d}{dt}\frac{d\mathrm{T}}{d\theta'}-\frac{d\mathrm{T}}{d\theta}+\frac{d\mathrm{V}}{d\theta}+\frac{d\mathrm{K}}{d\theta'}=0,$$
$$\frac{d}{dt}\frac{d\mathrm{T}}{d\phi}-\frac{d\mathrm{T}}{d\phi}+\frac{d\mathrm{V}}{d\phi}+\frac{d\mathrm{K}}{d\phi}=0,$$

where the several symbols are to be taken in the significations before explained.

In the particular problem, let z be measured vertically downwards from the plane of the table, then $\mathrm{Z}=g$, and repeating for the particular case the investigation *ab initio*, the general equation of motion is

$$\Sigma\left(\frac{d^2z}{dt^2}-g\right)\delta z\,dm+\frac{d\zeta}{dt}\,\delta\zeta\,\frac{1}{dt}\,d\mu=0.$$

Let s be the length in motion, or, what is the same thing, the z coordinate of the lower extremity; and suppose also that the mass of a unit of length is taken equal to unity, we have $\delta z=\delta s$, $\frac{d^2z}{dt^2}=\frac{d^2s}{dt^2}$, $dm=dz$, and the summation or integration with respect to z is from $z=0$ to $z=s$, whence

$$\Sigma\left(\frac{d^2z}{dt^2}-g\right)\delta z\,dm=\left(\frac{d^2s}{dt^2}-g\right)\delta s\ \Sigma dz=\left(\frac{d^2s}{dt^2}-g\right)s\,\delta s\,;$$

which is of the form

$$\left(\frac{d}{dt}\frac{d\mathrm{T}}{ds'}-\frac{d\mathrm{T}}{ds}+\frac{d\mathrm{V}}{ds}\right)\delta s,$$

if

$$\mathrm{T}=\tfrac{1}{2}s'^2.\,\bar{s},\ \ \mathrm{V}=-gs\bar{s},$$

where the bar is used to denote exemption from differentiation, but ultimately $\bar{s}$ is to be replaced by s. Considering now the second term here $\zeta=0$, but $\delta\zeta=\delta s$, and thence $\frac{d\zeta}{dt}=s'$. Moreover, $d\mu=s'dt$, and thence finally the second term is s'^2, which is of the form $\frac{dK}{d\bar{s'}}$, if

$$K=\tfrac{1}{2}\bar{s'}\,.\,s'^2,$$

the bar having the same signification as before, but after the differentiation $\bar{s'}=s'$. The resulting equation is

$$\left(\frac{d^2s}{dt^2}-g\right)s+\left(\frac{ds}{dt}\right)^2=0,$$

which may be written in the form

$$s\frac{ds}{dt}\,d\left(s\frac{ds}{dt}\right)=gs^2ds,$$

and the first integral is therefore

$$\frac{sds}{\sqrt{s^3-a^3}}=\sqrt{\frac{2g}{3}}\,dt,$$

where a is the length hanging over at the commencement of the motion. If $a=0$, then the equation is

$$\frac{ds}{\sqrt{s}}=\sqrt{\frac{2g}{3}}\,dt,$$

and integrating from $t=0$, $2\sqrt{s}=\sqrt{\frac{2g}{3}}\,t$, or finally $s=\frac{1}{6}gt^2$, so that the motion is the same as that of a body falling under the influence of a constant force $\frac{1}{3}g$. It is perhaps worth noticing that the differential equation may be obtained as follows:—We have, in the first instance, a mass s moving with a velocity s', and after the particle ds $(=s'dt)$ has been set in motion, a mass $s+s'dt$ moving say with a velocity $s'+\delta s'$, whence neglecting for the moment the effect of gravity on the mass s, the momentum of the mass in motion will be constant, or we shall have

$$ss'=(s+s'dt)\,(s'+\delta s')=ss'+s'^2dt+s\delta s',$$

and therefore $s\delta s'=-s'^2dt$. Hence, adding on the right-hand side the term $gsdt$ arising from gravity, and substituting $\frac{d^2s}{dt^2}dt$ for $\delta s'$, we have the equation $s\frac{d^2s}{dt^2}=gs-\left(\frac{ds}{dt}\right)^2$ as before.

"On the Causes and Phænomena of the Repulsion of Water from the Feathers of Water-Fowl and the Leaves of Plants." By George Buist, D.C.L., of Bombay, F.R.S.

"Experimental Researches on the Conductive Powers of various Substances, with the application of the Results to the Problem of Terrestrial Temperature." By William Hopkins, Esq., M.A., F.R.S.

1. The author remarks, that in giving an account of these experimental researches, it is first necessary to define strictly the manner in

which the *conductivity* or *conducting power* of a substance with reference to heat, is accurately *measured*. For this purpose, conceive the conducting substance to be bounded by two parallel plane surfaces of indefinite extent, the distance between them being h. Suppose one of these bounding surfaces (which, for convenience, may be called the *lower* one) to be kept at a uniform and constant temperature t_1; let the temperature of the *upper* surface be also constant and uniform, and equal to t_2; and let τ denote the temperature of the free space into which the heat radiates from the *upper* surface. Then, if we denote the *conducting power* of the substance by k, and the *radiating power* of its upper surface by p, we obtain by mathematical investigation,

$$\frac{k}{p}=\frac{t_2-\tau}{t_1-t_2}h.$$

It is here supposed that k is independent of the temperature of the substance, and that p is equally independent of that of the surface from which the radiation takes place. It may also be remarked, that the quantity of heat which radiates from a unit of area in a unit of time, is measured by the product of p, and the difference of the temperatures, t_2 and τ, of the radiating surface and surrounding medium. It is the ratio $\left(\frac{k}{p}\right)$ which the conducting bears to the radiating power, which has more frequently been determined in researches of this kind; but this would not have sufficed for the author's object, which has been the determination of the values of k for different substances. The radiating power (p) probably varies for different substances as much as the conductive power (k), but all consideration of the former power will be avoided if we suppose the radiating surface of the substance to be covered with a thin layer of some given substance which shall take the temperature of the upper surface of the substance itself, and from which the radiation shall always take place, whatever be the nature of the substance experimented on. Thus if c denote the radiating power of the superimposed thin layer (which was mercury in these experiments), we shall have

$$\frac{k}{c}=\frac{t_2-\tau}{t_1-t_2}h;$$

a formula which (c being always the same) enables us to compare the conducting powers for different substances, or to determine their absolute numerical values when that of c is once determined. In the actual experiments some error was necessarily superinduced by the necessity of working with portions of the different substances of comparatively small instead of indefinitely large horizontal extent, such as strict mathematical accuracy would require. This error, however, was undoubtedly small, and, moreover, can have had extremely little effect on the *relative* values of k, since it must have been nearly the same for all the substances on which the experiments were made.

The apparatus made use of was sufficiently simple. The heat was derived from a stove, the fire within which could be elevated, depressed, or entirely withdrawn at pleasure. A very shallow pan of mercury was placed over the stove, the fire being so regulated as to preserve the mercury at any constant required temperature. A cylindrical block of any substance, the conductive power of which was to be determined, was so placed as to rest with its base just in contact with this mercury, from which it derived its temperature (t_1). Its upper end was also covered with sufficient mercury just to cover the small bulb of a thermometer. The temperature of this latter mercury gave t_2. Careful arrangements were made for observing these temperatures, as well as that of the air into which the heat radiated from the upper mercury. Precautions were also taken to prevent the lateral transference of heat through the sides of the block, and any influence of radiation from the heated stove which might affect the results of the experiments. When the temperature (t_2) of the upper mercury became stationary, the experiment was completed, and the substitution of this stationary value of t_2, together with the values of t_1 and τ in the above formula, gave the numerical results required.

2. The following were some of the results obtained for conductive powers as measured by the ratio $\frac{k}{c}$:—

Chalk	·056
Clay	·07
Sand	·15
Sand and clay	·11.

These substances were all in the state of *very dry powder*. In the last case the sand and clay were in equal quantities.

Substances in the state of rock-masses.

(1) *Calcareous rocks.*	
Chalk (same block from a dry state to a state of saturation with water) from	·17 to ·30
Oolites from Ancaster (dry to saturated)	·30 to ·40
Hard compact limestones	·50 to ·55
(2) *Argillaceous substances.*	
Clay, very dry to very moist	·23 to ·37
(3) *Siliceous rocks.*	
New red sandstone (same block dry to saturated)	·25 to ·60
Freestone	·33 to ·45
Hard compact sandstones (Millstone-grit)	·51 to ·76
(4) Hard, compact, old sedimentary rocks	·50 to ·61
(5) Igneous rocks	·53 to 1·00

Effect of Pressure.

3. This effect was not appreciable for a pressure of 7500 lbs. per square inch in such substances as bees'-wax and spermaceti. Nor

was there any sensible effect with chalk between a pressure of 4300 lbs. and 7500 lbs. per square inch.

Clay which when incompressed had a conducting power $=\cdot 26$, had when compressed with 7500 lbs. per inch, a power $=\cdot 33$; and the conducting power of a mixture of sand and clay in equal quantities rose from ·36 to ·378 by an increase of pressure from 4300 lbs. to 7500 lbs. per inch.

Generally the effect of pressure is much less than might have been anticipated.

Effect of Discontinuity.

4. When the conducting mass consists of a number of strata superimposed on each other, the mathematical problem presented to us requires a distinct investigation, which is here given under a very general form, together with the experiments necessary to determine the effect of this kind of discontinuity. The result is that if a mass of sandstone consisted of a number of strata, the conducting powers of which should be about ·5, the mean conductivity of the whole would not be diminished by more than about $\frac{1}{20}$th part, supposing the average thickness of strata to be 1 foot; or by about $\frac{1}{10}$th, if that average thickness should be 6 inches. This effect is much less than might possibly have been anticipated.

Effect of Moisture.

5. This effect was very considerable in those rocks which are great absorbents of water. The maximum effect appears to be produced by a quantity of moisture which falls considerably short of producing complete saturation. The conducting power of a piece of dried chalk was $=\cdot 19$, but became $=\cdot 30$ when the substance was very moist. That of a well-dried piece of new red sandstone was $=\cdot 25$, but became as much as ·60 when saturated. Both these substances absorbed a large quantity of water. Ancaster oolites absorbed considerably less, and their conductivity was affected in a smaller degree. For a block of dry clay the conductive power was ·23, and became ·37 when well moistened. Close indurated sandstone, palæozoic rocks of close texture, and igneous rocks are bad absorbents, and are very little affected in their conductive powers by moisture.

Comparison of Deductions from Theories of Terrestrial Temperature with the Results of Observation.

6. It has long been established by mathematical investigation, that if a large globe like the Earth be heated in any manner and in any degree, its temperature at points not too remote from its surface, and after a sufficient lapse of time, will necessarily become such that the increase of temperature in descending along a vertical line will be proportional to the increase of depth. In this enunciation, however, it is assumed that the conductive power throughout the mass, or at least throughout its more external portion, is uniform. The difference of conductive power between the unstratified and sedimentary

portion of the earth's crust, or that between one sedimentary portion and another, has not hitherto been taken into account*. The author has investigated the problem assuming the crust of the globe to consist of any number of strata of different conductive powers and bounded by parallel surfaces, the problem being much simplified by considering their surfaces as plane instead of spherical. Then, assuming the temperature of the crust of the globe to be due entirely to the transference of heat from its central portions to its surface, it is shown that the increase of temperature in descending vertically through any two strata, ought to be in the inverse ratio of the conductive powers of those strata, whether the two strata belong to the same group of stratified beds, or to two different groups in different localities. Such at least must be the result unless we introduce very arbitrary and, as the author conceives, entirely inadmissible hypotheses into the problem.

For the purpose of testing this theory in its application to our own globe, four or five cases of Artesian wells and vertical shafts are especially referred to, in which the temperature has been carefully observed at greater depths than at any other places in Western Europe, or probably in any other part of the globe†. The cases spoken of are the following:—

(1) An Artesian well near Geneva.—Depth = 225 metres; increase of depth for 1° (F.) = 55 feet.

(2) An Artesian well at Mondorff in the Grand Duchy of Luxembourg.—Depth = 730 metres; increase of depth for 1° (F.) = 57 feet.

(3) An Artesian well at New-Saltzwerk in Westphalia.—Depth = 644·5 metres; increase of depth for 1° (F.) = 54 feet.

(4) The Puis de Grenelle at Paris.—Depth = 546 metres; increase of depth for 1° (F.) = 60 feet.

(5) A coal shaft at Duckenfield, near Manchester.—Depth = 1400 feet; increase of depth for 1° (F.) = 65 feet.

(6) A coal shaft at Monkwearmouth.—Depth about 1700 or 1800 feet; increase of depth for 1° (F.) about = 60 feet.

The general rate of increase of temperature in our own deeper coal-mines is that of about 1° (F.) for 60 feet in depth; and the same result has been obtained for many parts of the chalk in Northern France.

These cases present a remarkable approximation to uniformity, whereas the conductive powers of the strata which have been penetrated are very different. Cases (4) and (5) present the best means of comparison. The Puis de Grenelle passes through nearly 500 metres of chalk, the conducting power of which is estimated by the author at somewhat more than ·25, while the mean conducting power of the rocks through which the coal shaft at Duckenfield passes, is estimated, by means of experiments performed on specimens of these rocks, at rather more than ·5. This is about twice as

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* Except in the case in which Poisson investigates the state of temperature of a sphere surrounded by a single concentric spherical shell of different conductivity.

† In a great majority of instances observations of this kind have not been made with sufficient care to be relied on.

much as in the former case, whereas the depths corresponding to the same increase of temperature are only as 65 to 60, instead of being in the ratio of about 65 to 35, as they ought to be according to the theory here considered. In all the other cases the conductive powers of the masses penetrated are doubtless greater than that of the chalk at Paris, though, for the most part, they present a *more* rapid increase of temperature in descending, instead of a *less* rapid increase (as this theory would prescribe) than the Puis de Grenelle.

Within the region comprising the cases above cited, there are many local variations as to the rate of increase of terrestrial temperature in descending below the earth's surface. The author conceives that these phenomena cannot be accounted for according to this theory without the introduction of arbitrary hypotheses.

Upon the whole, he believes that in the present state of our knowledge of terrestrial temperature, it is impossible to account for its phenomena by regarding them as the consequence simply of heat, not generated in, but transmitted through the crust of the globe from some deep-seated central source.

The discrepancy between the actual terrestrial temperatures and those which would be assigned by the theory here discussed, may be illustrated perhaps by placing the subject in a rather different point of view. It is assumed in the theoretical investigation, that the isothermal surfaces at depths sufficiently great (as 50 or 100 miles for example) are approximately concentric with the earth's external surface, or, speaking with reference to areas not too large, parallel to that surface, in which case it is proved that the isothermal surfaces at comparatively small depths (not much exceeding that of the sedimentary beds) cannot be parallel to the external surface. For example, the depth of an isothermal surface of given temperature which should be some 3000 feet at the Puis de Grenelle, ought to be nearly 6000 feet at the coal shaft at Duckenfield; and at other places it ought to be very nearly proportional to the conductive power of the terrestrial mass lying above it. But the observations above cited demonstrate that, independently of local irregularities, such an isothermal surface is nearly at equal depths throughout the whole region of Western Europe.

No theory of terrestrial temperature, then, can meet the requirements of observation which does not account for isothermal surfaces approximately parallel (with local variations) to the earth's external surface at comparatively small depths beneath it. Moreover, it is easily shown that the quantity of heat transmitted from such a surface to the external surface, must be proportional to the conductive power of the superincumbent mass through which the transmission takes place (in the previous case the quantity of transmitted heat is independent of that power). Consequently, whatever may be the cause supplying the heat at depths not much exceeding the general aggregate depth of the sedimentary beds, it must furnish a *quantity of heat* proportional to the vertical flow of heat, *i. e.* a quantity proportional to the conductive power of the superincumbent mass. Thus the energy of the producing cause must have distinct relations to *superficial conditions*. Must not, then, the cause itself be at least partly *superficial*, and not entirely *central*? The author is convinced that such must be the case. He does not profess, in this paper, to carry his speculations further.

It should be remarked that the argument derived from the above investigations is not directly against the theory of a *primitive* heat, but only against the manifestation of the remains of such heat as the sole cause of existing terrestrial temperatures in the superficial crust of the globe, at depths beyond the sensible effect of the direct solar heat. Whatever may be the weight of the argument in favour of the earth's original fluidity (and therefore of its primitive heat), founded on the oblateness of its form, for example, the cogency of such argument remains unaltered. At the same time, all the collateral arguments in favour of primitive heat, founded on the existing temperature of the earth's crust, or the climatal changes which are believed to have taken place on its surface, are deprived, the author conceives, of nearly all their weight. Moreover, admitting only a part of the existing terrestrial heat to be due to superficial causes, the flow of heat from the earth's central portions must be less by that amount than if the whole flow were due to central heat. Consequently the rate of increase of terrestrial temperature *due to the flow of central heat* must be proportionally diminished, and the depth at which we should arrive at the temperature of fusion proportionally increased. The conclusion, therefore, that the earth's solid crust is as thin as some geologists have supposed it to be, as well as all theories based on that conclusion—whether of volcanic action, or of elevation and depression of the earth's surface—must be deprived of nearly all their force.

The remainder of this paper contains details of experiments, and descriptions of the apparatus used in making them.

"On the Perihelia and Nodes of the Planets." By Edward J. Cooper, Esq., F.R.S.

Early in the year 1855 I had the honour to transmit to the Royal Society a paper on the distribution of the perihelia and ascending nodes of the then discovered planets, which was read at the meeting of the Society held on the 8th of March in that year. In that paper I called attention to my first notice of the phenomena in the Preface to my little work on Cometic Orbits.

Ten asteroids having been since added to the number, I requested my first assistant, Mr. Graham, to include them in a new distribution of the perihelia and nodes, and he has just reported to me the results. Instead, however, of following precisely the same semicircles which I adopted, he referred in the first instance to the larger planets, with a view to ascertain whether or not in the small planets there were an apparent preference for the heliocentric semicircles in which the perihelia and nodes of the majority of the larger planets are found. Thus,—

For large Planets.

Longitude of Perihelion, 1856·0.	Longitude of ascending Nodes.
7 from 12° 1′ to 168° 19′	7 from 46° 36′ to 130° 12′
1 at 333° 24′	0 elsewhere
Middle point of Arc containing greatest number.	
90° 10′	88° 24′

Mean of these........ 89° 17′

Taking this in round numbers=90°, and dividing the asteroids into three groups in the order of their discovery, we have

	Perihelia.			Ascending Nodes.		
		0° to 180°.	180° to 360°.		0° to 180°.	180° to 360°.
	14	10	4	14	11	3
	14	10	4	14	10	4
	15	9	6	15	7	8
	43	29	14	43	28	15
Large	8	7	1	7	7	0
	51	36	15	50	35	15

In the case of the perihelia no other two semicircles give a greater disproportion between the numbers. The semicircle 355° to 175° contains 37 of the nodes, the opposite one 13.

But in addition to this development of my original plan regarding the heliocentric longitudes of the perihelia and nodes, Mr. Graham has found a remarkable coincidence between the foregoing numbers and the periods of the discovery of the small planets. It appears that 28 have been detected between the vernal and autumnal equinoxes, and only 15 in the other half-year.

He states that this circumstance, which at a first glance might seem to throw some light upon the facts, proves, after a moment's consideration, the exact opposite to what might have been expected, at least in its bearing on the perihelia, for

	180° to 360°.	0° to 100°.
Longitudes of Aphelia	29	14
Longitudes of descending Nodes ..	28	15
Point of Ecliptic in opposition at date of discovery	28	15

"If, then," Mr. Graham adds, "there be any connexion between these results, it is not easy to imagine why discoveries should be more frequent near the *descending* node; and it is quite contradictory that there should be a greater facility of finding the planets in the more remote parts of their orbits." Upon these facts I abstain from making any comment, excepting that the present data tend to strengthen the conviction that some physical cause, as yet unapplied to these phenomena, may be in operation. Appended to the paper are two diagrams, bringing before the eye more clearly than numbers, the heliocentric places of the perihelia and nodes which are the subjects of this notice.

GEOLOGICAL SOCIETY.

[Continued from p. 235.]

December 16, 1857.—L. Horner, Esq., V.P.G.S., in the Chair.

The following communication was read:—

"On a remarkable fossil Specimen belonging to the Genus *Neuropteris*, from the Coal-measures of Lancashire, and Remarks on that Genus." By C. J. F. Bunbury, Esq., F.R.S., F.G.S.

The author begins by noticing the comparative rarity, in a fossil state, of the young half-expanded fronds of Ferns, showing the characteristic *circinate vernation*; and he remarks that the specimens in that state, hitherto figured, belong to the genus *Pecopteris*. He then describes a well-characterized specimen of *Neuropteris* in this circinate condition; it appears to belong to *N. gigantea*, or a variety of it, and was procured from Oldham in Lancashire. This specimen affords a strong confirmation of the opinion, that the fossil Neuropterides were really Ferns, which some have been tempted to doubt, in the absence of any knowledge of their fructification. This specimen shows that they had the characteristic vernation of Ferns; in particular, it shows a striking agreement in structure with the young fronds of *Aspidium exaltatum*. It is thus clear, at any rate, that *Neuropteris* did not belong to the Coniferous Order, in which there never is any approach to the circinate vernation; even in *Salisburia*, the leaves of which have, in their form and veining, so much the appearance of a Fern, their arrangement in the young state is quite different. The only flowering plants which can be compared with Ferns in this respect are the Cycadeæ; and in the absence of fructification it is not easy to prove positively that *Neuropteris* may not have belonged to that family. It is most probable, however, from the composition of the frond, the veining, texture, and all the characters together, that these fossil plants were true Ferns. To determine their nearest affinities in that family is hardly in our power, as there seems to be no constant relation between the vernation or other external characters and the fructification.

The genus *Neuropteris* is chiefly characteristic of the Coal-measures. The author has scarcely seen a genuine species of it from any formation later than the Trias, unless we except the enigmatical Anthracitic beds of the Alps, which afford several species apparently identical with those of the Coal. The Oolitic species referred to this genus by Lindley and Hutton do not agree with its characters. Two species, *Neuropteris Loshii* and *N. tenuifolia*, appear to be common to the Carboniferous and Permian systems.

The author then points out, that, owing to the variations in different parts of the same frond (variations corresponding to those in many recent Ferns), the described species of *Neuropteris* have been too much multiplied; and he concludes with critical observations on a few of them.

January 6, 1858.—Major-General Portlock, LL.D., President, in the Chair.

The following communications were read:—

1. "On *Cephalaspis* and *Pteraspis*." By Prof. Huxley, F.R.S., F.G.S.

Of the four species originally included by Prof. Agassiz in the genus *Cephalaspis*, two, *C. Lloydii* and *C. Lewisii*, are so different from the others that the possibility of their proving generically distinct is hinted at in the 'Recherches sur les Poissons Fossiles.'

Subsequently M. Kner endeavoured to prove that these two species are not fish-remains at all, but are the internal shells of a Cephalopod, for which he proposed the generic name of *Pteraspis*.

Roemer has still more recently expressed the opinion that the *Pteraspides* are *Crustacea*. These conflicting opinions clearly indicate the necessity of revising and comparing anew the characters of the different species of *Cephalaspis* and *Pteraspis*. And a still greater interest is lent to the inquiry into the true nature of *Pteraspis*, from the fact that species of this genus are now known to occur in undoubtedly Upper Silurian rocks. As the evidence stands at present, they are, if fish, among the oldest (and nearly the very oldest) representatives of their class.

In undertaking this inquiry, the author of the present paper considered it desirable, in the first place, to determine with precision the microscopical characters of the shield of *Cephalaspis*. This shield is exceedingly thin, nowhere exceeding $\frac{1}{40}$th of an inch in thickness on the dorsal surface, and on the ventral suddenly thinning off a little way from the margin into a mere membrane.

The subjacent cranium appears to have been wholly composed of cartilaginous or soft fibrous tissue; for the "layer of fibrous bone," which has been said to exist immediately beneath the shield, is in reality nothing more than the matrix, which in these fossils, as in others, is stained of a deep reddish-brown colour in the immediate neighbourhood of the animal substance; the "fibres" of the supposed bone are casts of the radiating semi-canals or grooves on the under surface of the shield.

The shield consists of three principal layers; the outermost is distinctly laminated, and contains numerous osseous lacunæ, whose long axes are disposed at a considerable angle to one another in the successive layers, as in *Megalichthys*. The lamellæ and lacunæ disappear in the middle and outer layers. The latter is arranged in irregular tubercles, consisting of a substance very similar to the "Kosmine" of Prof. Williamson. The inner openings of numerous vascular canals are seen as points scattered over the inner surface of the shield. These canals traverse the inner layer obliquely, and then ramify in the middle layer in a very peculiar manner, described at length in the paper.

It is from the disposition of these vascular ramifications that the appearance of distinct ossicles or scales, interlocking by sutures, which has been described, arises. The entire absence of any such appearance of sutures on the inner surface of the shield is, indeed, alone sufficient to prove that it is not composed of distinct scales.

In the shield of *Pteraspis* three principal layers are similarly discoverable: the inner is very distinctly laminated; the outer, almost wholly constituted by the characteristic "enamel-ridges," consists of Kosmine. Vascular canals pass from the inner surface, and ramify in the middle layer, terminating in cæca in the outer layer, as in *Cephalaspis*.

But there are no osseous lacunæ; and the vascular canals communicate with large polygonal cells (which were either empty, or more or less occupied by membranous substance in the recent state) situated in the inner part of the middle layer.

Specimens were exhibited in which these cellular cavities were empty; but ordinarily they are filled with the matrix, which then assumes the form of polygonal prisms separated by the thin walls of the cells. It is these prisms which have been mistaken for part of the bony structure itself.

On examining a thin section of one of M. Kner's specimens (for which the author is indebted to the liberality of Sir P. Egerton), the structure, though much altered, showed sufficient similarity to that of the specimens of *C. Lloydii* in the Museum of the Society to leave no doubt as to the generic identity of the two.

The microscopic examination of *Pteraspis* demonstrates its unquestionably piscine nature; and shows that, while in many respects similar to *Cephalaspis*, the species included under *Pteraspis* are rightly separated from the others. The leading distinctive characters of the former are the absence of osseous lacunæ,—the cellular character of the middle layer,—and the ridged and not tuberculated enamel.

In conclusion, the author inquired into the evidence of the Ganoid nature of the *Cephalaspidæ*, and into the value of the relative and absolute development of the endo- and exo-skeletons in fishes, considered as indications of the perfection of their general organization.

2. "On a New Species of *Plesiosaurus*; with Remarks on the Structure of the Atlas and Axis, and of the Cranium in that genus." By Prof. Huxley, F.R.S., F.G.S.

The specimen which is the subject of the present paper was procured at Street, near Glastonbury. It is now in the Collection of the Museum of Practical Geology, Jermyn Street, and it will be described at length in the Decades of the Geological Survey.

It approaches most nearly to *P. Hawkinsii*; but the head is smaller in proportion to the body and neck, and the number of the cervical and dorsal vertebræ is different, there being altogether fifty-three cervico-dorsal vertebræ, of which thirty are cervical; while in *P. Hawkinsii* the cervical vertebræ are thirty-one, and the dorsal at least twenty-three. For this species, characterized by fifty-three cervico-dorsal vertebræ,—by a cranium at most not more than $\frac{1}{12}$th of the length of the body,—and by having the anterior thirty vertebræ fully, or more than, equal to four lengths of the cranium, the name of *P. Etheridgii* is proposed. Its dimensions are nearly the same as those of *P. Hawkinsii*, its length being between 7 and 8 feet.

By a happy accident the only displacement in the whole length of the vertebral column of this specimen has taken place between the head and the atlas and axis, on the one hand, and between the latter and the third cervical vertebra on the other. By a little careful clearing away of the surrounding parts, it has thus been possible to expose the atlas and axis very easily. They are, as Prof. Owen has stated to be their character in this genus, anchylosed; but their structure is totally different from what is seen in the *Ichthyosaurus*, and closely resembles that of the corresponding parts in the Crocodile. An os odontoideum, very similar to that in the Crocodile, represents, as Rathke long since demonstrated in other *Reptilia*, the central portion of the body of the atlas; while its cortical inferior portion and its neural arches form an anterior articular cup for the occipital condyle, as in the Crocodile.

The author next adverts to the many points of structural correspondence observable between *Plesiosaurus* and *Teleosaurus*, not only as regards the atlas and axis, but as respects the cranium.

The existence of a distinct jugal and squamosal, and of a union between the latter and the post-frontal, and the consequent subdivision of the temporal fossa, as in the Crocodile, are indicated. The extension of the exoccipitals and of the pterygoids to the os quadratum is adverted to; and the very backward position of the posterior nares ascribed to *Plesiosaurus* is questioned. Teleologically, such an arrangement appears not very comprehensible: and, on morphological grounds, it is unlikely; for the posterior nares are more forward on the base of the skull in *Gavialis* than in *Crocodilus*, and far more forward in *Teleosaurus* than in *Gavialis*. It seems more probable that the so-called posterior nares of *Plesiosaurus* correspond with the deep fossæ on either side of a prominent median ridge visible on the under surface of the basisphenoid of *Teleosaurus*.

The petrosal bone, completely covered externally by the quadratum in *Crocodilus*, is partially exposed in *Gavialis*, and completely so in *Teleosaurus* and in *Plesiosaurus*.

Similar comparisons were pursued with respect to other parts; and it is shown that in many respects the *Teleosauria* bridge over the gap between the long-necked *Enaliosauria* and the existing *Crocodilia*,—a conclusion not without interest, when the relations in time of the two orders are considered.

3. "On the Coal found south of Concepcion, in Southern Chili." By Dr. C. Forbes, R.N. (In a letter to the President.)

The coal is found in seams alternating with shales and overlaid by calcareous sandstone; fire-clay underlies the whole. The shales contain fine impressions of dicotyledonous leaves; and some of the sandstones above the coal abound with casts of a Mactra-like bivalve; and others with Turritellæ. From this association of fossils, Dr. C. Forbes believes that the coal is decidedly not of palæozoic age, and may be tertiary.

4. "On a quantity of Crabs thrown up on the beach in Payta Bay." By Dr. C. Forbes, R.N. (In a letter to Prof. Ansted, F.G.S.)

For some time previous to the occurrence of a severe earthquake-shock, on or about the 30th August 1857, the Bay of Payta swarmed with crabs, of a kind not generally observed, and ten days after the earthquake they were thrown up on the beach, in a raised wall-like line, 3 to 4 feet wide, and to the height of about 3 feet, along the whole extent of the bay, and above highwater-mark.

At the same time as the upheaval of the crabs took place, the water of the bay became changed, from a clear blue, to a dirty blackish-green colour, much resembling that off the Island of Chiloe, Concepcion, and the southern parts of Chili. Ten days afterwards, Dr. C. Forbes found that living specimens of the crabs were still numerous in the bay, but all appeared to be sickly, and numbers came ashore to die.

There were no appearances of any alteration of the relative position of sea and land in the vicinity, nor had any ebullition of gases been observed; although probably to both these causes combined the phænomenon described was due.

February 3, 1858.—Major-Gen. Portlock, President, in the Chair.

The following communication was read:—

"On the Succession of Rocks in the Northern Highlands, from the oldest Gneiss, through Strata of Cambrian and Lower Silurian age, to the Old Red Sandstone inclusive." By Sir R. I. Murchison, F.R.S., V.P.G.S.

This memoir comprised a general sketch of the succession of the stratified rock-masses occupying the northernmost counties of Scotland (Sutherland, Caithness, and Ross), as determined by former observations of Prof. Sedgwick and the author, and of Macculloch, Jameson, Cunningham, Miller, and Nicol, and by the recent discoveries of Mr. Peach. In the commencement, Sir Roderick, having referred to the long-held opinion that the great mountainous masses of red conglomerate and sandstone of the west coast were detached portions of the Old Red Sandstone, alluded to Mr. C. Peach's discovery (in 1854) of organic remains in the limestone of Durness, which led the author to revisit the Highlands (accompanied by Prof. Nicol), when having found still more fossils, he expressed his conviction (at the British Association, Glasgow Meeting, 1854) that the quartzites of Sutherland and their subordinate limestones were of Lower Silurian age; and was strengthened in the opinion (which he had already published) that large portions of the crystalline rocks of the Highlands would prove to be the equivalents of Lower Silurian deposits in the South of Scotland. In 1856 Colonel James and Prof. Nicol separately observed the unconformable overlap of the great conglomerates by the quartzite series; and the latter geologist greatly extended all previous observations, and communicated to the Society a memoir, showing that the old gneiss and its superposed conglomerate, as seen along a very extensive region of the Western Coast, formed the buttresses upon which all the crystalline quartz-rock and limestone of the western parts of

Ross-shire and Sutherlandshire reposed. At the same time Prof. Nicol hypothetically suggested, that, until the evidence of fossils was more complete, the quartzite and limestone might be considered as the equivalent of the Carboniferous series of the South of Scotland. Another hypothesis, which had been propounded by the late Mr. Hugh Miller, regarded the quartz-rocks and hard limestones of Sutherland merely as the metamorphosed representatives of the Old Red and Caithness series of the Eastern Coast.

Both of these hypotheses, however, seemed to the author to be quite incompatible with the physical order of the rock-masses in question; for, according to the observations made long ago by Prof. Sedgwick and himself, the above-mentioned crystalline rocks, in the lower part of which the Durness fossils have recently been found, are the inferior members of the great undulating mass of micaceous and schistose rocks, which, rolling over to Caithness on the east, there constitute the basis out of which the bottom strata of the Old Red Sandstone are chiefly formed.

Of late, Mr. Peach has, by his untiring perseverance, obtained a still larger collection of fossils from Durness, and in better preservation than those found in 1854, and Mr. Salter finds that this collection of well-defined forms comprises genera belonging only to the Lower Silurian of North America. Hence all doubt is now dispelled; and the author, following up the suggestions which he offered at the Glasgow Meeting of the British Association, describes in the present paper these rocks and their fossils; defining the great unfossiliferous conglomerate-masses of Sutherland as of Cambrian age; the quartzites and limestones as Lower Silurian; and the overlying micaceous and gneissose schists and flagstones as also of Silurian age.

In the body of the memoir, Sir Roderick, after a brief notice of the "fundamental gneiss," described the "Cambrian red sandstone and conglomerate," alluding to the faithful descriptions by Hugh Miller and Nicol. He also detailed certain subsequent observations of Colonel James and Mr. Peach on the unconformity of these rocks to the overlying quartzites, and on the great dislocations exhibited in these masses; and he also noticed the discovery of a porphyry between the gneiss and the conglomerate by the latter observer.

The "Lower Silurian rocks, in the form of quartz-rock, crystalline limestone, chloritic and micaceous schists, and younger gneiss," were then described. The fossils from the quartz-rock consist of small annelide tubes now named *Serpulites Maccullochii*, and traces of fucoids. These fossils were long ago noticed, but of late they have been traced in beds for great distances by Mr. Peach. The strong band of limestone between two quartz-rocks is estimated by Colonel James to lie about 800 feet above the base of the series, and is of great extent. The fossils detected in it have been determined by Mr. Salter to be *Maclurea Peachii*, spec. nov. (and its curious twisted operculum), *Ophileta compacta*, well known in Canada, *Oncoceras*, spec., and *Orthoceras*, a smooth species with a compressed siphuncle. They all closely resemble fossils of the Lower Silurian rocks of North America, which range from the Calciferous rock up to the Trenton limestone, both inclusive; a group especially to be found in the limestones of the Ottawa River in Canada.

Passing across Ross-shire in a more southern parallel, from Loch Duich in Kintail, on the west, to the frontier of the Old Red Sandstone on the east, the general succession of rocks was described to be much the same as that in North-west Sutherlandshire, though there are considerable changes of lithological character when the same rocks are followed southwards or south-south-west upon their strike; and the author stated his belief, that not only may the regularly bedded limestones which are intercalated in the chloritic and quartzose rocks of Dumbartonshire be classed with some of the oldest of those stratified masses which, like the limestones of Sutherland, are unquestionably of Lower Silurian age, but that the vast and evidently overlying masses of mica-schist and quartzose-gneissic flag-rocks of the Breadalbane district may be some day found to be simply the prolongations of the micaceous flagstones of the North-western Highlands above alluded to, as overlying the quartz-rock and fossiliferous limestone: further, that in the still higher limestones and schists seen on the banks of Loch Tay, we may speculate on the existence of the equivalents of younger and higher strata than any which are observed in the Northern Counties.

After some observations on the truly stratified condition of these micaceous and gneissose schists (younger gneiss) of the Highlands, Sir Roderick proceeded to the consideration of the "Old Red Sandstone of the North-east of Scotland;" defining the tripartite division of this great series, and demonstrating that the beds with *Cephalaspis Lyellii* and *Pterygotus Anglicus* of Forfarshire really lie at the base of the series, and are certainly of greater antiquity than the bituminous fossil-bearing schists of Caithness. This division is in accordance with the relations of the deposits of the Devonian period, as seen in Devonshire and Germany; though the lowest member of the Old Red of Scotland has no representative in the Devonian rocks of Russia. The Caithness flagstones were described as being in the middle of the series; whilst the underlying conglomerates and sandstones were shown to be the true equivalents of the Cephalaspis-beds of Forfarshire, and of the lower cornstone-strata of Herefordshire, which there graduate downwards, through the tilestones, into the uppermost Silurian rocks of Ludlow.

The Old Red rocks of the North Highlands were described in more or less detail by the author, who showed that the group, as seen in Caithness and the Orkney Islands, is composed of—1st, lower red conglomerate and sandstones; 2nd, grey and dark-coloured flagstones and schists, both bituminous and calcareous (this portion being in Elginshire and Murrayshire represented by Cornstones); and 3rd, upper red sandstones. The North Scottish Old Red contains one great inferior portion which has no representative in the Devonian rocks of some foreign countries, though it is completely represented in all its parts in other tracts both of Britain and the Continent.

Having next described the conditions under which many of the species of fish (at least twenty-one) found fossil in Caithness and Cromarty occur in Russia commingled with the middle Devonian mollusks of Devon, the Boulonnais, and the Rhine; and having pointed out that the lowest member of the Devonian series, with its *Cephalaspides*, is wanting in Russia, Sir Roderick insisted on the importance of the Devonian series in the scale of formations, and on the fact that the Old Red conglomerates, ichthyolitic schists, and cornstones, with the overlying sandstones, of Scotland and Herefordshire fully represent in time the Devonian rocks of the South of England and the Continent, so full of corals, crinoids, and marine mollusks.

Some brief observations on the Newer Red Sandstone of the West Coast of Ross-shire, and the Lias and Oolitic deposits of the North of Scotland and the Western Isles, concluded this paper.

February 24, 1858.—Prof. Phillips, President, in the Chair.

The following communications were read:—

1. "On the Gradual Elevation of a part of the Coast of Sicily, from the Mouth of the Simeto to the Onobola." By Signor Gaetano Georgio Gemmellaro. Communicated by Sir C. Lyell, F.G.S.

In this paper the author described in detail the physical evidences, observed by him along a great part of the eastern coast of Sicily, which prove—1st, that from the shores of the Simeto to the Onobola undeniable characters of the former levels of the sea in the recent period are traceable from place to place. 2ndly, that great blocks of lava, with blunted angles, and rolled and corroded on the surface, a calcareo-siliceous shelly deposit, and a marine breccia, which are seen at different heights above the present sea-level, are the effects of the continued and daily action of the waves of the sea at successive levels. 3rdly, that the existence and disposition of the holes of the *Modiola lithophaga*, Lamarck, in the calcareo-siliceous shelly deposit, and the local presence of shells, both Gasteropods and Lamellibranchiates, in their normal positions, support the view of a slow and gradual elevation of the coast. 4thly and lastly, that the lithodomous molluscs and the calc-siliceous deposit being found on the Cyclopean Islands (Faraglioni) up to the height of almost 13 metres, and large rolled blocks of lava, invested with *Serpulæ* being also found there to the height of 14 metres, a mean height of 13 metres and 5 decim. is established as the greatest extent of the now undeniable gradual elevation of this portion of the coast of Sicily during the present period.

2. "On the occurrence of transported Pebbles and Boulders at high levels in Aberdeenshire." By T. F. Jamieson, Esq. In letters to Sir R. I. Murchison, V.P.G.S.

The author stated that he had found indications of the district having been submerged beneath the sea to the height of about 430 or 450 feet for a considerable time during the later Tertiary period. Extensive ridges or hillocks of water-worn gravelly debris, bearing large boulders on their surfaces, occur in Aberdeenshire. Beneath the gravel of these mounds, the author has found beds of sand and clay containing marine shells, generally broken, such as *Nucula tenuis, Leda pygmæa, Lucina ferruginosa, Cyprina Islandica, Mactra, Pecten* and *Mangelia.*

At higher levels, to the elevation of 800 feet at least, Mr. Jamieson has found striated pebbles and boulders, but unaccompanied by deposits similar to the above. The last change of level in Aberdeenshire seems to have been one of depression, indicated by beds of peat passing below the sea at various places between Aberdeen and the Moray Firth.

3. Mr. Kennedy Macnab, of Inverness, communicated, in a letter to the Secretaries, the fact of flint arrow-heads and whelk-shells having been found at the depth of about 3 ft. 6 in. beneath the surface of a moss, covered with wood, in the parish of Abernethy (Inverness and Elgin).

4. Mr. Richard Mason, of Tenby, in a communication to the Secretaries, offered a *résumé* of the evidences, both traditionary, historical, and physical, of—1st, the probable depression at some pre-historic period of an extensive tract of country, covering the site of the Bristol Channel and Cardigan Bay; and 2ndly, of the more recent elevation of the land in the neighbourhood of Tenby, South Wales; the elevated district being apparently confined to that lying on the carboniferous limestone. Evidence of a comparatively recent depression of the Cardiff area was also alluded to.

XLI. *Intelligence and Miscellaneous Articles.*

ON FLUORESCENCE PRODUCED BY THE AURORA.

To John Tyndall, Esq.

Observatory, Armagh,
March 24, 1858.

My dear Sir,

I DO not know whether the fact mentioned in the enclosed has been noticed before. If not, perhaps you may think it worth inserting in the Philosophical Magazine.

Yours ever,

T. R. Robinson.

On the 14th instant an aurora was visible here of more than the average brightness. At 11 p.m. it showed an arch extending from W. to N.E. by E., which emitted a few yellow streamers; and the sky above it was covered with diffused light, over which brighter portions flickered like waves, extending several degrees beyond the zenith. I availed myself of the opportunity to try whether this light was rich in those highly refrangible rays which produce fluorescence, and which are so abundant in the light of electric discharges; and I found it to be so. A drop of disulphate of quinine on a porcelain tablet seemed like a luminous patch on a faint ground; and crystals of platinocyanide of potassium were so bright, that the label on the tube which contained them (and which by lamplight could not be distinguished from the salt at a little distance) seemed almost black by contrast.

These effects were so strong in relation to the actual intensity of the light, that they appear to afford an additional evidence of the electric origin of this phænomenon, and as such I hope they may interest some of your readers.

NOTE ON THE TANGENT-GALVANOMETER.

BY G. JOHNSTONE STONEY.

To the Editors of the Philosophical Magazine and Journal.

Queen's University, Dublin Castle,
March 15, 1858.

Gentlemen,

My friend Dr. Robinson of Armagh has been so good as to point out to me the fact, that a modification of Gaugain's rheometer has been suggested by Jacobi which combines the advantage of M. Gaugain's arrangement with a freedom from liability to those errors of which I sought to determine the laws in a paper on the adjustments of the tangent-galvanometer published in last February's Number of the Philosophical Magazine. As Jacobi does not seem to have contemplated this advantage in the form which he gave to the instrument, it may be useful to direct the attention of physicists to its existence.

Gaugain's rheometer, as modified by Jacobi, consists of two equal current circles having a common axis, along which they are separated by an interval equal to the radius of either. The needle is swung midway between them, and the circles are to be traversed in the same direction by currents of equal intensity.

I am, Gentlemen,
Yours faithfully,
G. Johnstone Stoney.

ON THE DAILY FALL OF THE BAROMETER AT TORONTO.

BY THOMAS HOPKINS, ESQ.

In this paper the author endeavours to show that the vapour, which in the early part of the day was produced by solar heat at the surface, by its expansive power, bore that heat to the upper regions of the air, where it was condensed by the cold of the gases in that situation, when the heat of elasticity was set at liberty to warm and expand the gases, and that it was this expansion which reduced atmospheric pressure in the locality and caused a fall of the barometer.—*Proc. Roy. Soc.* Feb. 4, 1858.

ON THE ACTION OF THE ELECTRIC CURRENT ON AQUEOUS SOLUTIONS OF CHLORINE, BROMINE, AND IODINE. BY M. A. RICHE.

On passing an electric current through a solution of chlorine prepared in the dark with boiled distilled water, two kinds of phænomena are observed. The first effect of the current, like that of light and heat, is to decompose the water, whereby the oxygen escapes, and the nascent hydrogen recombines with the excess of chlorine, forming hydrochloric acid. Soon, however, the opposite effect is produced, that is, the nascent hydrogen escapes in the free state, while the nascent oxygen combines with the chlorine present.

Thus with ten of Bunsen's elements, the volume of free oxygen after the first eleven minutes was about double that of the hydrogen. In about an hour the whole quantity of the gases liberated were nearly equal, showing that the hydrogen was gaining on the oxygen. In an hour and fifty minutes after the commencement, equal volumes of hydrogen and oxygen were liberated in fifty-three and eighty-nine minutes respectively. After four hours twenty-six minutes, the times for the liberation of equal volumes were thirty-four minutes for hydrogen and 182 for oxygen.

At this point the absorption of the oxygen was at its maximum; it commenced diminishing until the liberated oxygen was exactly half that of the hydrogen.

The oxygen absorbed forms perchloric acid with the chlorine. M. Riche explains this phænomenon by supposing the water to be first decomposed alone, and thereby free oxygen and hydrochloric acid to be produced; secondly, the so-formed hydrochloric acid to be also decomposed simultaneously with the water, so that at the negative pole hydrogen is given off, while at the positive oxygen and chlorine being liberated together, combine to form perchloric acid. He supports this by showing that in chlorine water prepared twenty-four hours previously, in very old chlorine water and in distilled water acidulated with hydrochloric acid, the oxygen is absorbed from the commencement.

M. Riche recommends the above method for preparing perchloric acid.

Bromine, iodine, hydrobromic and hydriodic acids furnished very analogous results, with the exception that with these the oxidation ceases as soon as bromic and iodic acids are formed, the higher degree of oxidation not being reached.

In the same manner, bromide of potassium gives rise to bromate of potash.

If dry bromine and hydrogen be subjected together to the continued action of the electric spark, the two combine, and hydrobromic acid is formed.

Dry chlorine and oxygen do not combine when subjected together to the action of the electric spark; but if moisture be present, the chlorine is oxidized to perchloric acid.—*Comptes Rendus de l'Acad. de Scien.*, February 15, 1858.

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XLII. *On the Integral of Gravitation, and its consequents with reference to the Measure and Transfer, or communication of Force.* *By* J. J. WATERSTON, *Esq*[*].

MODERN ideas with relation to heat and the active condition of the molecular element naturally incline us to estimate every force with regard to its work-producing capacity. In the following paper I have considered gravitation under this aspect, and in doing so, have been led to discuss some points relating to dynamical sequence in the abstract.

The principle of the conservation of force may now be termed the guiding star of physical inquiry, inasmuch as it appears to be generally admitted as the effective antecedent of the transmutations and correlations of natural forces. Although received and upheld as an abstract principle in philosophy before the time of Newton, it was distinctly rejected both by him and by Laplace as inconsistent with natural phænomena. Even at the present day, mathematicians have been so long accustomed to and brought up in the statical method of treating molecular physics, initiated by those great leaders, that the inefficiency and inconsistency of this mode of inquiry with the mechanical theory of heat seems as yet not to be fully appreciated by some even of the most zealous upholders of that theory. With this impression, it seems desirable that every effort should be made to arrive at a clear understanding of fundamental pionts, and of the principle of physical causation which the mechanical theory supplies.

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[*] Communicated by the Author.

§ 1.

The Integral of Gravitation is a function of Space.

Suppose a central homogeneous globe to augment in bulk by the descent of similar matter from an infinite distance in radial directions all round; each descending element, on arriving at the surface of the globe, presents itself charged with a certain amount of mechanical force equivalent to the square velocity with which it impinges. If we confine our attention to *the centripetal influence of the original central globe only,* the square velocity of the descending element diminishes in the inverse ratio of the radius of the augmenting globular mass; for it is upon the surface of this that impact takes place, and the matter that has been added to the original globe is assumed not to augment the centripetal force acting upon the descending matter.

Taking for standard unit the square velocity generated by falling through the radius of the globe with the force of gravity uniform as at surface, and computing the integral mechanical effect between the original surface of the globe and any other spherical surface external to and concentric with that surface, it is found to be equivalent to the product of the standard square velocity by a mass of matter that would cover a surface equal to a great circle of the outer sphere, *minus* a great circle of the central globe, to a depth equal to the radius of the globe. This ratio is equal to the ratio of the square of the cube root of the space between the concentric spherical surfaces, and continually approaches the ratio of the surface of the outer sphere.

If the matter composing a planetary globe is assumed to have originally descended from space, and to have become centrally collocated in successive layers, it may be viewed with reference to its gravitation integral (1) as having accomplished work, (2) as having the faculty of accomplishing work, of generating force. The following are a few theorems developing the quantitative relations.

1. *The work accomplished, or the mean square velocity of the molecules of a planetary globe acquired by the centripetal precipitation of the matter of that globe from an infinite distance, is equal to $\frac{3}{5}$ the square velocity acquired by a body falling through radius of the globe with the uniform force of gravity at its surface.*

The following may serve to illustrate this.

If as much matter (iron) descended to the earth from planetary space as would cover the whole surface to a uniform depth of $\frac{1}{200}$th of an inch, it would, on entering the atmosphere with the mean velocity of 20 miles per second, generate as much heat as the whole atmosphere contains. The steps of the computation of this are as follows:—the motion of water moving with velocity of 20 miles per second, converted into heat, would raise its temperature about one million degrees; iron about six millions. In about 10 feet depth of water there is as much heat as in the superjacent atmosphere. The quantity of heat in a cubic foot of iron is nearly the same as in a cubic foot of water. Iron $\frac{1}{100}$th inch in thickness, heated six million degrees, is equivalent to 10 feet heated 500°, and 500° absolute is 40° on Fahr. scale. The mean absolute temperature of the atmosphere is 250° when temperature at base is 40° F.

2. *Comparing the work accomplished, or molecular square velocity in globes of different radii and composed of the same kind of homogeneous matter, it is found to follow the ratio of the square of radius.*

As an example, the earth may be compared with the sun in this respect, supposing them of equal density and homogeneous. If the temperature in descending the earth's radius increases uniformly 1° in 50 feet, the mean temperature of its whole mass would be about 100,000°. This is also nearly the mean temperature that would result from the *work accomplished*, if the matter composing the earth had the specific heat of iron, the mean square velocity being nearly the same as the square velocity of a projectile moving in a circular orbit round the earth grazing its surface. The sun's radius being about 100 times that of the earth, its mean temperature would be $100{,}000 \times 10{,}000$, or one thousand million degrees. The force of gravitation may thus be viewed as the effective antecedent of the sun's heat.

3. *The capacity of a globe to generate force, increases in a ratio that continually approaches the square of the radius of the sphere within which its force of gravity is assumed to be limited, and the capacity of different globes follows a ratio compounded of the mass of the globe and the square of the cube root of the space throughout which the action is supposed to be limited.*

4. *The limiting sphere of the action of gravity being supposed constant, and the size of the globe to vary, the capacity increases as the surface of the globe.*

5. *The capacity of different globes is the same if their force of gravity is assumed to be limited within spheres that are in the inverse ratio of the globes respectively.*

§ 2.

The force-generating faculty exists in space and is directed centripetally.

This is proved by the following considerations.

The integral force-producing power of any body, however small, subject to the law of universal gravitation, is illimitable as space.

It is impossible to imagine an infinite attribute belonging to a finite entity. It is therefore in space that the energy that contributes the power of gravitation exists, and the element of matter merely gives to it a centripetal direction.

This, as a consequent of the law of gravitation, seems noteworthy from it probably being applicable to molecular forces generally. It favours the idea that the function of the material element is to give *direction* to a living force that pervades space.

This is further discussed in § 14.

§ 3.

The law of Gravitation with respect to the element of radial space.

This law is usually defined with reference to a constant *element of time*; the increment of velocity generated being proportional to the increment of time—whatever the direction or velocity of the motion—and inversely as the square of the central distance. If we view it with reference to a constant *element of radial space,* we find that the increment of square velocity generated by the force of gravitation is proportional to decrement of radial distance, and inversely as square of central distance. This holds whatever the velocity or direction of the motion, whatever the orbit of the projectile.

Thus each element of radial distance has associated with it a fixed element of mechanical force, to be given to or taken from all bodies traversing it; whatever may be the direction of their motion or the time taken to pass through it. A weight attached to clockwork may take a day to descend through one foot, and the same weight falling freely from a height may take only $\frac{1}{1000}$th of a second to pass through the same foot, yet the mechanical force communicated in each case is the same.

§ 4.

The mutual gravitation of two bodies developes mechanical force in each of them inversely proportional to its mass.

Suppose two bodies to descend towards each other by their mutual gravitation, their common centre of gravity being at rest. At any time before they meet, their acquired velocities being examined will be found inversely as their masses, which assume as 1 to 10. Suppose them removed to the earth's surface and each projected up a vertical with their acquired velocities respectively: the smaller body rises 100 times the height ascended by the larger, and thus in again descending would be able to perform ten times the work.

Although in ordinary parlance the action and reaction of these two attracting bodies are equal according to Newton's third law, their common centre of gravity being at rest, so that if we "suppose an obstacle interposed to hinder their congress"*, that obstacle must be as much pressed on one side as the other; nevertheless, when after being set free to obtain motion from the impulse of the mutual gravitation, and after a certain time their condition is examined, we find that the lesser has gained the power of performing the work 10, while the greater has only obtained the power of performing the work 1.

§ 5.

The development of mechanical force, during the same interval of time, in bodies set in motion by the same pressure is inversly proportional to their mass, if the pressure acts with the same intensity when a body is in motion as when it is at rest.

Suppose a long horizontal cylinder with its transverse section equal to one square inch. Let one end be stopped with a piston weighing 15 lbs., and the other end with another piston weighing 150 lbs. Let the cylinder be now exhausted of air, and suppose the pistons to be moveable air-tight without friction. If they are supposed to press on each end of a rod passing through the axis of the cylinder, they would be *in equilibrio*, they would press equally in opposite directions. Take away the rod, and allow them to move freely by the atmospheric pressure. At the end of one second the 150 lb. piston acquires a velocity of 3·2 feet per second, and the 15 lb. pound piston a velocity of 32 feet per second. With these velocities, suppose them to be projected upwards in a vertical, the lighter would ascend 16 feet and the heavier $\frac{16}{100}$ths of a foot. In descending, the lighter would thus be enabled to exert ten times the mechanical power of the heavier.

Strictly, the pressure on the smaller piston in this example is not quite equal to that on the larger after they have begun to move, in consequence (if the expression may be allowed) of the *vis à tergo* not being infinitely prompt.

§ 6.

Equal pressure does not imply equal capacity of generating motion.

This position, inferred from the examples given in §§ 4, 5, shows the necessity of guarding against being led away by the words action (which implies states of acting or moving) and reaction, to conceive that an equal impressed tendency to move implies an equal capacity of generating motion.

A motive cause may act *equally* on two bodies at *rest*, but *unequally* on bodies moving with *unequal velocities*, and the effect may be in the proportion of these velocities respectively.

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* Principia.

The following dynamic arrangement will serve perhaps to convey a distinct idea of this:—

Let there be two perfectly elastic plates presenting the same surface, but unequal in mass, say in the ratio of 1 to 10. On one side of each let a current of perfectly elastic particles be supposed to impinge continually,—the minuteness as well as velocity of these particles being excessive. Whether these plates are at rest or moving in a direction perpendicular to the plane of their surface, the number of particles that impinge in an element of time must be constant so long as the velocity of the particles is incomparably greater than the velocity of the plates. If the plates are kept at rest by an interposed obstacle, the impinging particles are reflected without change of velocity, and no motion is transferred from them to the plate, or from the plate to them. If the plates move in the same direction as the particles with velocity 1, the particles at impact lose one decrement of square velocity, and if with velocity 10, then the particles lose ten decrements of square velocity. These decrements being transferred to the plates, become collectively increments of motion having the character of an accelerating force.

While the plates are prevented from moving by resting on an interposed obstacle, the current of impinging particles forms a *pressure or tendency to put the plates in motion,* which is equal in both plates, but *the faculty of taking motion from the current depends on the plates being in a state of motion and increases with their velocity.* The heavier plate, by its greater mass being confined in its velocity to one-tenth, has its ability to take force from the current reduced in this proportion.

§ 7.

Gravitation as an impulse.

If the fact of the integral of gravitation as a force generator, being a function of space, is admitted as an argument for the existence of the force in space before it is communicated to the falling body, it realizes the idea or impression which Newton has expressed in the introduction to Section 11 of the first Book of the 'Principia,' viz. "I shall therefore at present go on to treat of the motion of bodies mutually attracting each other, considering the centripetal forces as attractions, though perhaps *in a physical strictness they may more truly be called impulses.* But these propositions are to be considered as *purely mathematical,* and therefore *laying aside all physical considerations,* I make use of a familiar way of speaking to make myself more easily understood."

Considered as an impulse, gravitation has several characteristics,—statical, dynamical, and directive.

1. It is an impulse the *statical* magnitude of which upon an

element of matter is determined by the inverse square distance of that element from another: precisely as if an emanative influence had issued from each as a centre and become diluted as it became diffused through an augmenting spherical surface.

2. An impulse that accumulates square velocity on an element in motion in a ratio combined of the rate at which its distance from the central element diminishes, and of the amount of statical impulse due to that distance, and to the number of elements to which the impulse is directed. This defines its *dynamical* or force-producing magnitude.

3. An impulse operating exactly in the direction of and towards the distant material element.

4. An impulse cumulative to an apparently illimitable extent, according to the number of the distant collocated elements of matter: *e. g.*, at the earth's surface an element of matter in passing—in any direction and with any velocity—through a foot vertical, has given to it 64 feet of square velocity. At the surface of the sun, a foot vertical contributes 1800 feet square velocity.

5. An impulse, that, so far as observation extends, although dependent on matter at a distance, is uninfluenced by intervening matter.

6. An impulse that is not diminished by the velocity with which a body moves in the direction in which it operates.

Two of these characteristics, the fifth and sixth, are physical paradoxes of the same degree as the marvel in the wave theory of light, that the medium or media that convey the impressions of heat and light, although acting and acted upon by the elements of bodies, presents no resistance to the planetary motions.

The sixth has been put to the test by Laplace while seeking to account for the secular inequality of the moon. Mr. Adams having lately found the treatment of this subject defective, it might be worth while to examine analytically the influence that an infinitesimal divergence from the law of gravitation in the case of bodies approaching or receding from each other would have on the planetary motions, so as to be prepared to recognize its accumulated effects through geological periods of time. The tendency of first differentials of an elliptic orbit seems to be motion of apses in the direction of the motion of the planet; diminution of the major axis, and the minor axis unchanged; ellipses thus tending towards circles with fixed diameters. The chief question is as to the moon's motion being so delicate a test of the divergence as Laplace has deduced from his analysis.

§ 8.

The quantity of motion in a body is proportional to the square of its velocity.

Motion as a physical entity is a combination of two quantitative elements, *matter, velocity,* separately measurable. When united the standard of measurement is not obvious. Is the velocity or square of the velocity to be taken *as that which takes equal part with matter* in the phænomenon which results from their union? This question, debated since the time of Newton*, seems now to have settled down into a tacit admission of both. This was the opinion of Dr. Thomas Young, who considered the fact that two bodies while freely acting on each other obtained from their mutual action velocities inversely proportional to their respective masses, as conclusive of the simple ratio of the velocity being the natural standard. If this proposition had been stated in other terms as above, § 4, viz. that bodies while freely acting on each other obtained from their mutual action powers of producing work inversely proportional to their masses, no such necessity of adopting the simple ratio of the velocity would have been felt; only a noteworthy characteristic of centripetal action. The position of the centre of gyration in a revolving body is clearly on the side of the square velocity being the natural standard. The motion of impinging elastic bodies also requires the square velocity.

The following considerations as to impacts, it may be useful to keep in view as accessory to the principle of the conservation of force.

§ 9.

Force of impact depends on the relative motion of the impinging bodies.

Whatever be the molecular texture of bodies, we may bring all impacts to the condition of perfect elasticity by interposing a spring. There are three stages in such phænomena:—1. The motions before impact before the spring is acted upon. 2. The motion at impact, or at the instant when the maximum compression of the spring has been effected by the force of impact. 3. The motions after recoil, after the spring has given out again the force it had received. During all three the common centre of gravity is unaffected in its state of motion or rest, and *the force of impact, as measured by the spring's tension, is dependent, not on the absolute, but on the relative* motion of the impinging bodies.

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* This is discussed in the last part of the article on virtual velocities, 'Penny Cyclopædia.' It concludes as follows:—"But if pressure be merely considered as the cause of motion, and called force, in that sense it is very difficult to see why the cause, which is only known by the effect, is to be measured by anything but the simple effect. Probably this discussion gave rise to the chapter of the *Mécanique Céleste,* in which Laplace speculates upon what the laws of motion would have been if force had been as a function of the velocity instead of as the simple velocity. We have never met with any one who could give us an intelligible account of the meaning of this investigation."

§ 10.

The condition of impinging bodies demonstrates the absurdity of assuming the motion of a body to be proportional to its velocity.

Let us take a simple numerical example. A ball of 1 lb. weight with velocity 9 strikes a spring interposed upon another ball of 2 lbs. weight which is at rest. The velocity of the centre of gravity is 3; the motion of the 1 lb. ball *relative to centre of gravity* is 6, and of the 2 lb. ball 3. These relative motions are exactly reversed by the recoil of the spring after impact. Thus the *absolute* motion of the 1 lb. ball after impact becomes 3, and the absolute motion of the 2 lb. ball 6. At the instant when the spring has obtained its maximum compression by the force of the collision, the velocity of both balls is 3, the same as the centre of gravity.

Let us now compute the quantity of motion in the system, assuming the motion of a body to be proportional to its velocity.

Before impact the amount is . . . . $1^{\text{lb.}} \times 9 + 2^{\text{lbs.}} \times 0 = 9$.

After the spring has received the force of compression, the amount is . . . . $(1^{\text{lb.}} + 2^{\text{lbs.}}) \times 3 = 9$.

After recoil, when the spring has given out its force, the amount is . . . . $1^{\text{lb.}} \times 3 + 2^{\text{lbs.}} \times 6 = 15$.

Thus the spring has been bent, compressed, or as it were, wound up without taking any motion from the system, without any cost of work; and the system has actually gained motion by the collision. The concurrence of the two bodies has nearly doubled their collective momentum.

If the motion of a body is assumed to be proportional to the square of its velocity, the quantity of motion in the system before impact is . . . . $1^{\text{lb.}} \times 81 + 2^{\text{lbs.}} \times 0 = 81$.

After the spring has received the force of compression, the amount is . . $(1^{\text{lb.}} + 2^{\text{lbs.}}) \times 9 = 27$.

After recoil, when the spring has given out its force, the amount is . . . $1^{\text{lb.}} \times 9 + 2^{\text{lbs.}} \times 36 = 81$.

The collective motion before and after impact is thus the same, and $81 - 27 = 54$ is the force that is taken to compress, bend, or wind up the interposed spring: it is the work rendered latent at the instant of impact but immediately restored. It is a dynamic, not a static force, and with the same impinging bodies it is proportional to the *square* of their *relative* velocity. Thus if the 2 lb. ball with velocity 1 meets the 1 lb. ball with velocity 8, the relative velocity is still 9, and the force of impact still 54. This is also apparent from the general expression for the force of impact, viz. $\overline{v_1 - v_2}^2 \left(\frac{w_1 w_2}{w_1 + w_2}\right)$.

§ 11.

The condition of a system having both a rotatory and rectilinael motion demonstrates the absurdity of assuming the motion of a body to be proportional to its velocity.

In the 31st Query of Newton's 'Optics' (vol. iv. p. 258, Horsley's edition), the following occurs:—"From the various composition of two motions, it is very certain that there is not always the same quantity of motion in the world. For if two globes joined by a slender rod revolve about their common centre of gravity with a uniform motion, while that centre moves on uniformly in a right line drawn in the plane of their circular motion, the sum of the motions of the two globes, as often as the globes are in the right line described by their common centre of gravity, will be bigger than the sum of their motions when they are in a line perpendicular to that line. By this instance, it appears that motion may be got or lost. But by reason of the tenacity of fluids and attrition of their parts, and the weakness of elasticity in solids, motion is much more apt to be lost than got, and is always upon the decay."

This extract is remarkable as showing the obstinate influence of a preconceived notion upon a powerful intellect. The alternate appearance and disappearance of motion in connected globes did not seem to Newton inconsistent with any of the laws of motion previously laid down in his 'Principia.' Let us view a numerical example. The globes suppose to be equal and to move with a circular velocity of 3, and the common centre of gravity with a rectilineal velocity of 4. As often as the globes are in a line perpendicular to the line described by the centre of gravity, the absolute velocity of one is 7, and of the other 1, making the sum of their motions 8. As often as the globes are in the right line described by the common centre of gravity, the absolute velocity of each is 5, making the sum of their motions 10. From the first to the second position, the motion in one has diminished from 7 to 5, losing 2, and the motion of the other has increased from 1 to 5, gaining 4. Hence not only has the 2 been transferred from the one globe to the other, but another 2 has been supplied. There is an intermittent augmentation and diminution of motion; and as by the second law of motion "the alteration of motion is ever proportional to the motive force impressed, and is made in the direction of the right line in which that force is impressed," it comes to pass, that, by giving motion to the centre of gravity of two revolving balls in the plane of revolution, we introduce the action of a tangential force; but if the same motion is given in a direction perpendicular to the plane of revolution, no force is introduced, the sum of the motions being always 10.

If we inquire what is the function of the absolute velocity which, being multiplied by the mass of each globe respectively, shall give a sum that is constant, whatever may be the direction of the motion of the common centre of gravity, we shall find that the square velocity is the only function that answers, and that although the absolute velocities are continually changing through a cycle, the sum of the products of the mass of each body by the square of its respective absolute velocity, is a constant quantity, and equal to the sum of the rectilineal and rotatory *vis viva* taken separate.

§ 12.

Distinction between momentum and motion.

In our educational treatises, up to the present time the idea continues to be inoculated into the student of physical science, that the natural standard measure of motion in the abstract is the product of the mass of a body or system by the velocity of its centre of gravity. The ambiguity and contradictions that this involves when he extends his studies, might be avoided if such definition was confined to the word momentum, and the word motion taken to mean the product of the mass by its square velocity. The following instance will serve to make this appreciable.

In the use of the Ballistic pendulum, the same velocity is communicated to it by the stroke of a 2 lb. ball moving 100 feet per second as by a 1 lb. ball moving 200 feet per second; the *momentum* or velocity of the centre of gravity of the system composed of the bullet and pendulum being in the two cases the same, or very nearly so. Suppose the 1 lb. ball to be hollow, so as to have the same diameter as the 2 lb. ball, its penetration into the timber of the pendulum would be found to be double that of the 2 lb. ball, because its *quantity of motion* is double. Its working power or efficacy is double. The definition of *momentum* would thus conform to the condition of inelastic impact, where the force of impact disappears in molecular disruption or production of heat, or both combined. The definition of *motion* confined to the natural standard of living force would conform to the causal relation of dependent action.

§ 13.

Newton and Laplace's arguments in favour of the simple ratio of the velocity being the natural standard of motion.

Newton employed the word motion to denote the velocity of a body conjointly with its quantity of matter, and it is evident that, confining the word to this meaning, the correctness of the 'Principia' is no argument in favour of such definition applying to the natural standard. There is no doubt, however, that both Newton and Laplace believed it to be the natural standard. It may assist in setting the subject in a clear light, if we take a deliberate view of the arguments they have advanced.

In Dr. Brewster's 'Life of Newton,' vol. ii. p. 379, there is given what is stated to be Newton's argument as opposed to Leibnitz, who had asserted the force of falling bodies to be as the square of their velocities. It is based on the assumption that fine threads of silk may be broken with less and less force as the velocity of the body that breaks through them increases. These threads individually may be taken to represent the resistance that gravity opposes to upward motion through a constant element of the vertical, as they are supposed to be arranged at equal distances, and each requires the expenditure of an element of force, or square velocity, to rend it. The retardation of the motion of a globe through such threads horizontally, is precisely similar to the retardation of the motion of a body projected up a vertical. In an element of time there is a loss of an element of velocity, or in an element of space there is the loss of an element of square velocity. Thus the argument derived from such an instance is not conclusive; it is not, any more than the laws of falling bodies, decisive of the question.

Laplace's argument is stated in the *Système du Monde,* p. 300 (Pond's translation), as follows:—

"It is observed upon the earth that a body solicited by any force moves in the same manner, whatever be the angle which the direction of this force makes with the direction of the motion which is common to the body and to the part of the terrestrial surface to which it corresponds: the same thing takes place in a vessel whose motion is uniform: a *moveable body submitted to the action of a spring or of gravity,* or any other force, moves *relatively* to the parts of the ship in the same manner, whatever be the velocity and direction of the vessel. It may then be established as a general law of terrestrial motions, that if in a system of bodies carried on by a common motion, *any force be impressed on one of them, its apparent or relative motion* will be the same, whatever be the general motion of the system and the angle which its direction makes with the impelling force. *The proportionality of force to velocity results from this law, supposed rigorously exact; for if we suppose two bodies moving upon one straight line with equal velocities, and that by impressing on one of them a force, which increases the primitive force, its relative velocity to the other body remains the same as if both of* *them had been primitively in a state of repose*: it is evident that the space described by the body in consequence of its primitive force, and of that which is added to it, becomes equal to the sum of the spaces which each of them would have caused it to describe in the same time; which supposes the force proportional to the velocity."

We may understand, from the instance of collision given in § 10, that the fact of relative motion being the same whatever the general motion of the system, is quite consistent with the proportionality of force to square velocity. It may seem at first somewhat of a paradox, that on striking a billiard ball in a westerly direction at noon so as to give it a velocity of 10 feet per second, we should actually communicate to that ball such a force or motion as would give it a velocity of 1500 feet per second if the earth were at rest; but the difficulty disappears when we consider that *the force of impact or shock is made up of the difference between the sum of the absolute motions before impact, and at impact or just before recoil begins, and that this difference is proportional to the square of the relative velocity.* The transference of force from one body to another depends on the *absolute velocities,* so that it is physically possible to transfer any amount of force, however large, by an impinging velocity, however small. The shock of impact, and the amount of force transferred, do not stand in the relation of cause and effect. The one is not a function of the other, and the force of impact indicates nothing in regard to the "*impressing of force*" or "*increasing of force*" which depend on the absolute velocities, of which we cannot be said to know anything certain.

Take the instance of a ball swinging as a pendulum within a railway carriage in the direction of its motion. Whether the carriage is at rest or in uniform motion at any speed, the motion of the pendulum relative to the carriage is the same. This supposes the mass of the carriage to be incomparably greater than the ball. When they draw near to each other, as with a gunner's eprouvette, the carriage sensibly reciprocates the oscillation of the suspended cannon. Suppose between the pendulum ball and point of suspension, a spring is interposed and the force on this spring resolved in a horizontal direction. This horizontal part accumulates to a maximum in one direction at the lowest part of the arc of oscillation, then changes the direction of strain, and accumulates to a maximum in the opposite direction; in one-half of the arc pulling forwards and in the other pulling backwards. The amount of strain is like the force of impact, a function of the *relative* velocity: the force transferred backwards and forwards from the carriage to the ball, and *vice versâ,* is a function of the *absolute* velocities.

§ 14.

On the Transfusion of *vis viva* across a system that simultaneously revolves and progresses.

Perhaps the most difficult point to realize to the mind consistently with the causal relation of dependent action, is the periodical ebbing and flowing of *vis viva* from and to each part of a system endowed with a rectilineal as well as a rotatory movement. In the simple example of two equal globes, given in § 11, as often as they are in a line perpendicular to the line described by the common centre of gravity, the *vis viva* of one (A) is 49, and of the other (B) 1. Setting out from this, A loses and B gains *vis viva* until they come to the line in which the common centre of gravity moves, when A is found reduced to 25 and B has risen to the same amount, having in the quarter revolution gained 24 parts of square velocity which has been transferred to it from A. During the next quarter another 24 parts have passed over. The ebb of force from A here ceases, and is succeeded in the next quarter revolution by a flow of *vis viva* from B to A, which continues until the revolution is completed, when the same circle of changes is repeated. This transference of force from one part of a system to another being unaccompanied with any appearance of action, may be termed *transfusion*, as in the heading of this paragraph; it is nevertheless as real a transference of force as what takes place on a billiard table.

How are we to present such phænomena to our minds in conformity with Newton's second law of motion, viz. "*The alteration of motion is ever proportional to the motive force impressed, and is made in the direction of the right line in which that force is impressed*"?

Let us take, for example, the system formed by the earth and the moon. At full moon the absolute motion of our satellite considerably exceeds the amount it contains when at the opposite point of its orbit, and in the earth it is *vice versâ*. How may we conceive this flux and reflux to take place?

In § 6 a dynamic arrangement was suggested, which represented the peculiarity in the development of mechanical force, specified in §§ 4 and 5, as a physical characteristic deduced from the law of elastic impact: is it possible to deduce transfusion from the same primary mode of action?

Let there be two perfectly elastic plates of equal surface and mass placed opposite and parallel to each other; on the exterior surface of each, let a current of perfectly elastic particles be supposed to impinge continually; the minuteness as well as velocity of these particles being excessive. The effect of the current would be to drive the plates towards each other with an accelerative force (§ 6). Let the velocity acquired at the end of nine seconds be 9, and in the next second 10; the square velocity increases in one second from 81 to 100, the augmentation in each being 19, or 38 in the sum of both. Let us now suppose the plates, before the respective currents set upon them, to have had a uniform velocity of 20 feet in the direction AB. In nine seconds after the currents commenced to act, the velocity of A has become 29, and that of B 11. In the next second, A is up to 30 and B down to 10, and in one second the square velocity in A has increased from 841 to 900, and that of B decreased from 121 to 100. The sum of both has during this second increased from 962 to 1000; the augmentation being 38, as before, when the common centre of gravity was at rest. It will be remarked that the relative motion of A and B, as well as the increase of their united *vis viva,* is the same in this last case as in the former; nevertheless there has been a transfusion of force from B to A, the amount of which is 40, being equal to twice the velocity of the common centre of gravity. If A had received no greater augmentation of square velocity than in the former case, the amount at the end of the tenth second would be $841+19=860$, but it is actually 900. Again, if B had received an augmentation the same as in the previous case, the amount at the end of the tenth second would be $121+19=140$, but it is 100. Hence 40 has been transfused from B to A.

If the motion of the common centre of gravity had been in a direction perpendicular to AB, the relative motion and augmentation of united *vis viva* would be the same, but there would be no transfusion; it is only when the current acts in the direction of or contrary to the absolute motion.

Suppose the plates A, B to revolve around their common centre of gravity with such velocity that the centrifugal tendency just equilibriates the effect of the currents. So long as the centre of gravity is at rest, no force is given to or taken from the impinging currents or particles; but when rectilineal motion is given to the system, transfusion takes place. The greatest rate at which this takes place is when A and B are in the line of their absolute motion. At other parts of the revolution, when A and B have a position oblique to the direction of absolute motion, the rate of transfusion is diminished in the ratio of the cosine of the contained angle. During one half of a revolution, the absolute motion resolved in the direction of the current goes with the current upon one of the plates and against it on the other, so that the flux and reflux of the force upon each plate alternates positive and negative to exactly the same amount.

It is thus that we may obtain a clear conception of transfusion as a necessary sequent to the mode of action of the forces of gravity, cohesion, &c., viewed in their dynamic aspect as conforming to impulses derived from elastic impact.

§ 15.

Destruction and Reproduction of Force in Nature is apparent, not real.

In the mutual action of bodies there is an apparent destruction and reproduction of force, but the more such phænomena are investigated experimentally with respect to their quantitative elements, the more reason there is to believe that the disappearance and reappearance are merely transference from one agent to another, and are as exactly regulated as the communication of force by impact in the preceding examples.

Even if we had not those proofs of the existence of a universal medium that the undulatory theory of light supplies, it would be well to admit the existence of media to which ordinary matter may be assumed to be entirely subordinate in all their potential relations, so as to give order to our ideas in tracing out the dynamical sequence of nature. It would be taking too narrow a view if we limited the function of the luminiferous æther to the conveying of physical pulses only. The atmosphere also conveys physical pulses, but that is the least important of its functions in the economy of nature. There is nothing that should hinder us attributing to the media concerned in the radiation of heat and light, the higher functions of electric polarity and gravitation. The special dynamic arrangements by which this is effected may ever elude research, but as there is no limit to the *vis viva* which such media may conserve in their minutest parts, so there is no physical impossibility in that *vis viva* being suddenly transferred to the molecules of ordinary matter in the proportions and sequence required to carry out the order and system of nature.

The fundamental principle of action in such media must be in accordance with *elastic impact,* for upon that the dynamic theory of heat and conservation of force rests as a foundation. The statical and dynamical characteristics of gravitation and transfusion of force have also been shown to conform to it, so that all the forces that hold the molecules of bodies together must also be in subjection to it.

Force issues from such media and becomes apparent in ordinary matter as *vis viva.* Force disappears from ordinary matter, and being transferred to the media, becomes non-apparent or latent. When we raise a weight from the ground, we give force (A) to the agent of gravitation; when we let fall the same weight, we take force (B) from the agent. Directing attention to the *mode* of raising the weight, we inquire where was A before it was given to the agent; what other agent held it? Also what becomes of B; what new agent holds it? Suppose the weight was raised by the action of steam, we trace it back to the force engendered by the chemical action of the fuel combining with oxygen. The force of the chemical action is transferred to the molecules of water and makes its appearance as molecular *vis viva.* Before the chemical action, the relation of A to the molecules about to combine is similar to the relation of force to a body about to fall from a height. The force about to be given is concealed in an agent, a medium, the instrument of chemical or electric force; is evolved from it, and returns to it again when the products of combustion are decomposed at a future time.

Thus may every appearance in nature be viewed as the exhibition of a part of a cycle of dependent action; and we have reason to be impressed with the necessity of keeping the dynamical aspect of a phænomenon in view, if we desire to explore its relations with others, and its place in the general scheme.

Edinburgh, March 18, 1858.

XLIII. *On the Law of Storms.* *By* Prof. James P. Espy. *To W. Francis, Esq.*

Sir, Washington City, March 9, 1858.

I SEND you this paper, not for the purpose of demonstrating anything, but to induce the various learned bodies in your country, especially those which are engaged at present in similar investigations, to take up the subject and examine whether the wind, as I maintain, blows in towards the centre of great storms, hurricanes, and tornadoes, according to the evidence furnished in my 'Fourth Report' on Meteorology herewith sent to you.

I have now examined 170 storms as they pass over the United States from west to east, of which 100 were given in the preceding Report, and more than fifty in this, from which I have drawn more than twenty generalizations, of which the first three are the following:—

The rain- and snow-storms, and even the moderate rains and snows, travel from the west towards the east in the United States.

The storms are accompanied with a depression of the barometer near the central line of the storm, and a rise of the barometer in front and rear.

This central line of minimum is, in the cold months, of great length north and south, and travels with the storm towards the east, side foremost.

In great storms, the wind, on both sides of the line of minimum pressure, blows towards that line directly or obliquely.

I have examined also Redfield's, Reid's, Piddington's, and Thom's storms in the following manner:—In reading the account of a storm, I spread the chart on which was laid down the position of the ships in the storm, and drew arrows showing the direction of the wind at a particular moment, and I found that all the arrows of the ships within the violence of the hurricane pointed inwards to a central space of no great magnitude.

So of tornadoes: they have been examined as they pass through forests and cities, by Bache, Henry, Walter Johnson, Loomis, Olmsted, Prof. Eustice and the Rev. Mr. Brooks; and they all declare, without a dissenting voice, that the air blew inwards, and threw down the trees at the sides perpendicularly towards a central line in which the tornado moved, occasionally throwing bodies backwards by the front of the tornado, and then forwards by the rear.

The inward motion below, indeed, might be inferred from the upward motion in the middle, which was so violent as to carry up all kinds of materials from the surface of the earth, and let them fall many miles from where they were taken up, along with a severe shower of hail, imbedding in it sand and leaves, and limbs of trees covered with a thick coating of ice.

In storms then the air comes inwards on all sides towards the centre.

Therefore, It goes upwards.
It comes under less pressure.
It expands.
It grows colder about one degree for 100 yards of ascent.
It begins to condense its vapour at a certain height*.
It liberates the caloric of the condensing vapour.
It then cools only half as fast as in going up, as dry air would.

When vapour enough is condensed in the cloud to make one inch of rain, sufficient latent caloric will be evolved to hinder the air from cooling, so much that the whole column of air containing the cloud, from the surface of the earth to the top of the atmosphere, if the heat was equally distributed, would be 10° warmer than the surrounding air.

The column then containing the cloud would be $\frac{10}{448}$ lighter than the surrounding air (the mean temperature of the air being zero). The air then would run in at the base of the light column, and push it up with great velocity, and carry in its vapour with it, which, on going up, would condense as before, give out

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[*] At 1000 yards if the dew-point is 10° below the temperature of the air, and at 1500 yards if the dew-point is 15° below the temperature of the air.

its latent caloric, expand the air in the cloud, spread out on all sides, and roll out above at the top of the atmosphere, making the barometer rise on all sides round on the outside of the cloud, at a distance from the upmoving column in proportion to the diameter of the cloud, whilst under the cloud the barometer would fall; and when the barometer falls one inch, the air will be pressed up, in case an allowance is made for friction, with a velocity of 240 feet per second.

If the base of such an upmoving column as this were to remain stationary over the same region, it would throw down, in rain or hail, in a few hours ten times as much as is contained in the air at any one time over the region of the rain.

If vapour enough is condensed in a cloud to make one inch deep of rain, it is plain that the same quantity of caloric is evolved into the air which would be required to evaporate that inch deep of rain into the air; and it will be found by calculation that it will require 644,700 tons of coal to be burnt to evaporate the water which would cover ten miles square one inch deep, on the supposition that one pound of coal would evaporate 13 lbs of water; and if the specific caloric of air is one-fourth of that of water, this amount of coal burnt in the air would heat the whole atmosphere from top to bottom 10°; for one inch deep of rain is $\frac{1}{400}$th of the weight of the whole atmosphere over ten miles square; and it is known by experiment that if one pound of steam is condensed in 400 pounds of water it will heat it $2\frac{1}{2}°$, and the specific caloric of air being only one-fourth of that of water, one pound of steam being condensed in 400 pounds of air will heat that air 10 degrees.

I say this to excite the philosophers in your country to examine my theory of storms. But fortunately the leading fact, the in-blowing of the air, can be determined without studying the theory, and that too with but little labour.

The fact can be determined by the data furnished by examining the storms of Redfield, Reid, Piddington, and Thom. (For the only storm of Thom which I have examined, see pp. 28 and 29 of my 'Fourth Report,' and for the others see p. 101 *et seq.*)

I find also in the storm of the Black Sea of Nov. 14th, 1854, as laid down in the first Number of Meteorological papers published by the authority of the Board of Trade, that if arrows be drawn representing the course of the wind at 10 o'clock, at all the different places where the storm was violent at that hour, they will all point inwards to a common centre, nearly about the south-west side of the Sea of Azov. Perhaps by this time observations in that sea may be obtained, and if so, I venture to predict that the wind there was from some eastern or north-eastern direction at the same hour, according as the observations were near the southern or northern part of that sea. Now I most earnestly hope, if you publish these few remarks, that the subject will at least appear worthy of examination, and that very many learned bodies (and especially the Board of Trade) will appoint committees of men who have the confidence of these bodies, with instructions to examine whether or not the wind did blow inwards in all the storms which I have examined of Redfield, Reid, Piddington, and Thom at the time mentioned in my diagrams. I doubt not the result.

In all the great winter storms of the United States, their north and south diameter is four or five times greater than their east and west; and it is not certain that hurricanes are all round, and if not, the wind will blow in towards a line rather than towards a point; and even if it blows inwards spirally, that is not inconsistent with my theory.

In narrow storms of great length it will hardly be contended that there could be a whirl, nor in the belt of rain at the equator. There the wind blows inwards nearly perpendicular to the equator near the belt of rain, and yet the barometer stands below the mean under the belt, which shows that the air runs out above, and keeps the barometer above the mean at the outer borders of the trade-winds, just as it does both in front and in the rear of all the great storms which pass over the United States.

I think if Sir John Herschel examines this subject, he will retract what he said nearly twenty years ago, before the British Association, "That the leading fact of Espy's theory could not be true, for if the wind blew inwards towards the centre of storms, it would make the barometer rise there above the mean." And if he shall find, as I think he will on examination, that the theory explains more than a hundred phænomena never explained before, many of which were not known before, but predicted by the theory, he will cheerfully acknowledge that the long chain of cause and effect exhibited by my theory of storms, is a far more magnificent specimen of inductive philosophy than Wells' theory of dew, which he so justly praises. I have tried every means in my power for years to induce Sir John to examine my theory carefully, and then re-affirm his former statement or retract it. Perhaps this eulogium of my own theory may induce him, from its very impudence, to take up the subject. If he does, he will find that atmospheric waves, which seem to trouble him, are the very reverse of what is generally believed; the depth of the atmosphere is greater where the barometer stands low, and less where the barometer stands high, and that in proportion to the fall of the barometer. Over all great storm-clouds, the air is swelled up by the latent caloric evolved by the condensing vapour; and the higher it is swelled up the more the barometer falls under the cloud, because the higher it swells up the more runs off at the top. I am now nearly seventy-three years of age, and it would gratify me much to see my theory universally received before I die; but my gratification is a small matter compared with the interest of all mankind in adopting the true, instead of a false system. It is painful to me to see the whole meteorological world groping in the dark, for more than twenty years after the true system has been developed.

Again I most earnestly entreat that the subject be taken up by many of the numerous learned societies in Great Britain and Ireland.

Very respectfully, &c.,
James P. Espy.

P.S. I have left some copies of my 'Fourth Report' at the Smithsonian Institution, at Washington City, to be sent to any Society that will promise to investigate the subject carefully and report upon it.—J. P. E.

XLIV. *On the Percussion of Bodies.* *By* M. Poinsot.

[Continued from p. 290.]

Corollary III.

On the points at which the body is capable of producing the same percussion.

23. To find the points at which the body strikes with a given force nP, we have merely to make

$$Q=nP$$

in the general expression for Q. By so doing, the equation of the locus of these points will be found to be

$$n(\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2)=\alpha^2ax+\beta^2by+\alpha^2\beta^2.$$

By transforming to parallel coordinate axes of X and Y whose origin is at the point $x=\frac{a}{2n}$, $y=\frac{b}{2n}$, this equation becomes

$$4n^2(\alpha^2X^2+\beta^2Y^2)=\alpha^2a^2+\beta^2b^2-4\alpha^2\beta^2(n^2-n),$$

or, in a simpler form,

$$4n^2\left(\frac{X^2}{\beta^2}+\frac{Y^2}{\alpha^2}\right)=\frac{H}{A}-4(n^2-n).$$

When the second part of this equation is positive, the curve is evidently an ellipse similar, and similarly placed to the central ellipse, but having its centre at the point whose abscissa is $x=\frac{a}{2n}$, and whose ordinate is $y=\frac{b}{2n}$.

If the expression on the right of the last equation is zero, the curve becomes reduced to a single point, and we may remark that this point is precisely that which constitutes the centre of *maximum* percussion; for to make

$$\frac{H}{A}-4(n^2-n)=0$$

is equivalent to giving to the number n one of the values

$$n=\frac{1\pm\sqrt{1+\frac{H}{A}}}{2},$$

and consequently to the supposition that the given percussion nP has one of the values

$$Q=P\frac{1\pm\sqrt{1+\frac{H}{A}}}{2};$$

but these are precisely the values of the two *maxima* percussions; the positive one corresponding to the greatest of all percussions in the same direction as the impulsion P, and the negative one to the greatest of all the percussions in the opposite direction.

Lastly, if the second part of our equation were negative, the ellipse would be imaginary, and there would be no centres of the given percussion nP.

Hence the curve of the centres of equal percussion nP is an ellipse similar to the central ellipse, or a point, or an imaginary curve, according as

$$H-4A(n^2-n)>0, \text{ or } =0, \text{ or } <0.$$

24. It is easy to see why, in the last case, there is no point capable of producing the given percussion nP. For to suppose that

$$H-4A(n^2-n)$$

is negative, is to suppose, in the case of n positive, that this number n has a value greater than $\frac{1}{2}+\frac{1}{2}\sqrt{1+\frac{H}{A}}$, and thus that the centre of a positive percussion greater than the greatest of all such percussions is demanded. And similarly, in the case of n negative, the above supposition is equivalent to giving to n an absolute value greater than $-\frac{1}{2}+\frac{1}{2}\sqrt{1+\frac{H}{A}}$, and thus to demanding the centre of a negative percussion greater than the greatest of all negative percussions.

COROLLARY IV.

PARTICULAR CASES OF THE FOREGOING THEOREMS.

25. If, in the general expression

$$Q=P\,.\,\frac{\alpha^2ax+\beta^2by+\alpha^2\beta^3}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2}$$

(see art. 18), we suppose $a=0$ and $b=0$, we have the particular case where the impulsion P passes through the centre of gravity G of the body, and consequently where the body is animated only by a translatory motion in the direction of its principal axis GZ.

In this particular case, therefore, the percussion which the body is capable of producing at any point of the principal plane XY is

$$Q=P\frac{\alpha^2\beta^2}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2},$$

the *maximum* of which corresponds to $x=0$ and $y=0$; so that the centre of *maximum* percussion then coincides—as it clearly should—with the centre of gravity itself.

26. The curve formed by the points of equal given percussion nP will now be represented by the equation

$$\alpha^2x^2+\beta^3y^2=\alpha^2\beta^2\left(\frac{1}{n}-1\right),$$

whence we see that these points are situated on an ellipse similar to the central ellipse, described around the same centre as the latter, and placed in a similar manner. The magnitude of this ellipse will depend upon the constant value nP given to Q, which value, however, must not be assumed greater than P, since P is now the greatest force of percussion that the body can produce.

If, for example, we suppose

$$Q=\tfrac{1}{2}P,$$

we shall have

$$\alpha^2x^2+\beta^2y^2=\alpha^2\beta^2,$$

the equation of the central ellipse; whence we conclude that the contour of this ellipse is the locus of the points at or with which the body strikes with a force equal to half that of the impulse by which it is animated.

COROLLARY V.

ON THE PARTICULAR CASE WHERE THE BODY IS ANIMATED SOLELY BY THE IMPULSION OF A COUPLE.

27. Let us give to the general expression for Q the form

$$Q=Pa\frac{\alpha^2x+\dfrac{b}{a}\,\beta^2y+\dfrac{\alpha^2\beta^2}{a}}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2},$$

and let us assume that P, a, b approach, indefinitely, the respective limits 0, ∞, ∞, but in such a manner that the ratio $\frac{b}{a}$ remains unaltered, and that the products or moments Pa and Pb always maintain the same finite values as if P, a, and b had not changed: we shall then have ultimately the particular case where the body is animated solely by the impulsion of a couple (P, −P) applied to the lever arm $CG = \sqrt{a^2+b^2}$.

To express the percussion which the body is capable of producing at any point whose coordinates are x and y, we shall then have the formula

$$Q = P\frac{\alpha^2 ax + \beta^2 by}{\alpha^2 x^2 + \beta^2 y^2 + \alpha^2\beta^2}.$$

This result may easily be verified by seeking, directly, the percussion which the body is capable of producing when it is animated solely by the impulsion of the couple in question.

Without allowing P, a, and b thus to approach the particular values 0, ∞, and ∞, the conception of which is always somewhat obscure, we may deduce the same result from the expression (A), art. 18, in a very simple manner.

28. In fact, since

$$P\frac{\alpha^2 ax + \beta^2 by + \alpha^2\beta^2}{\alpha^2 x^2 + \beta^2 y^2 + \alpha^2\beta^2}$$

expresses the percussion due to a single force P applied at C, whose coordinates are a and b,

$$-P\frac{\alpha^2 a'x + \beta^2 b'y + \alpha^2\beta^2}{\alpha^2 x^2 + \beta^2 y^2 + \alpha^2\beta^2}$$

will express the percussion due to a single force −P equal, parallel and opposite to P, but applied at C′ whose coordinates are a' and b'; consequently the percussion due to the combined forces, that is to say, to the couple (P, −P), will be expressed by

$$Q = P\frac{\alpha^2(a-a')x + \beta^2(b-b')y}{\alpha^2 x^2 + \beta^2 y^2 + \alpha^2\beta^2}. \quad . \quad . \quad . \quad . \quad (B)$$

In the case where a' and b' are each zero, this expression coincides with, and confirms the one previously given, for it then corresponds to the percussion of the couple whose lever arm CC′ coincides with CG.

29. Since a couple may always be transported anywhere in its own plane, or in any plane parallel thereto, and transformed into innumerable other couples, having the same moment, without its effect upon the body being thereby changed*, it is evident that the percussion Q ought to remain the same in whatever ad-

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* *Eléments de Statique*, art. 49 *et seq.*

missible manner the couple to which it is due may have been represented in the figure. In fact, the preceding expression (B) shows this; for by it, it is clear that the value of Q does not depend upon the five particular values of P, a, b, a' and b', but solely upon the values of the two products $P(a-a')$ and $P(b-b')$, which are the respective moments of the proposed couple relative to the axes of y and of x. On this account we shall find it more convenient to preserve these moments, solely, in the formula (B), and to represent them by the simple letters L and M, which latter will suffice to define at once the magnitude and the position of the given couple under consideration. The expression (B) is thus replaced by the more concise one

$$Q=\frac{L\alpha^2x+M\beta^2y}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2}, \quad . \quad . \quad . \quad . \quad . \quad (B')$$

in which nothing, beyond the necessary data of the question, is visible.

Before proceeding further, a brief remark still remains to be made.

30. In art. 28 we have seen how, from the formula (A) relative to the impulsion of a *single* force, the formula (B) relative to the impulsion of a *couple* may be deduced. Now it may be shown that, conversely, from the latter, supposed to be known —and it would be easy to demonstrate it directly—we may also deduce the former. In fact, let it be required to find the percussion Q which the body is capable of producing at any point D, whose coordinates are x and y, in virtue of a single impulse P which the body has received at the point C, whose coordinates are a and b. We may consider the simple force applied at C to be decomposed into an equal, parallel and like-directed force applied at D, and into a couple (P, $-$P) applied to the arm CD. Now the force applied at the point D will evidently there cause a percussion equal to P; and according to the formula (B), the couple, on its part, will cause a percussion at the same point D equal to

$$P\frac{\alpha^2(a-x)x+\beta^2(b-y)y}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2}.$$

The required percussion Q, therefore, being the sum of these two, is

$$Q=P\frac{\alpha^2ax+\beta^2by+\alpha^2\beta^2}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2}:$$

this is precisely the formula (A) which was demonstrated at the commencement.

Let us now return to the case where the body is animated by a couple only.

ON THE CENTRE OF *MAXIMUM* PERCUSSION.

31. The percussion due to a couple, whose two moments with respect to the two principal axes α and β are L and M, being expressed by

$$Q=\frac{L\alpha^2x+M\beta^2y}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2},$$

and the coordinates x and y of the centre D, where the percussion is a *maximum,* satisfying the equations

$$\frac{dQ}{dx}=0, \text{ and } \frac{dQ}{dy}=0,$$

we easily find

$$x=L\frac{\alpha\beta}{\sqrt{\alpha^2L^2+\beta^2M^2}},$$

$$y=M\frac{\alpha\beta}{\sqrt{\alpha^2L^2+\beta^2M^2}};$$

from which we immediately deduce

$$x:y=L:M,$$
$$\alpha^2x^2+\beta^2y^2=\alpha^2\beta^2.$$

The centre of *maximum* percussion, therefore, is at once upon the central ellipse and upon the intersection of a plane through the origin G, parallel to the plane of the couple. Consequently each extremity of the diameter 2δ determined by the plane of the couple is such a centre.

If we represent by (Q) the value of this *maximum* percussion, the preceding equations give

$$(Q)=\frac{\sqrt{\alpha^2L^2+\beta^2M^2}}{2\alpha\beta},$$

or, since

$$\delta^2=x^2+y^2=\frac{\alpha^2\beta^2(L^2+M^2)}{\alpha^2L^2+\beta^2M^2},$$

the simpler expression

$$(Q)=\frac{\sqrt{L^2+M^2}}{2\delta},$$

which is nothing more than the moment of the couple of impulsion divided by the length of the diameter parallel to its plane.

Hence when a body is animated solely by the impulse of a couple perpendicular to one of its three principal planes, or, in other words, when a body actually turns around a diameter $2\delta'$ of the central ellipse described in this plane, the point where it strikes with the greatest violence is at one of the extremities of the diameter 2δ conjugate to $2\delta'$*; at the other extremity of 2δ the body strikes with the same force, but in an opposite direction.

The magnitude of this percussion is measured by that of the couple divided by the diameter 2δ which the plane of the couple determines in the central ellipse.

32. A close relation, too, may be established between this theorem and that in art. 20, so as to include both in one enunciation. For without distinguishing the two kinds of impulsion which the body may have received, and regarding merely the motion it possesses, it is evident that in both cases we have to consider a body which actually turns around a spontaneous axis OS situated in one of its principal planes. Now in art. 20 we found the centre of *maximum* percussion D to be situated in the direction GO of the diameter 2δ conjugate to the direction of the spontaneous axis, and its distance λ from the point O to be

$$\lambda = \pm \sqrt{A^2 + AH};$$

or, since $AH = \delta^2$ (see art. 3),

$$\lambda = \pm \sqrt{A^2 + \delta^2},$$

where the letter A represents the distance GO.

But, ϕ being the inclination of the diameter δ to the spontaneous axis OS or to its parallel, the conjugate diameter δ', $\delta \sin \phi$ will be the arm of inertia of the body around δ', and consequently

$$\sqrt{\delta^2 \sin^2 \phi + A^2 \sin^2 \phi}$$

the arm of inertia around OS; hence representing this line by K, as usual,

$$\lambda \sin \phi = \sin \phi \sqrt{A^2 + \delta^2} = K.$$

Hence we may say, in general, if a body is actually animated by a motion of rotation around a spontaneous axis situated anywhere in one of its three principal planes, the two points of this plane at which the body strikes with the greatest possible force

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* The couple L tends to make the body rotate around the axis of y with an angular velocity equal to $\frac{-L}{m\beta^2}$, and the couple M, alone, would produce a rotation around the axis of x whose velocity would be $\frac{M}{m\alpha^2}$. By the composition of rotations, therefore, the axis $2\delta'$, around which the body will actually rotate, makes an angle with the axis of x whose tangent is $-\frac{L}{m\beta^2} : \frac{M}{m\alpha^2} = -\frac{\alpha^2}{\beta^2} \cdot \frac{L}{M}$; but the diameter 2δ, parallel to the intersection of the plane of the couple, is inclined to the axis of x at an angle whose tangent is $\frac{M}{L}$; hence, the product of both tangents being $-\frac{\alpha^2}{\beta^2}$, 2δ and $2\delta'$ are conjugate diameters (see art. 3).

are to the right and left of this spontaneous axis, and at a distance $\lambda \sin \phi$ from the same precisely equal to the arm K of inertia of the body with respect to this axis. This property, together with that of being situated in the diameter of the central ellipse conjugate to the direction of the spontaneous axis, completely determine the positions of these centres of *maximum* percussion.

This theorem, therefore, applies to all possible positions of the spontaneous axis in the principal plane under consideration. When this axis passes through the body's centre of gravity, as was the case in the preceding article, we have $A=0$, and the distance $\lambda \sin \phi = K$ reduces itself to $\pm \delta \sin \phi$, as already found in art. 31.

COROLLARY VI.

ON A NEW EXPRESSION FOR THE FORCE Q WITH WHICH EACH POINT IS ENDUED IN VIRTUE OF THE ROTATION OF THE BODY.

33. In art. 18 we found that when a and b represent the coordinates of the point C where the body received the impulse P to which it owes its motion, and x and y the coordinates of any point D, the force Q with which the body strikes an obstacle presented at D may be expressed by

$$Q=P\frac{\alpha^2 ax+\beta^2 by+\alpha^2\beta^2}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2}.$$

If from the point D, whose coordinates are x and y, we let fall a perpendicular upon the spontaneous axis whose equation is

$$\alpha^2 at+\beta^2 bu+\alpha^2\beta^2=0,$$

its length π will be found to be

$$\pi=\frac{\alpha^2 ax+\beta^2 by+\alpha^2\beta^2}{\sqrt{\alpha^4a^2+\beta^4b^2}},$$

and the above expression for Q will take the form

$$Q=P\,.\,\frac{\pi\sqrt{\alpha^4a^2+\beta^4b^2}}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2};$$

but, in virtue of the preceding expressions,

$$CG=H=\sqrt{a^2+b^2},$$

$$OG=A=\frac{\alpha^2\beta^2 H}{\alpha^2a^2+\beta^2b^2},$$

and of the relation

$$A\,.\,H=\delta^2,$$

the radical which enters into the expression of Q will have the value

$$\sqrt{\alpha^4a^2+\beta^4b^2}=\frac{\alpha\beta\delta\delta'}{A},$$

where $2\delta'$ is the diameter parallel to the spontaneous axis OS, and 2δ its conjugate diameter. By substitution, therefore, we deduce the simpler expression

$$Q=\frac{P\pi}{A}\cdot\frac{\alpha\beta\,.\,\delta\delta'}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2}.$$

Further, let θ be the angular velocity with which the body rotates around the spontaneous axis OS, and ϕ the inclination of OG to OS or, what is the same, of δ to δ'; it is evident that $\theta\,.\,\overline{OG}\,.\sin\phi=\theta\,.\,A\sin\phi$ is the velocity of the centre of gravity G of the body, and consequently that the impulse P which set the body in motion has the value

$$P=m\theta\,.\,A\sin\phi,$$

m being the mass of the body.

If, in place of P, in the above formula we substitute this its value, and remember that $\delta\delta'\sin\phi=\alpha\beta$, we shall find

$$Q=\theta\pi\,.\,\frac{m\alpha^2\beta^2}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2}.$$

The factor $\theta\pi$ in this expression represents the velocity of the point D, whose coordinates are x and y, and the other factor is a part of the whole mass m of the body expressed by the fraction

$$\frac{\alpha^2\beta^2}{\alpha^2x^2+\beta^2y^2+\alpha^2\beta^2};$$

hence we may say that, in the movement of the body, the point in question strikes with the same force as it would do if this fraction of the mass were there concentrated.

Similarly, if D' were any other point having the coordinates x and y', π' its shortest distance from the spontaneous axis, and Q' the percussion which it is capable of producing, we should find

$$Q'=\theta\pi'\,.\,\frac{m\alpha^2\beta^2}{\alpha^2x'^2+\beta^2y'^2+\alpha^2\beta^2}.$$

If we suppose D' to be the reciprocal of the point D, then, by art. 2, we shall have, between the coordinates of these two points, the relations

$$x'=\frac{-\alpha^2\beta^2x}{\alpha^2x^2+\beta^2y^2},\quad y'=\frac{-\alpha^2\beta^2y}{\alpha^2x^2+\beta^2y^2};$$

these values of x' and y', being substituted in the last expression, give

$$Q' = \theta\pi' . \frac{m(\alpha^2x^2 + \beta^2y^2)}{\alpha^2x^2 + \beta^2y^2 + \alpha^2\beta^2}.$$

Here also the first factor $\theta\pi'$ is the velocity of the point D$'$, and the other factor is the fraction

$$\frac{\alpha^2x^2 + \beta^2y^2}{\alpha^2x^2 + \beta^2y^2 + \alpha^2\beta^2}$$

of the body's whole mass m; but this and the preceding fraction together amount to unity, and by the foregoing relations their ratio $\alpha^2\beta^2 : (\alpha^2x^2 + \beta^2y^2)$ is the same as that of x' to x or y' to y, and consequently also of $\sqrt{x'^2 + y'^2}$ to $\sqrt{x^2 + y^2}$; in short, the ratio in question is the inverse of that of the distances u and u' of the two points D and D$'$ from the centre of gravity G.

We may say, therefore, that during the movement of the body the two reciprocal points D and D$'$ divide, as it were, the whole mass m into two parts μ and μ', inversely proportional to their distances from the centre of gravity G; and, further, that the percussions Q and Q$'$, which these two points are capable of producing, are the same as if these portions of the whole mass were respectively concentrated therein.

If we call Δ the semi-diameter of the central ellipse upon whose direction the two points D and D$'$ fall, we shall have

$$uu' = \Delta^2,$$

and the two portions μ and μ' of the mass m, which are

$$\mu = m\frac{u'}{u + u'} \text{ and } \mu' = m\frac{u}{u + u'},$$

may be expressed by

$$m\frac{\Delta^2}{u^2 + \Delta^2} \text{ and } m\frac{u^2}{u^2 + \Delta^2};$$

so that the percussions Q and Q$'$ will have the simpler expressions

$$Q = \theta\pi . m\frac{\Delta^2}{u^2 + \Delta^2},$$

$$Q' = \theta\pi' . m\frac{u^2}{u^2 + \Delta^2}.$$

These expressions are quite similar to those which we should obtain in the case of a rigid immaterial rod DD$'$, loaded at its extremities by the two massive points μ and μ', and, like the body itself, animated with the same rotation θ around the same spontaneous axis OS.

At the moment of the shock, therefore, we may imagine the body to be replaced by this rigid rod DD$'$,—or indeed by any other having the same centre G, the same mass m, and the same arm of inertia Δ. For not only at its two extremities, but at every other point of its direction,—considered as an inflexible line,—such a rod would be capable of striking an obstacle with the same force Q as the body itself at its corresponding point.

It is true that this rod, by moving with the same angular velocity in an opposite direction to the body, would not, on encountering the latter, be able to produce equilibrium and destroy all the body's motion; nevertheless, it would reduce to a state of rest all points along its own direction, so that the body would be able to preserve only a motion of rotation around this line. But a body which rotates around a line is incapable of producing any percussion at any point of that line; hence, if we regard merely the percussion which the body is capable of producing at any point of the line DD′, this percussion is exactly the same as that of the rod; it is in this sense only that, at the moment of the shock, the rod may be substituted for the body*.

NOTE. Mr. Hayward of University College, Durham, has kindly drawn my attention to the following oversight:—

At the end of the second paragraph of art. 21, Chap. I., instead of the words, "but this is always less than the impulse P," read "*but this is greater than, equal to, or less than the impulse* P *according as* K^2 *is greater than, equal to, or less than* $8a^2$." A correction of a similar kind is also necessary at the end of the last paragraph of art. 22, Chap. II.—T. A. H.

XLV. *On the Solar Spots and the Variable Stars.* *By* DANIEL VAUGHAN†.

IF the chemical phænomena of the heavens can be traced to determinate causes, we may confidently hope for a considerable extension of our knowledge respecting the fixed stars. It seems unreasonable to regard the development of light on these distant orbs, as dependent on operations wholly different from any which we are permitted to examine on our own globe. A careful study of the subject for many years has led me to conclude, that the space-pervading medium is the essential element of suns. As this fluid attains an adequate density by their powerful attraction, it undergoes a continual chemical action above their surfaces, and serves to dispense floods of heat and light to surrounding worlds. In the brief exposition of my views, which was sent to the last

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* M. Poinsot has announced, but has not yet published, the continuation of his memoir.

† Communicated by the Author.

meeting of the British Association for the Advancement of Science, I alluded to the influence of large planets in altering the amount of luciferous æther supplied to suns, and thus causing a periodical occurrence of spots on our own luminary, and far more decided changes in the brilliancy of many of the stars. On comparing my theoretical conclusions with the results of observations on the variable stars, the coincidence appears so satisfactory that I again venture to call attention to the subject.

The action of gravity must cause the æther of space to increase its density, not only on the surfaces of suns and planets, but also along the regions through which they have taken their rapid course. To prove this, it is only necessary to investigate the movements of an immense number of bodies not larger than cannon-balls; if they were scattered over an extensive tract of space, and were travelling in parallel paths with the same velocity, until they came within the range of the sun's attraction. It will be readily seen, that the several planes in which each of these bodies are urged by the solar power, must have a common intersection in a line coinciding with the direction of their primitive motion. If our sun should travel through a region occupied by a similar assemblage of bodies which had previously been at rest, he would compel each of them to describe hyperbolic orbits, all intersecting the line of his progressive motion; and the space along this line would be most densely populated by the transient masses. Now the influence of elasticity would prevent the particles of the interplanetary medium from describing similar orbits; but it could not change the planes of their motion, nor prevent all from intersecting the same line. The region from which the sun departs will accordingly be the focus to which the æther must press from all surrounding space; and here it will be concentrated in the greatest quantity, having its density much augmented by the conflict of opposing currents of enormous extent, and moving with an immense velocity.

Nor must it be supposed that the great elasticity of the ætherial particles will render the augmentation of density inconsiderable. The pressure of an atmospheric column, about 40 miles high, makes the air we breathe several thousand times more dense than it would be if the height of the atmosphere were only one thousand feet above the level of the sea. Though the modulus of elasticity should be many million times as great in the luciferous æther as in our common air, we must recollect that it has to contend with the weight and inertia of a far more extensive mass of the rare fluid. The pressure of a column, whose height is commensurate with the range of solar attraction, must increase to a considerable extent the density of the æther; while the repulsion of its particles would require years to restore it to the regions from which it was withdrawn. In like manner, large planets have the power of concentrating the ætherial medium in the regions over which they move; especially when their orbits are so wide, and the movement of the system so rapid, that their tracks in space deviate little from a straight line.

Had the planets moved in planes coincident with the line of solar motion, the columns of dense æther which the larger ones leave behind them, would alternately pass over the sun and cause a perceptible augmentation of his brilliancy. In many of the distant systems, the track of the central body has so small an inclination to the planes in which its attendants revolve, that each of these bodies takes some part in maintaining the great fire by which they are warmed and illuminated. The stars χ Cygni and Mira exhibit, in their change of magnitude, the peculiar phænomena which might be expected to result from such an arrangement. The mean time which the former star occupies in its course of variation has been estimated at 406 days; but it sometimes differs several days from this amount; and in its maximum brightness it varies from the fourth to the seventh magnitude. It appears, however, that these irregularities are most decided after a lapse of $8\frac{1}{2}$ and 100 periods of variation; and Argelander shows that its changes may be calculated with tolerable accuracy by a formula similar to those used for the determination of planetary perturbations. The variable splendour of this distant sun must be ascribed to the influence of three planets; one revolving around it in 406 days, another in 3451 days, and the third in 40,600 days; and being confined to planes which pass close to the line of the proper motion of the central luminary, are enabled to affect the development of his light. The effects of planets in condensing the ætherial contents of space, is also indicated by the fluctuating brilliancy of Mira; for though there are great inequalities in the time and the degree of its variation, they are periodical in their occurrence, and seem to be regulated by determinate laws.

Of the other variable stars, the greater part are in like manner characterized by such irregularities as might be expected to arise from the action of a plurality of worlds which attend them. In considering the conditions which planetary attraction requires to produce such effects, it would seem that the members of other systems, like those of our own, have the planes of their orbits confined to a limited range. There is, indeed, a great regularity in the decline and return of brightness in some stars of short periods of variability; but in these cases the effect must be mainly dependent on the presence of a large planet which revolves in a small orbit, and alters, in a sensible degree, the attraction of the central sphere. Though this alteration might diminish weight and pressure only in certain localities, yet, from the conditions of equilibrium in gases, it must reduce the density of all parts of the sun's luciferous atmosphere, and lead to a corresponding diminution of his lustre. Accordingly, when large planets describe orbits of small size and great eccentricity, they must exert a considerable influence on the illuminating power of the central orb, retarding the great conflagration when they are near, and allowing it to proceed with more vigour when they are most distant.

In condensing in the regions over which suns and worlds have passed, the æther must be withdrawn from more distant localities, and be there compelled to assume a more rarefied condition. Accordingly planets, whose planes deviate much from the direction of their sun's progressive motion, only curtail the supplies of his etherial fuel, and diminish the amount of heat and light which he diffuses around him. It is only in this way that the members of our own system can influence the production of light in our central luminary; and the slight effects which they occasion are exhibited in the periodicity of the solar spots. In consequence of the great mass of Jupiter, his effects on the vast illumination must preponderate over that of the other planets; and it may be observed, the period of his revolution does not differ very much from the interval between the times at which the spots appear in the greatest numbers.

Modern observations prove that the sun's spots are not only deficient in light, but also that they have a lower temperature than other parts of his surface. If, therefore, they are caused by exhalations from the solar mass, we may reasonably conclude that the gas which rises from his internal regions, only serves to interrupt his heating and illuminating action. If they are to be regarded as indications of a dark atmosphere beneath the luminous one, the mere situation of this dark envelope would be an evidence that it was derived from the sun himself, and would invalidate the opinion that he emits combustible gases. Indeed, if his heat and light were maintained by gases expelled from him by the high external temperature, the fuel must be supplied in the greatest abundance when the fire is most violent; and the great conflagration should steadily increase until the solar mass was entirely consumed, or continually decline until it sunk to perpetual darkness. Supported by the medium which pervades all space and burns in accordance with fixed laws, the brightness of suns can only fluctuate in obedience to the influence of attendant worlds; and future observations on the variable stars with better photometric instruments than we can now command, may yet enable astronomers to trace the movements of planets, in systems far too distant to be even explored with the telescope.

Cincinnati, Ohio, March 30, 1858.

XLVI. *On the Cubic Transformation of an Elliptic Function.*

By Arthur Cayley, *Esq.**

LET

$$z=\frac{(a', b', c', d' \between x, 1)^3}{(a, b, c, a \between x, 1)^3}$$

be any cubic fraction whatever of x, then it is always possible to find quartic functions of z, x respectively, such that

$$\frac{dz}{\sqrt{(\mathrm{a}, \mathrm{b}, \mathrm{c}, \mathrm{d}, \mathrm{e} \between z, 1)^4}}=\frac{dx}{\sqrt{(\mathrm{A}, \mathrm{B}, \mathrm{C}, \mathrm{D}, \mathrm{E} \between x, 1)^4}}.$$

This depends upon the following theorem, viz. putting for shortness,

$$\mathrm{U}=(a, b, c, d \between x, y)^3,$$

$$\mathrm{U}'=(a', b', c', d' \between x, y)^3,$$

and representing by the notation

$$\text{disct.}\ (a\mathrm{U}'-a'\mathrm{U},\quad b\mathrm{U}'-b'\mathrm{U},\quad c\mathrm{U}'-c'\mathrm{U},\quad d\mathrm{U}'-d'\mathrm{U});$$

or more shortly by

$$\text{disct.}\ (a\mathrm{U}'-a'\mathrm{U},\ldots),$$

the discriminant in regard to the facients (λ, μ) of the cubic function

$$(a\mathrm{U}'-a'\mathrm{U},\quad b\mathrm{U}'-b'\mathrm{U},\quad c\mathrm{U}'-c'\mathrm{U},\quad d\mathrm{U}'-d'\mathrm{U} \between \lambda, \mu)^3;$$

or what is the same thing, the cubic function

$$(a, b, c, d \between \lambda, \mu)^3 . (a', b', c', d' \between x, y)^3$$

$$-(a', b', c', d' \between \lambda, \mu)^3 . (a, b, c, d \between x, y)^3;$$

and by $\mathrm{J}(\mathrm{U}, \mathrm{U}')$ the functional determinant or Jacobian of the two cubics U, U′, the theorem is that the discriminant contains as a factor the square of the Jacobian, or that we have

$$\text{disct.}\ (a\mathrm{U}'-a'\mathrm{U},\ldots)=\{\mathrm{J}(\mathrm{U}, \mathrm{U}')\}^2(\mathrm{A}, \mathrm{B}, \mathrm{C}, \mathrm{D}, \mathrm{E} \between x, y)^4.$$

For assuming this to be the case, then disregarding a mere numerical factor, we have

$$\mathrm{U}d\mathrm{U}'-\mathrm{U}'d\mathrm{U}=\mathrm{J}(\mathrm{U}, \mathrm{U}')(y\,dx-x\,dy),$$

and the two equations give

$$\frac{\mathrm{U}d\mathrm{U}'-\mathrm{U}'d\mathrm{U}}{\sqrt{\text{disct.}\ (a\mathrm{U}'-a'\mathrm{U},\ldots)}}=\frac{y\,dx-x\,dy}{\sqrt{(\mathrm{A}, \mathrm{B}, \mathrm{C}, \mathrm{D}, \mathrm{E} \between x, y)^4}},$$

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* Communicated by the Author.

whence writing z for $\mathrm{U}' : \mathrm{U}$, and putting y equal to unity, we have

$$\frac{dz}{\sqrt{\text{disct.}\,(az-a',..)}}=\frac{dx}{\sqrt{(\mathrm{A},\mathrm{B},\mathrm{C},\mathrm{D},\mathrm{E}\between x,1)^4}};$$

where disct. $(az-a',\ldots)$, or at full length,

$$\text{disct.}\,(az-a',\ bz-b',\ cz-c',\ dz-d'),$$

is a given quartic function of z,

$$=(\mathrm{a},\mathrm{b},\mathrm{c},\mathrm{d},\mathrm{e}\between z,1)^4$$

suppose, which proves the theorem of transformation.

The assumed subsidiary theorem may be thus proved: suppose that the parameter θ is determined so that the cubic

$$\mathrm{U}+\theta\mathrm{U}'$$

may have a square factor, the cubic may be written

$$(a+\theta a',\ b+\theta b'\ c+\theta c',\ d+\theta d'\between x,y)^3,$$

and the requisite condition is

$$\text{disct.}\,(a+\theta a',\ldots)=0.$$

There are consequently four roots; and calling these $\theta_1,\theta_2,\theta_3,\theta_4$, we have identically

$$\text{disct.}\,(a+\theta a',\ldots)=\mathrm{K}(\theta-\theta_1)(\theta-\theta_2)(\theta-\theta_3)(\theta-\theta_4),$$

or what is the same thing,

$$\text{disct.}(a\mathrm{U}'-a'\mathrm{U},..)=\mathrm{K}(\mathrm{U}+\theta_1\mathrm{U}')(\mathrm{U}+\theta_2\mathrm{U}')(\mathrm{U}+\theta_3\mathrm{U}')(\mathrm{U}+\theta_4\mathrm{U}').$$

Now any double factor of U or U′ (that is the linear factor which enters twice into U or U′) is a simple factor of $\mathrm{J}(\mathrm{U},\mathrm{U}')$, and we have $\mathrm{J}(\mathrm{U},\mathrm{U}')=\mathrm{J}(\mathrm{U},\mathrm{U}+\theta\mathrm{U}')$, and consequently

$$\mathrm{J}(\mathrm{U},\mathrm{U}')=\mathrm{J}(\mathrm{U},\mathrm{U}+\theta_1\mathrm{U}')=\&\text{c.};$$

hence the double factors of each of the expressions $\mathrm{U}+\theta_1\mathrm{U}'$, $\mathrm{U}+\theta_2\mathrm{U}'$, $\mathrm{U}+\theta_3\mathrm{U}'$, $\mathrm{U}+\theta_4\mathrm{U}'$ are simple factors of $\mathrm{J}(\mathrm{U},\mathrm{U}')$, or what is the same thing, $\mathrm{J}(\mathrm{U},\mathrm{U}')$ is the product of four linear factors, which are respectively double factors of the product

$$(\mathrm{U}+\theta_1\mathrm{U}')(\mathrm{U}+\theta_2\mathrm{U}')(\mathrm{U}+\theta_3\mathrm{U}')(\mathrm{U}+\theta_4\mathrm{U}'),$$

or this product contains the factor $\{\mathrm{J}(\mathrm{U},\mathrm{U}')\}^2$, which proves the theorem.

2 Stone Buildings, W.C.,
March 5, 1858.

XLVII. *On the Structure and Motion of Glaciers. By* JOHN TYNDALL, *F.R.S., Professor of Natural Philosophy, Royal Institution; and* THOMAS H. HUXLEY, *F.R.S., Fullerian Professor of Physiology, Royal Institution*[*].

§ 1.

IN a lecture given at the Royal Institution on the 6th of June, 1856, by Mr. Tyndall, certain views regarding the origin of slaty cleavage were brought forward, and afterwards reported in the 'Philosophical Magazine' for July. A short time subsequently the attention of the lecturer was drawn by Mr. Huxley to the observations of Professor J. D. Forbes on the veined or laminar structure of glacier ice, and the surmise was expressed, that the same explanation might apply to it as to slaty cleavage. On consulting the observations referred to, the probability of the surmise seemed apparent, and the result was a mutual arrangement to visit some of the Swiss glaciers, for the purpose of observing the structure of the ice. This arrangement was carried out, the field of observation comprising the glaciers of Grindelwald, the Aar, and the Rhone. After returning to England, the one in whose department it more immediately lay, followed up the inquiry, which gradually expanded, until at length it touched the main divisions of the problem of glacier structure and motion. An account of the experiments and observations, and our joint reflections on them, are embodied in the memoir now submitted to the Royal Society.

§ 2. *On the Viscous Theory of Glaciers.*

A glacier is a mass of ice which, connected at its upper extremity with the snow which fills vast mountain-basins, thrusts its lower extremity into the warm air which lies below the snow-line. The glacier moves. It yields in conformity with the sinuosities of its walls, and otherwise accommodates itself to the inequalities of the valley which it fills. It is not therefore surprising that the glacier should have been regarded as an ice-river by those who dwelt in its vicinity, or that this notion should have found a place in the speculations of writers upon the subject. The statements of M. Rendu in connexion with this point are particularly distinct:—"There are," he writes, "a multitude of facts which seem to necessitate the belief that the substance of glaciers enjoys a kind of ductility which permits it to remodel itself on the locality which it occupies, to become thin and narrow, and to elongate itself like a soft paste[†]." But this observer put forward his speculations with great caution, and often in the

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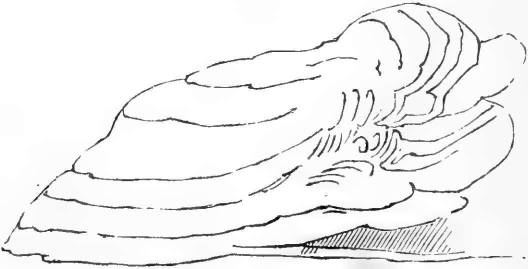
[*] From the Philosophical Transactions, Part II. for 1857; having been received and read by the Royal Society January 15, 1857.

[†] *Théorie des Glaciers de la Savoie*, p. 84.

form of questions which he confessed his inability to answer. "M. Rendu," says Professor Forbes, "has the candour not to treat his ingenious speculations as leading to any certain result, not being founded on experiments worthy of confidence. . . . My theory of glacial motion, then, is this:—*A glacier is an imperfect fluid or viscous body, which is urged down slopes of a certain inclination by the mutual pressure of its parts.*"

"The sort of consistency to which we refer," proceeds Professor Forbes, "may be illustrated by that of moderately thick mortar, or the contents of a tar-barrel poured into a sloping channel." Treacle and honey are also referred to as illustrative of the consistency of a glacier. The author of the theory endeavours, with much ability, to show that the notion of semifluidity as applied to ice, is not an absurdity, but on the contrary, that the motion of a glacier exactly resembles that of a viscous body. Like the latter, he urges, it accommodates itself to the twistings of valleys, and moves through narrow gorges. Like a viscous mass, it moves quickest at its centre, the body there being most free from the retarding influence of the lateral walls. He refers to the "Dirt-Bands" upon the surface of the glacier, and shows that they resemble what would be formed on the surface of a sluggish river. In short, the analogies are put forth so clearly, so ably, and so persistently, that it is not surprising that this theory stands at present without a competitor. The phænomena, indeed, are really such as to render it difficult to abstain from forming some such opinion as to their cause. The resemblance of many glaciers to "a pail of thickish mortar poured out;" the gradual changing of a straight line transverse to the glacier into a curve, in consequence of the swifter motion of the centre; the bent grooves upon the surface; the disposition of the dirt, the contortions of the ice, a specimen of which, as sketched near the Heisseplatte upon the Lower Grindelwald glacier, is given in fig. 1, and of which other striking examples

Fig. 1.



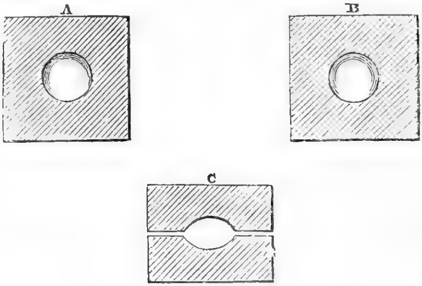
have been adduced by M. Escher in proof of the plasticity of the substance,—are all calculated to establish the conviction, that the mass must be either viscous, *or endowed with some other property mechanically equivalent to viscosity.* The question then occurs, is the viscosity real or apparent? Does any property equivalent to viscosity exist, in virtue of which ice can move and mould itself in the manner indicated, and which is still in harmony with our experience of the non-viscous character of the substance? If such a property can be shown to exist, the choice will rest between a quality which ice is *proved* to possess, and one which, in opposition to general experience, it is assumed to possess, in accounting for a series of phænomena which either the real or the hypothetical property might be sufficient to produce. In the next section, the existence of a true cause will be pointed out, which reconciles the properties of ice, exhibited even by hand specimens, with the apparent evidences of viscosity already referred to, and which, though it has been overlooked hitherto, must play a part of the highest importance in the phænomena of the glacier world.

§ 3. *On the Regelation of Ice, and its application to Glacial Phænomena.*

In a lecture given by Mr. Faraday at the Royal Institution on the 7th of June, 1850, and briefly reported in the 'Athenæum' and 'Literary Gazette' for the same month, it was shown that when two pieces of ice, at 32° F., with moistened surfaces, were placed in contact, they became cemented together by the freezing of the film of water between them. When the ice was below 32°, and therefore dry, no adhesion took place between the pieces. Mr. Faraday referred, in illustration of this point, to the well-known experiment of making a snowball. In frosty weather the dry particles of ice will scarcely cohere, but when the snow is in a thawing condition, it may be squeezed into a hard compact mass. On one of the warmest days of last July, when the thermometer stood at upwards of 80° F. in the shade, and above 100° in the sun, a pile of ice-blocks was observed by one of us in a shop window, and he thought it interesting to examine whether the pieces were united at their places of contact. Laying hold of the topmost block, the whole heap, consisting of several large lumps, was lifted bodily out of its vessel. Even at this high temperature the pieces were frozen together at the places of contact, though the ice all round these places had been melted away, leaving the lumps in some cases united by slender cylinders of the substance. A similar experiment may be made in water as hot as the hands can bear; two pieces of ice will freeze together, and sometimes continue so frozen in the hot water, until, as in the case above mentioned, the melting of the ice around the points of contact leaves the pieces united by slender columns of the substance.

Acquainted with these facts, the thought arose of examining how far, in virtue of the property referred to, the *form* of ice could be changed without final prejudice to its continuity. It was supposed that though crushed by great pressure, new attachments would be formed by the cementing, through regelation, of the severed surfaces; and that a resemblance to an effect due to viscosity might be produced. To test this conjecture the following experiments were made:—Two pieces of seasoned boxwood, A and B, fig. 2, 4 inches square and 2 deep, had two cavities hollowed out, so that when one was placed upon the other, a lenticular space, shown in section at C, was enclosed between them. A *sphere* of compact, transparent ice, of a volume rather more than sufficient to fill the cavity, was placed between the pieces of wood, and subjected to the pressure of a small hydraulic press. The ice broke, as was expected, but it soon reattached itself; the pressure was continued, and in a few seconds *the sphere was reduced to a transparent lens of the shape and size of the mould in which it had been formed.*

Fig. 2.



This lens was placed in a cylindrical cavity, 2 inches wide and ½ an inch deep, hollowed out in a piece of boxwood, C, fig. 3, as before; a *flat* plate, D, of the wood being placed over the lens, it was submitted to pressure. The lens broke as the sphere did, but the fragments attached themselves in accordance with their new conditions, *and in less than half a minute the mass was taken from the mould a transparent cake of ice.*

The substance was subjected to a still severer test. A hemispherical cavity was hollowed out in a block of boxwood, and a protuberant hemisphere was turned upon a second slab of the wood, so that, when the protuberance and the cavity were concentric, a distance of a quarter of an inch separated the convex surface of the former from the concave surface of the latter. Fig. 4 shows the arrangement in section. The pins of brass, *ab*, fixed in the slab AB, and entering suitable apertures in the mould CD, served to keep the two surfaces concentric. A lump of clear ice was placed in the cavity, the protuberance was brought down upon it, and the mould submitted to hydraulic pressure. After a short interval it was taken from the press, and when the upper slab was removed, a smooth concave surface of ice was exposed. By tapping the conical plug *p*, this ice was lifted from the cavity, *the lump having been converted by pressure into a hard transparent cup of ice.*

Fig. 3.

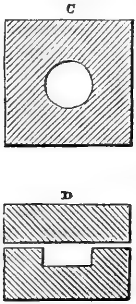
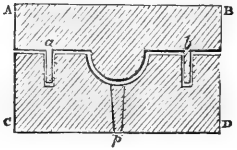
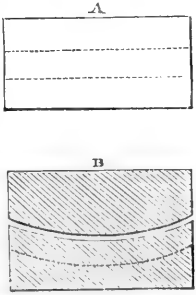


Fig. 4.



The application of the results here obtained to the "viscous flow" of glaciers will perhaps be facilitated by the following additional experiments.

Fig. 5.



A block of boxwood (A, fig. 5), 4 inches long, 3 wide and 3 deep, had its upper surface slightly curved, and a longitudinal groove (shown in dots in the figure), an inch wide and an inch deep, worked into it. A slab of the wood was prepared, the under surface of which was that of a convex cylinder, curved to the same degree as the concave surface of the former piece. The arrangement is shown in section at B. A straight prism of clear ice, 4 inches in length, an inch wide, and a little more than an inch in depth, was placed in the groove, and the upper slab of boxwood was placed upon it. The mould was submitted to hydraulic pressure, as in the former cases; the prism broke as a matter of course, but the quantity of ice being rather more than sufficient to fill the groove, and hence projecting above its edge, the pressure brought the fragments together and re-established the continuity of the ice. After a few seconds it was taken from the mould, bent as if it had been a plastic mass. Three other moulds similar to the last, but of augmenting curvature, were afterwards made use of, the same prism being passed through all of them in succession. *At the conclusion of the experiments the prism came out, bent to a transparent semi-ring of solid ice.*

In this way, by the proper application of force, all the bendings and contortions observed in glacier ice, and adduced in proof of its viscosity, can be accurately imitated. Any observer, seeing a straight bar of ice converted into a continuous semi-ring without being aware of the quality referred to, and having his attention fixed on the changes of external form alone, would be naturally led to the conclusion that the substance is viscous. But it is plainly not viscosity, properly so called, which enables it to change its shape in this way, but a property which has hitherto been entirely overlooked by writers upon glaciers.

It has been established by observation, that a vertical layer of ice originally plane, and perpendicular to the axis of a glacier, becomes bent, because the motion of its ends is retarded in comparison with that of its centre. This is the fact upon which the viscous theory principally rests.

In the experiments with the straight prism of ice, four successive moulds, gradually augmenting in curvature, were made use of. In passing suddenly from the shape of one to that of the other, the ice was fractured, but the pressure brought the separated surfaces again into contact and caused them to freeze together, thus restoring the continuity of the mass. The fracture was in every case both audible and tangible; it could be heard and it could be felt. A series of cracks occurred in succession as the different parts of the ice-prism gave way, and towards the conclusion of the experiment, the crackling in some instances melted into an almost musical tone. But if instead of causing the change to take place by such wide steps as those indicated; if instead of four moulds, forty, or four hundred were made use of; or better still, suppose a single mould to have the power of gradually changing its curvature from a straight line to a semicircle under the hydraulic press; the change in the curvature of the ice would closely approximate to that of a truly plastic or viscous body. This represents the state of things in a glacier. A transverse plate of ice, situated between the mass in front of it and the mass behind, is virtually squeezed in a press of the description which has been just imagined. The curvature of the ice-mould *does* change in the manner indicated, and so slowly, that the bending closely resembles what would take place if the substance were viscous. The gradual nature of the change of curvature may be inferred from an experiment made by Professor Forbes on an apparently compact portion of the Mer de Glace. He divided a distance of 90 feet transverse to the axis of the glacier into spaces of 2 feet each, and observed with a theodolite the gradual passage of this straight line into a bent one. The row of pins bent gradually so as to form a curve convex towards the lower extremity of the glacier; their deviations from a perfect curve were slight and irregular, nor was any great dislocation to be observed throughout their whole extent. After six days the summit of the curve formed by the forty-five pins was 1 inch in advance of the straight chord which united its two ends. It is not surprising if, with this extremely gradual change, the motion should have appeared to be the result of viscosity. It may, however, be remarked, that the slight and irregular variations to which Professor Forbes alludes, and which are such as would occur if the motion were such as we suppose it to be, are likely to throw much light upon the problem. It is also extremely probable that the motion, if effected in the manner referred to, will be sometimes accompanied by an audible crackling of the mass. To this we paid but little attention when on the ground; for the significance of this as well as of many other points was first suggested by the experiments made after our return. It is, however, we believe, a phænomenon of common occurrence. Professor Forbes calls the glacier a "crackling mass;" he speaks of the ice "cracking and straining forwards;" and in that concluding passage of his 'Travels' which has excited such general admiration, he says of the glacier, "it yields groaning to its fate." Other observers make use of similar expressions. M. Desor also speaks of the sudden change of the colour of the blue veins of the ice where a portion of the central moraine near the Abschwung is cleared away; the observation is very remarkable. "Au moment," says M. Desor, "où on la met à découvert, la glace des bandes bleues est parfaitement transparente, l'œil y plonge jusqu'à une profondeur de plusieurs pieds, mais cette pureté ne dure qu'un instant, et l'on voit bientôt se former des petites fêlures d'abord superficielles, qui se combinent en réseau de manière à enlever peu à peu à la glace bleue toute sa transparence. Ces fêlures propagent également dans les bandes blanches, et lorsqu'on approche l'oreille de la surface de la glace, on entend distinctement un *léger bruit de crépitation* qui les accompagne au moment de leur formation." These facts appear to be totally at variance with the idea of viscosity.

In a chapter on the "Appearance of the larger Glaciers," in an interesting little work by M. Mousson of Zurich, for which one of us has to thank the kindness of Professor Clausius, the phænomena which they exhibit are thus described*:—"The appearance of a large glacier of the first order has been compared, not without reason, with that of a high-swelled and suddenly solidified stream. It winds itself in a similar manner through the curving of the valley, is deflected by obstacles, contracts its width or spreads itself out. . . . . In short, the form is modified in the most complete manner to suit the character and iregularities of its bed. To this capacity to change its form, the ice of glaciers unites another property, which reminds us of the fluid condition; namely, the capability of joining and blending with other ice. Thus we see separate glacier-branches perfectly uniting themselves to a single trunk; regenerated glaciers formed from crushed fragments; fissures and chasms closed up, and other similar appearances. These phænomena evidently point to a slow movement of the particles of which the glacier consists, strange as the application of such an idea to a solid brittle mass such as glacier ice may appear to be. The solution of this enigma constitutes one of the most difficult points in the explanation of glaciers.

When the appearances here enumerated are considered with reference to the experiments on the regelation of ice above described, the enigma referred to by the writer appears to have received a satisfactory solution. The glacial valley is a mould through which the ice is pressed by its own gravity, and to which it will accommodate itself, while preserving its general continuity, as the hand specimens do to the moulds made use of in the experiments. Two glacial branches unite to form a single trunk, by the regelation of their pressed surfaces of junction. Crevasses are cemented for the same reason, and the broken ice of a cascade is reconstituted, as a heap of fragments under pressure become consolidated to a single mass. To those who occupy themselves with the external conditions merely of a glacier, it may appear of little consequence whether the flexures exhibited by the ice be the result of viscosity or of the principle demonstrated by the experiments above described. But the natural philosopher, whose vocation it is to inquire into the inner mechanism concerned in the production of the phænomena, will discern in the yielding of a glacier a case of simulated fluidity hitherto unexplained, and perhaps without a parallel in nature.

§ 4. *On the Veined Structure of Glacier Ice.*

This structure has been indifferently called the "veined structure," the "banded structure," the "ribboned structure," and the "laminar structure" of glacier ice. In a communication to the

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* *Die Gletscher des Jetztzeit*, by Albert Mousson. Zurich, 1854.

Geological Society of France assembled at Porentruy in September 1838, M. Guyot gave the following interesting description of the phænomenon:—"Since the word layer has escaped me, I cannot help recording as a subject of investigation for future observers a fact, regarding which I dare not hazard an explanation; especially as I have not encountered it more than once. It was at the summit of the Gries, at a height of about 7500 feet, a little below the line of the first or high *nevé*, where the ice passes into a state of granular snow..... In ascending to the origin of this latter (the glacier of Bettelmatten), for the purpose of examining the formation and direction of the great transverse fissures, I saw under my feet the surface of the glacier entirely covered with regular furrows from 1 to 2 inches in width, hollowed in a half-snowy mass, and separated by protruding plates of an ice more hard and transparent. It was evident that the mass of the glacier was here composed of two sorts of ice, one that of the furrows, still snowy and more easily melted, the other that of the plates, more perfect, crystalline, glassy and resistant; and that it was to the unequal resistance which they presented to the action of the atmosphere that was due the hollowing of the furrows, and the protrusion of the harder plates. After having followed them for several hundred yards, I reached the edge of a great fissure, 20 or 30 feet wide; which cutting the plates and furrows perpendicularly to their direction, and exposing the interior of the glacier to a depth of 30 or 40 feet, permitted the structure to be observed on a beautiful transverse section. As far down as my vision could reach I saw the mass of the glacier composed of a multitude of layers of snowy ice, each two separated by one of the plates of ice of which I have spoken, and forming a whole regularly laminated in the manner of certain calcareous slates."

A description of this structure, as observed upon the glacier of the Aar, was communicated by Professor Forbes to the Royal Society of Edinburgh on the 6th of December 1841, and published in the Edinburgh New Philosophical Journal for 1842*. He was undoubtedly the first to give the phænomenon a theoretic significance.

While engaged in the Lower Grindelwald glacier, we separated plates of ice perpendicular to the lamination of the glacier. The appearance presented on looking through them, was that sketched in fig. 6. The layers of transparent ice seemed imbedded in a general milky mass; through the former the light reached the eyes, while it was intercepted by the latter. Some of the transparent portions were sharply defined, and exhibited elongated oval sections, resembling that of a double convex lens, and

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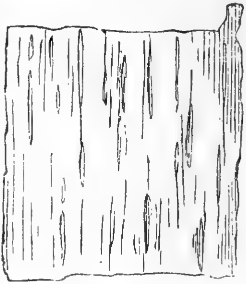
* This communication gave rise to a discussion as to priority between Professor Forbes and M. Agassiz, for the details of which we must refer to the original papers on the subject.

Fig. 6.



we therefore called this disposition of the veins "*the lenticular structure.*" In other cases, however, the sharpness of outline did not exist, but still the tendency to the lenticular form could be discerned, the veins in some cases terminating in washy streaks of blue. This structure is probably the same as that observed by Professor Forbes on the Glacier des Bossons, and described in the following words:—"The veins and bands . . . . are not formed in this glacier by a simple alternation of parallel layers, but the icy bands have all the appearance of posterior infiltration, occasioned by fissures, *thinning off both ways**.

In 1842 Professor Forbes undertook the survey and examination of the Mer de Glace, and finally arrived at a theory of glacier lamination, which both in his 'Travels' and in a series of letters, extending over a period of several years, he has expounded and illustrated with great skill. The theory is summed up in the following words:—"The whole phænomena in the case of any of the semifluids I have mentioned (treacle, tar, &c.), are such as, combined with the evidence which I have given, that the motion of a glacier is actually such as I have described that of a viscid fluid to be, can leave, I think no reasonable doubt, *that the crevices formed by the forced separation of a half rigid mass, whose parts are compelled to move with different velocities, becoming infiltrated with water, and frozen during winter, produce the bands which we have described*†."

This theory has been opposed by Mr. Hopkins, whose excellent papers, published in the 26th volume of the Philosophical Magazine, are replete with instruction as to the mechanical conditions of glaciers. On the other hand, the theory of Professor Forbes is defended in the same Journal by Dr. Whewell‡. We will leave the points discussed in their communications for the present untouched, and confine ourselves to stating a few of the circumstances which appear to us to render the theory doubtful.

1. It is not certain that the colds of winter penetrate to depths

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* Travels, p. 181.

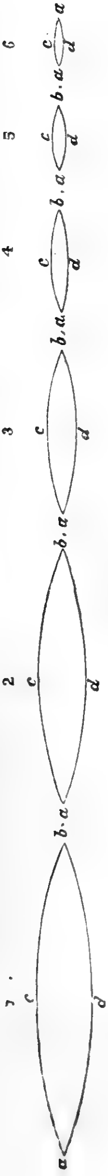
† Ibid. p. 377. M. Agassiz also seems disposed to regard the blue bands as the result of the freezing up of fissures, which, however, are supposed to be formed in a manner different from that assumed by Professor Forbes. But M. Agassiz calls the attention of future observers to some of the related phænomena; and gives it as his opinion, "qu'il n'est aucune phénomène dont l'explication offre plus de difficultés." See his important work, *Système Glacière,* which, until quite recently, we had not the opportunity of examining.

‡ Phil. Mag. S. 3. vol. xxvi. pp. 171, 217.

sufficient to produce the blue veins, which, it is affirmed, are "an integral part of the inmost structure" of the ice. Saussure was of opinion that the frosts of winter did not penetrate to a greater depth than 10 feet, even at the summit of Mont Blanc, and Professor Forbes considers this opinion to be a just one. But if so, there would be some difficulty in referring to the frosts of winter the blue veins which M. Agassiz observed at a depth of 120 feet below the surface of the glacier of the Aar.

2. It will be remembered that M. Guyot's statement regarding the blue veins is, that he saw the mass of the glacier composed of a multitude of layers of white ice, separated, each from the other, by a plate of transparent ice. The description of Professor Forbes is briefly this:—"Laminæ or thin plates of transparent blue ice, alternate in most parts of every glacier with laminæ of ice, not less hard and perfect, but filled with countless air-bubbles which give it a frothy semitransparent look." But there is another form of the blue veins, already referred to, which consists in transparent lenticular masses imbedded in the general substance of the white ice. Horizontal sections of these transparent lenses were exposed upon the surface of the Grindelwald glacier, and vertical sections of them upon the perpendicular sides of the water-courses, and upon the walls of the crevasses. The following measurements, taken on the spot, will give an idea of their varying dimensions. Such masses as these here figured were distributed in considerable numbers

Fig. 7.



Dimensions.

	in.	in.		in.	in.
No. 1.	*ab* 24	*cd* 2	No. 4.	*ab* $3\frac{1}{2}$	*cd* $\frac{1}{2}$
2.	*ab* 10	*cd* 1	5.	*ab* $1\frac{3}{4}$	*cd* $\frac{1}{4}$
3.	*ab* 6	*cd* 1	6.	*ab* 1	*cd* $\frac{1}{12}$

through the glacier; they had all the appearance of flattened cakes, and the smaller ones resembled the elongated green spots exhibited by sections of ordinary roofing-slate cut perpendicular to the planes of cleavage. Now it appears mechanically impossible that a solution of continuity, such as that supposed, could take the form of the detached lenticular spaces above figured.

3. The fissures to which the blue veins owe their existence are stated to be due to the motion of the glacier; and as this motion takes place both in summer and winter, it is to be inferred that the fissures are produced at both seasons of the year. Now as the fissures formed in winter cannot be filled with ice during that season for want of *water,* and as those formed in the ensuing summer cannot, while summer continues, be frozen for want of *cold,* we ought at the end of each summer to have *a whole year's fissures* in the ice. These fissures, which the ensuing winter is, according to the theory, to fill with blue ice, must in summer be filled with *blue water. Why then are they not seen in summer?* The fissures are such as can produce plates of ice varying "from a small fraction of an inch to several inches in thickness," which, according to our own observations, produce lenticular masses of ice 2 feet long and 2 inches thick, or even (for we have seen pieces of this description) 10 feet long and 10 inches thick; and M. Desor informs us in the memoir from which we have already quoted, that under the medial moraine of the Aar glacier, there are bands 10 inches and even a foot in thickness. Such fissures could not escape observation if they existed; but they never have been observed, and hence the theory which makes their preexistence necessary to the production of the blue veins appears to us improbable.

§ 5. *On the Relation of Slaty Cleavage to the Veined Structure.*

Within the last few years a mechanical theory of the cleavage of slate rocks has been gradually gaining ground among those who have reflected upon the subject. The observations of the late Daniel Sharpe appear to have originated this theory. He found that fossils contained in slate rocks were distorted in a manner which proved that they had suffered compression in a direction at right angles to the planes of cleavage. His specimens of shells, which are preserved in the Museum of Practical Geology, and other compressed fossils in the same collection, illustrate in a remarkable manner his important observations. The subsequent microscopic observations of Mr. Sorby, carried out with so much skill and patience, show convincingly that the effects of compression may be traced to the minutest constituents of the rocks in which cleavage is developed. More recently, Professor Haughton has endeavoured to give numerical accuracy to this theory, by computing, from the amount of the distortion of fossils, the magnitude of the change which cleaved rocks have undergone*. By the united testimony of these and other observers, whose researches have been carried out in different places, the association of cleavage and compression has been established in the most unequivocal manner; and hence the question naturally arises, "Is the pressure sufficient to produce the cleavage?" Sharpe appears to have despaired of an experimental answer to this question. "If," says he, "to this conclusion it should be objected, that no similar results can be produced by experiment, I reply, that we have never tried the experiment with a power at all to be compared with that employed; and that this may be one of the many cases where our attempts to imitate the operations of nature fail, owing to the feebleness of our means, and the shortness of the period during which we can employ them." The same opinion appears to have been entertained by Professor Forbes:—"The experiment," he says, "is one which the boldest philosopher would be puzzled to repeat in his laboratory; it probably requires acres for its scope, and years for its accomplishment."

While one of us was engaged in 1855 in examining the influence of pressure upon magnetism, he was fortunate enough to discover that in white wax, and other bodies, a cleavage of surpassing fineness may be developed by pressure, and he afterwards endeavoured, in a short paper†, to show the application of this result, both to slaty cleavage and to a number of other apparently unrelated phænomena. The theory propounded in this paper may be thus briefly stated. If a piece of clay, wax, marble or iron be broken, the surface of fracture will not be a plane surface, nor will it be a surface dependent only on the form of the body, and the strain to which it has been subjected; the fracture will be composed of innumerable indentations, or small facets, each of which marks a surface of weak cohesion. The body has yielded, where it could yield, most easily, and in exposing these facets, in some cases crystalline, in others purely mechanical, wherever the mass is broken, it is shown to be composed of an aggregate of irregularly shaped parts, which are separated from each other by surfaces of weak cohesion. Such a quality must, in an eminent degree, have been possessed by the mud of which slate-rocks are composed, after the water with which the mud had at first been saturated had drained away; and the result of the application of pressure to such a mass would be, to develope in it a lamination similar to that so perfectly produced on a small scale in white wax. Thus one cause of cleavage may

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* Philosophical Magazine for December, 1856.

† Ibid. for July, 1856.

be stated, in general terms, to be the conversion by pressure of irregularly-formed surfaces of weak cohesion into parallel planes. To produce lamination in a compact body such as wax, it is manifest, that while it yields to the compression in one direction, it must have an opportunity of expanding in a direction at right angles to that in which the pressure is exerted; a second cause is the lateral sliding of the particles which thus takes place, and which may be very influential in producing the cleavage*.

Before attempting to show the connexion between this theory and the case at present under consideration, a mode of experiment may be described which was found to assist in forming a conception of the mechanical conditions of a glacier, and which has already been resorted to by Professor Forbes in demonstration of the viscous theory. Owing to the property of ice described in § 3, the resemblance between the motion of a substance like mud and that of a glacier is so great, that considerable insight regarding the deportment of the latter may be derived from a study of the former. From the manner in which mud yields when subjected to mechanical strain, we may infer the manner in which ice would be *solicited to yield* under the same circumstances.

To represent then the principal accidents of a glacial valley, a wooden trough, A B C D, fig. 8, of varying width and inclination, was made use of. From A to C the trough measures 6 feet, and from A to B, 15 inches. It is divided into five segments; that between A B and *e f* is level, or nearly so, that between *e f* and *g h* is inclined; from *g h* to *i k* is again nearly level; from *i k* to *m n* inclined, while from *m n* to CD the inclination is less than between *i k* and *m n*. The section of the bot-

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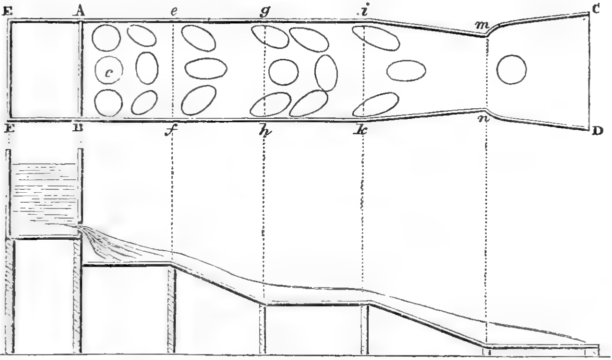
* Three principal causes may operate in producing cleavage:—1st, the reducing of surfaces of weak cohesion to parallel planes; 2nd, the flattening of minute cavities; and 3rd, the weakening of cohesion by tangential action. The third action is exemplified by the state of the rails near a station where the break is applied. In this case, while the weight of the train presses vertically, its motion tends to cause longitudinal sliding of the particles of the rail. Tangential action does not however necessarily imply a force of the latter kind. When a solid cylinder, an inch in height, is squeezed by vertical pressure to a cake a quarter of an inch in height, it is impossible, physically speaking, that the particles situated in the same vertical line shall move laterally with the same velocity; but if they do not, the cohesion between them will be weakened or ruptured. The pressure will produce new contact, and if the new contact have a cohesive value equal to that of the old, no cleavage from this cause can arise. The relative capacities of different substances for cleavage, appears to depend in a great measure upon their different properties in this respect. In butter, for example, the new attachments are equal, or nearly so, to the old, and the cleavage is consequently indistinct; in wax this does not appear to be the case, and hence may arise in a great degree the perfection of its cleavage. The further examination of this subject promises interesting results.

tom of the trough is figured underneath the plan. A B E F is a box supported at the end of the trough, and filled with a mixture of water and fine pipe-clay. The front, A B, can be raised, like a sluice, and the mud permitted to flow regularly into the trough. While the mud is in slow motion, a coloured circle, *c*, is stamped upon the white clay between A B and *e f*; the changes of shape which this circle undergoes in its passage downwards will indicate the forces acting upon it. The circle first moves on, being rather compressed, in the direction of the length of the trough until it reaches *e f*, on crossing which, and passing down the subsequent slope, it elongates as in the figure. Between *g h* and *i k* the figure passes through the circular form, and assumes that of an ellipse whose shorter axis is parallel to the length of the trough. It is manifest from this that the mud between *e f* and *g h* is in a state of longitudinal tension, while between *g h* and *i k* its state is that of longitudinal compression. On crossing *i k* and descending the second incline, the figure is again drawn out longitudinally, while between *m n* and C D the ellipse widens on account of the permission given to lateral expansion by the augmented width of the trough.

Fig. 8.



The side circles in the same figure will enable us to study the influence of lateral friction upon the descending stream. These circles are distorted into ellipses, whose major axes are oblique to the direction of the trough's length. Above the line *e f* central fissures perpendicular to the axis of the trough cannot be formed; for here, instead of tending to open into fissures, the flattening of the central circle shows that the mud is longitudinally compressed. On the slope below *e f*, the distortion of the circles into ellipses is very pronounced; and as the longer axis of each ellipse marks the line of maximum tension, and as the tendency of the mass is to form a fissure at right angles to such a line, we should have here, if the substance were not so plastic as to prevent the formation of fissures, the state of things observed upon the corresponding portion of the glacier; namely, central fissures perpendicular to the longitudinal axis of the trough, and side fissures inclined to the same axis because pointing in the direction of the shorter axis of each ellipse. Between *g h* and *i k* the longitudinal tension is changed to compression; the central figure is flattened, while the side ones remain stretched. In the corresponding portion of the glacier we should expect the central fissures formed between *e f* and *g h* to be squeezed together and closed up, while the lateral ones would remain open. This is also the case*. Between *i k* and *m n* we have again longitudinal tension, and at the corresponding portions of the glacier the transverse central crevasses ought to reappear, which they actually do. Below the line corresponding to *m n*, the widening of the valley, in the case now in our recollection, causes the ridges produced at the previous slope to break across and form prismatic blocks; while lower down the valley these prisms are converted by the action of sun and rain into shining minarets of ice. These results appear to be in perfect accordance with those arrived at by Mr. Hopkins on strict mechanical reasoning†.

We will now seek to show the analogy of slaty cleavage to the laminar structure of glacier ice. Referring to fig. 8, it will be seen that in the distortion of the side circles one diameter is elongated to form the transverse axis of the ellipse, while another is compressed to form the conjugate axis. In a substance like mud, as the elongation of the major axis continues, its inclination to the axis of the glacier continually changes; but were the

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* The possibility of the coexistence of lateral crevasses and compression at the centre may, perhaps, be thus rendered manifest:—Let *ab*, *cd*, be two linear elements of a glacier, situated near its side S I.

Suppose, on passing downward, the line *ab* becomes shortened by longitudinal pressure to *a'b'*, and *cd* to *c'd'*, which latter has passed *a'b'* on account of its greater distance from the side of the glacier. Taking the figure to represent the true change both of dimension and position, it is plain, that though each element has been *compressed*, the differential motion has been such as to *distend* the line of particles joining *a* and *d*, in the ratio $\frac{ad}{a'd'}$. If this ratio be more than that which the extensibility of ice can permit of, a side fissure will be formed.

† Phil. Mag. 1845, vol. xxvi.

substance one of limited extensibility like ice, fissures would be formed when the tension had reached a sufficient amount, or in other words, when the major axis of the ellipse had assumed a definite inclination to the axis of the glacier.

Thus, in a glacier of the form represented by our trough, owing to the swifter motion of the centre, we have a line of maximum pressure oblique to the wall of the glacier, and a line of maximum tension perpendicular to the former; crevasses are formed at right angles to the direction of tension, and *it is approximately at right angles to the direction of pressure, as in the case of slate rocks, that the lamination of glacier ice is developed.*

Under ordinary circumstances, therefore, the lamination near the sides of the glacier would, in accordance with the theory of compression, be oblique to the sides, which it actually is. It would be transverse to the crevasses wherever they occur, which it actually is. If the bed of a glacier at any place be so inclined as to cause its central portions to be longitudinally compressed, the lamination, if due to compression, ought to be carried across the glacier at such a place, being transverse to the axis of the glacier at its centre, which is actually the case. This relation of the planes of lamination to the direction of pressure is constant under a great variety of conditions. A local obstacle which produces a thrust and compression is also instrumental in developing the veined structure. In short, so far as our observations reach, wherever the necessary pressure comes into play, the veined structure is developed; being always approximately at right angles to the direction in which the pressure is exerted.

But we will not rely in the present instance upon our own observations alone. Before he formed any theory of the structure, and in his first letter upon the subject, Professor Forbes remarks, that "the whole phænomenon has a good deal the air of a structure induced *perpendicularly to the lines of greatest pressure.*" His later testimony is in substance the same. In his thirteenth letter, read before the Royal Society of Edinburgh on the 2nd of December, 1846, he says that the blue veins are formed *where the pressure is most intense.* In his reference to the development of the laminar structure on the glacier of the Brenva, the pressure is described as being "*violent,*" the effect being such as to produce "*a true cleavage when the ice is broken with a hammer or cut with an axe.*" So also with regard to the glacier of Allalein*, he says "the veined structure is especially developed in front, *i. e.* against the opposing side of the valley, where the pressure is greater than laterally." In fact, the parallelism of the phænomenon to that of slaty cleavage struck Prof. Forbes himself, as is evident from the use of the term "now" in

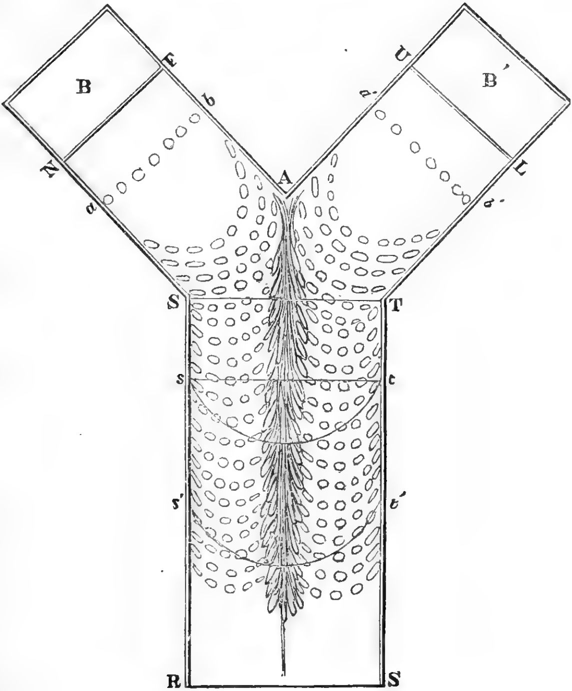
* Travels, p. 352.

the following passage:—"It will be understood that I do not *now* suppose that there is any parallelism between the phænomenon of rocky cleavage and the ribboned structure of the ice." This reads like the giving up of a previously held opinion; the term *now* being printed in italics by Professor Forbes himself. The adoption of the viscous theory appears to have carried the renunciation of this idea in its train.

Later still, and from a source wholly independent of the former, we have received additional testimony on the point in question. The following quotation is from a letter, dated 16th November, 1856, received by one of us from Professor Clausius of Zurich, so well known in this country through his important memoirs on the Mechanical Theory of Heat:—"I must now," writes M. Clausius, "describe to you another singular coincidence. I had read your paper upon the cleavage of rocks .... and it occurred to me at the time that the blue veins of glaciers, which indeed I had not seen, but which had been the subject of repeated conversations between Professor Studer of Berne, Professor Escher von der Linth, and myself, might be explained in the same manner. When, therefore, I reached the Rhone glacier for the first time, I walked along it for a considerable extent, and directed my attention particularly to the structure. I repeated this on the other glaciers which I visited during my excursion. I did not indeed pursue the subject so far into detail as to be able in all cases to deduce the blue veins from the existing conditions of pressure, but the correctness of the general explanation impressed itself upon me more and more. This was particularly the case in the glacier of the Rhone, where I saw the blue bands most distinctly, and where also their position harmonized with the pressure endured by the glacier when it was forced to change the direction of its motion. You can therefore imagine how astonished I was to learn that at the same time, and on this very glacier among others, you had been making the same investigations." It ought also to be remarked, that a similar thought occurred to Mr. Sorby, from whom after his return from Switzerland one of us received a note, in which pressure was referred to as the possible cause of the veined structure of glacier ice.

A fine example of ice lamination is that produced by the mutual thrust of two confluent glaciers. The junction of the Lauter Aar and Finster Aar glaciers to form the glaciers of the Unter Aar is a case in point, and the results obtained with a model of this glacier were highly interesting. Fig. 9 is a sketch of the trough in which the experiments were made. The branch terminating at UL is meant to represent the Lauter Aar glacier; that ending at FN the Finster Aar branch. The point at A represents the "Abschwung," so often referred to in the works of M. Agassiz. B and B′ are two boxes with sluice fronts, from which the mud flows into the trough. The object was to observe

Fig. 9.



the mechanical state of the mass along the line of junction of the two streams, and along their respective centres, and compare the result with the observations upon the glacier itself. The mud was first permitted to flow simultaneously from both boxes, and after it had covered the bottom of the trough to some distance below the line ST, the end of a glass tube was dipped into a fine mixture of the red oxide of iron and water, and the two arms of the glacier were covered all over with small circles similar to those between the points *ab* and *a′b′*. The mud was then permitted to flow, and the mechanical strains exerted on it were inferred from the distortion of the small circles. The figure represents the result of the experiment. The straight rows of circles bent in the first place into curves; at the point A both streams met, and by their mutual push actually squeezed the circles into lines. Along this central portion in the glacier itself the great medial moraine stands, and under it and beside it, as already stated, the lamination is most strikingly developed; the blue veins being parallel to the axis of the glacier, or, in other words, coinciding with the direction of the central moraine. Midway between the moraine and the sides of the glacier the structure is very imperfectly developed, and the deportment of our model, which shows that the circles here scarcely change their form, tells us that this is the result which ought to be expected. It may be urged, that the structure is here developed, because of the sliding motion produced by the swifter flow of one of the glaciers; but some of the experiments with the model were so arranged, that both of the branch streams flowed with the same velocity; the distortions, however, were such as are shown in the figure. The case is precisely the same in nature. On reference to the map of M. Agazziz, we find a straight line set out across the Unter Aar glacier bent in three successive years into a curve; but on the central moraine, which marks the common limit of the constituent streams, we find no breach in the continuity of the curve, which must be the case if one glacier slid past the other.

§ 6. *On the "Dirt-bands" of Glaciers.*

Wherever the veined structure of a glacier is highly developed, the surface of the ice, owing to the action of the weather, is grooved in accordance with the lamination underneath. These grooves are sometimes as fine as if drawn by a pencil, and bear in many instances a striking resemblance to those produced by the passage of a rake over a gravelled surface. In the furrows of the ice the smaller particles of dirt principally rest; and the direction of the furrows, which always corresponds with that of the blue veins, is thus rendered so manifest, that a practised observer can at any moment pronounce upon the direction of the lamination from the mere inspection of the surface of a glacier. But besides these narrow grooves, larger patches of discoloration are sometimes observed, which take the form of curves sufficient in width to cover hundreds or thousands of the smaller ones. To an eye placed at a sufficient height above a glacier on which they exist, their general arrangement and direction are distinctly visible. To these Professor Forbes has given the name of "Dirt-bands;" and the discovery of them, leading as it did to his theories of glacial motion, and of the veined structure of glacial ice, is to be regarded as one of the most important of his observations.

On the evening of the 24th of July he walked up the hill of

Charmoz to a height of about 1000 feet above the level of the glacier, and favoured by the peculiar light of the hour, observed "a series of nearly hyperbolic brownish bands on the glacier, the curves pointing downwards, and the two branches mingling indiscriminately with the moraines." The cause of these bands was the next point to be considered, and his examination of them satisfied him "that the particles of earth and sand and disintegrated rock, which the winds and avalanches and water-runs spread over the entire breadth of the ice, formed a *lodgement* in those portions of the glacier where the ice was most porous, and that, consequently, the 'dirt-bands' were merely *indices of a peculiarly porous veined structure traversing the mass of the glacier in these directions*."

Professor Forbes was afterwards led to regard these intervals as the marks of the annual growth of the glacier; he called the dirt-bands "annual rings*," and calculated, from their distance apart, the yearly rate of movement. In fine, the conclusion which he deduces from the dirt-bands is, that a glacier throughout its entire length is formed of alternate segments of porous and of hard ice. The dirt which falls upon the latter is washed away, as it has no hold upon the surface; that which falls upon the former remains, because the porous mass underneath gives it a lodgement. "The cause of the dazzling whiteness of the glacier des Bossons at Chamouni is the comparative absence of these layers of granular and compact ice: the whole is nearly of uniform consistence, the particles of rock scarcely find a lodgement, and the whole is washed clean by every shower†." "It must be owned, however," says Professor Forbes, "that there are several difficulties which require to be removed, as to the recurrence of these porous beds." In his fifteenth letter upon glaciers, and in reference to some interesting observations of Mr. Milward's, he endeavoured to account for the difference of structure by referring it to an annual "gush" of the ice, which is produced by the difference of action in summer and winter. We are ignorant of the nature of the experiments on which this theory of the dirt-bands is founded, and would offer the following simple explanation of those which came under our own observation.

Standing at a point which commanded a view of the Rhone glacier, both above and below the cascade, we observed that the extensive ice-field above was discoloured by sand and *débris*, distributed without regularity. At the summit of the ice-fall the

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* "I cannot help thinking that they are the *true annual rings* of the glacier, which mark its age like those of a tree."—Appendix to Travels, p. 408.

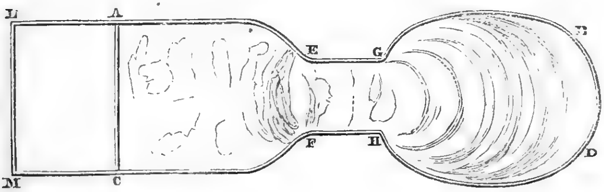
† Travels, p. 406.

valley narrows to a gorge, and the slope downwards is for some distance precipitous. In descending, the ice is greatly shattered; in fact, the glacier is broken repeatedly at the summit of the declivity, transverse chasms being thus formed; and these, as the ice descends, are broken up into confused ridges and peaks, with intervening spaces, where the mass is ground to pieces. By this breaking up of the glacier the dirt upon its surface undergoes fresh distribution: instead of being spread uniformly over the slope, spaces are observed quite free from dirt, and other spaces covered with it, but there is no appearance of regularity in this distribution. At some places large irregular patches appear, and at others elongated spaces covered with dirt. Towards the bottom of the cascade the aspect changes; but still, were the eye not instructed by what it sees lower down, the change would have no significance. When the ice has fairly escaped from the gorge, and has liberty to expand laterally in the valley below, the patches of dirt are squeezed by the push behind them, and drawn laterally into narrow stripes, which run across the glacier; and as the central portion moves more quickly than the sides, these stripes of discoloration form curves which turn their convexity downwards, constituting, we suppose, the "Dirt-bands" of Professor Forbes. On the Grindelwald glacier, where one of us, in his examination of the bands, was accompanied by Dr. Hooker, this change in the distribution of the dirt,—the squeezing, lateral drawing act, and bending of the dirt patches below the bottom of the ice-fall,—was especially striking.

Such, then, appears to be the explanation of the dirt-bands in the cases where we have had an opportunity of observing them. We have not seen those described by Professor Forbes, but the conditions under which he has observed them appear to be similar. An illustration of the explanation just given is furnished by the dirt-bands observed below the "cascade" of the Talèfre. The character of this ice-fall may be inferred from the following words of Professor Forbes, and from the map which accompanies his 'Travels.' "The structure," he says, "assumed by the ice of the Talèfre is extirpated wholly by its precipitous descent to the level of the Glacier de Léchand, where it reappears, or rather is reconstructed out of the broken fragments according to a wholly different scheme." One of the results of this "scheme" would, it is presumed, be a redistribution of the dirt, and the formation of bands in the manner described. Those who consult the map will, however, see dirt-bands marked on the Glacier du Géant also, while no cascade is sketched upon it; but at page 167 of the 'Travels,' Professor Forbes, in referring to this glacier, says, "I am not able to state the exact number of dirt-bands between *the foot of the ice cascade* opposite La Noire and the corner of Trelaporte." Here we are not only informed of the existence of a cascade, but are left to infer that the dirt-bands begin to form at its base, as in the Glacier du Géant, and in those which have come under our own observation. The clean Glacier des Bossons, also, which was referred to by Prof. Forbes, in one of his earliest letters, as affording no lodgement to the dirt, possesses its cascade (page 181); and here also we find (page 182) "that the peculiar phænomena of '*dirt-bands*' on a great scale are not wanting, although from the dazzing whiteness of the ice they may be very easily overlooked." We make these remarks with due reserve, not having yet seen the glaciers referred to.

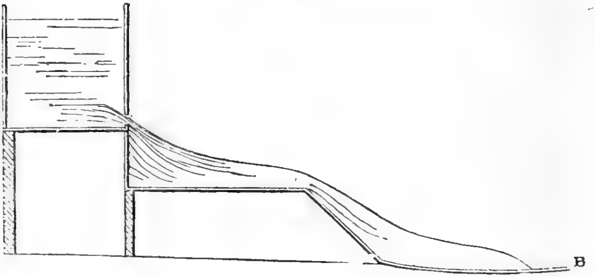
The explanation just given has been brought to the test of experiment. A B C D, fig. 10, is a wooden trough intended roughly to represent the glacier of the Rhone, the space A C E F being meant for the upper basin. Between E F and G H the trough narrows and represents the precipitous gorge down which the ice tumbles, while the wide space below represents the comparatively level valley below the fall, which is filled with the ice, and constitutes the portion of the glacier seen by travellers descending from the Grimsel or the Furka pass. A C L M is a box with a sluice front, which can be raised so that the fine mud within the box shall flow regularly into the trough, as in the cases already described. The disposition of the trough will be manifest from the section, fig. 11. While the mud was in slow motion downwards, a quantity of dark-coloured sand was sifted over the space A C E F, so as to represent the *débris* irregularly scattered over the corresponding surface of the glacier; during the passage of the mud over the brow at E F, and down the subsequent slope, it was hacked irregularly, so as to represent the dislocation of the ice in the glacier. Along the slope this hacking produced an irregular and confused distribution of the sand; but lower down, the patches of dirt and the clean spaces between them gradually assumed grace and symmetry; they were squeezed

Fig. 10.



together longitudinally and drawn out laterally, bending with the convexity downwards in consequence of the speedier flow of the central portions, until finally a system of bands was established which appeared to be an exact miniature of those exhibited by the glacier. On fig. 10 is a sketch of the bands observed upon the surface of the mud, which, however, falls short of the beauty and symmetry of the original. These experiments have been varied in many ways, with the same general result.

Fig. 11.



In conclusion we would remark, that our joint observations upon the glaciers of Switzerland extended over a period of a few days only. Guided by the experience of our predecessors, much was seen even in this brief period; but many points of interest first suggested themselves during the subsequent experimental investigation. While, therefore, expressing our trust that the substance of the foregoing memoir will be found in accordance with future observation, we would also express our belief in the necessity of such observation. Indeed the very introduction of the principle of regelation, without which it may be doubted whether the existence of a glacier would be at all possible, opens, in itself, a new field of investigation. This and other questions introduced in the foregoing pages, must, however, be discussed with strict reference to the phænomena as Nature presents them. Much might be said even now upon these subjects, but the known liability of the human mind to error when speculation is substituted for observation, renders it safer to wait for more exact knowledge, than to hazard opinions which an imperfect acquaintance with the facts must necessarily render to some extent uncertain.

Royal Institution, January 1857.

XLVIII. *On certain Researches of* Euler. *By* James Cockle, *M.A., F.R.A.S., F.C.P.S., Barrister-at-Law*[*].

The formulæ employed by Euler at p. 97 (art. 45) of his paper "De Reductione," &c. in vol. ix. of the Petersburgh *Novi Commentarii*[†], will be found to give the results

$$\mathrm{PQ}=\left(\frac{m}{n}\right)^2 r^4k^3g^6, \quad \mathrm{RS}=g^3k^6,$$

so that, substituting, as we may without loss of generality, 1 for v in art. 36 (p. 92), we are led to

$$\mathfrak{A}\mathfrak{D}=\frac{g^2}{k}\sqrt[5]{\left(\frac{mr^2}{n}\right)^2}, \quad \mathfrak{B}\mathfrak{C}=\frac{k^2}{g}.$$

But (p. 97, art. 44) $\mathrm{A}=5\left(\frac{g^2}{k}+\frac{k^2}{g}\right)$, therefore

$$\mathfrak{A}\mathfrak{D}=\frac{g^2}{k}, \quad \sqrt[5]{\left(\frac{mr^2}{n}\right)^2}=1\,;$$

and, A being either finite or zero, g and k, PQ and RS, $\mathfrak{A}\mathfrak{D}$ and $\mathfrak{B}\mathfrak{C}$ vanish, if at all, simultaneously.

It follows that Euler's later form (arts. 44 to 46), although a generalization of his earlier one (arts. 39 to 42), does not include De Moivre's, nor, *à fortiori*, the result at which I have arrived[‡], viz. that

$$y^5-5\mathrm{P}y^3-5\mathrm{Q}y^2-5\mathrm{R}y+\mathrm{E}=0$$

admits of finite algebraic solution when the symmetric product

$$\begin{aligned}
&\mathrm{P(E^2-7Q^2S)(QSE+Q^4+S^3)+(11P^4S+13P^2S^2+S^3)^2}\\
&\quad\mathrm{+(QSE+Q^4+S^3)^2+\{(2Q)^3-(5P)(2Q)(3S)\}Q(PS)^2}\\
&\quad\mathrm{+\{(PQ)^2+PS^2+(2Q)^2S\}(PE)^2+137(PS-Q^2)P^4Q^2S}\\
&\quad\mathrm{+11P^3\{11P^3Q-(2Q)^3\}SE+\{7(5P^2S)^2+P^2(3S)^3\}QE}\\
&\quad\mathrm{+17P^3\{Q^4+(3P)(2Q)P^2Q-(11P^3)(3S)P\}Q^2-S^6+(11P^4Q)^2P}\\
&\quad\mathrm{PQE\{(5P)(2Q^2)P^3+(3P)(2Q^2)^2\}+\{(5P)(2Q)(3S)^2\}P^4Q,}
\end{aligned}$$

or $\pi\div 5^{14}$, vanishes.

It should be observed that π is an element, not only of a new criterion of solvability, but of the algebraic roots, if indeed any exist, of a general quintic.

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[*] Communicated by the Author.

[†] Pro annis 1762 et 1763, Petropoli 1764, pp. 70–98. When either g, k, m, n, or r vanish, Euler's T (at. p. 97) becomes a square.

[‡] Phil. Mag. May 1857, p. 354; Quarterly Journal of Pure and Applied Mathematics, May 1857, p. 144; Lady's and Gentleman's Diary for 1858, p. 77.

Further, the evanescence of a linear factor of the symmetric product affords a test of the solvability of at least one class of equations higher than quintics. For, if the roots of

$$y^6+2By^4+2Cy^3+Dy^2+2Ey+F=0 \quad . \quad . \quad . \quad (1)$$

satisfy the relation

$$y_1-y_2+y_3-y_4+y_5-y_6=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

then (1) is equivalent to

$$(y^3+cy+e)(y^3+dy+f)=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where

$$\left.\begin{matrix} c \\ d \end{matrix}\right\} = B \pm \sqrt{B^2-D}, \quad \left.\begin{matrix} e \\ f \end{matrix}\right\} = C \pm \sqrt{C^2-F},$$

and the equation (2) indicates that

$$(E-BC)^2-(B^2-D)(C^2-F)=0, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

the left of which is the symmetric product.

Again, the decomposition* of a linear factor may be applied with effect to an equation below a quintic. Let

$$\zeta=x+\alpha y+\alpha^2 z=\zeta'-\xi\zeta''+(\xi^2+\alpha\xi+\alpha^2)z,$$

where α is an unreal cube root of unity, and

$$\zeta'=x+(\alpha+\xi)y, \quad \zeta''=y+(\alpha+\xi)z\,;$$

then if $\xi=\alpha^2$, we find that

$$\zeta=\zeta'-\alpha^2\zeta''=(x-y)-\alpha^2(y-z)\,;$$

and that when ζ' and ζ'' vanish simultaneously, the three roots of a cubic are equal.

76 Cambridge Terrace, Hyde Park,
March 24, 1858.

XLIX. *Notices respecting New Books.*

Investigations in the Theory of Reflected Ray-surfaces, and their relation to Plane Reflected Caustics. By the Rev. G. F. Childe, *M.A., Mathematical Professor in the South African College, Cape of Good Hope.* J. C. Juta, Cape Town.

THIS is a work of much merit. The investigations which it contains are strictly confined to the consideration of ray-surfaces generated when the reflector is situated in one plane, and capable of reflecting rays about the normals coincident with the plane or per-

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* Phil. Mag. May 1857, p. 360, Section III.

pendicular to it. But, although Mr. Childe has thus limited the subject, he nevertheless introduces so many illustrative propositions of a novel and interesting kind, that it cannot fail to attract the attention of all who desire to see the higher methods of analysis skilfully applied to the solution of optical questions. The polar equations investigated in the Appendix, although they are unusually simple and easy of application, afford the means of resolving inverse problems by integration, and lead to some useful differential equations. In discussing the theory of plane reflected caustics, Mr. Childe has, very properly, omitted the consideration, as special examples, of the cycloid, circle, logarithmic spiral and other well-known curves. The examples which he adduces have, therefore, with some few exceptions, a marked character of novelty.

One of the most interesting illustrations in the book is that with reference to the rings of the planet Saturn. After showing that if rays of light from a luminous spheroid are reflected from a circular ring symmetrically placed with respect to the luminous surface, the envelope of the reflected ray-surfaces will be two right cones of which the axes pass through the centre of the ring perpendicular to its plane, he says:—

"In the complex system of the planet Saturn with his attendant rings there seems to be presented to us a physical illustration of the preceding investigation. If, for the sake of illustration, we should conceive the planetary rings to be composed of a congeries of circular rings of infinitesimal thickness, concentric with the planet, and capable of reflecting light, a certain portion of each component ring will reflect all the incident rays which emanate from a definable surface of the planet. Each luminous point in this surface will generate a ray-surface containing all the rays which, issuing from that point, have been reflected from any one of the elementary rings; and the intersection of all such ray-surfaces, reflected from that elementary ring, will be in the conical envelope which we have here considered. Now if we conceive these concentric reflecting circles to be multiplied within the known limits, so as to generate the satellite rings of Saturn, the ray-surfaces reflected from each component ring will be multiplied in like manner, generating an infinite number of conical surfaces of light. The absolute thickness of any section in the stream of light (so to speak) which is thus thrown off from the rings, is a function of the distance between the interior limit of the inner ring and the exterior limit of the outer ring, an interval of the known magnitude 59079 miles; and it can hardly be doubted, that in this manner an important element in the optical œconomy of the planet is provided. There will be in this system three separate classes of ray-surfaces generated from the rings. Considering the rings to be circular, the rays incident directly from the sun will be reflected from each elementary circle in a cylindrical surface determined by the equations in page 17, chapter 1. Again, from the same elementary circle, the rays which are incident upon it after reflexion from the planet will generate a conical surface, of which the general equation is that obtained in page 65. In addition to these ray-surfaces, there are also to be considered those which are due to all the rays reflected from the lateral surface of the interior ring; consisting, in certain positions, of light incident directly from the sun, but principally of rays which are incident after a first reflexion at the surface of the planet. In the former case, the direct solar rays will pass off in ray-surfaces defined by the equation

$$(\xi^2+\eta^2-m^2\zeta^2)^2=r^2\{(\xi-m\zeta)^2+\eta^2\};$$

assuming the origin at the centre of the planet, and the plane of $\xi\zeta$, at any moment to pass through the centre of the sun. The rays which are incident after reflexion from the planet, will be situated in surfaces of a similar character given by the general equation. The surfaces which are thus generated from the interior parts of the ring, all pass into the less enlightened atmosphere, and must contribute in some degree to its illumination. In virtue of these ray-surfaces, we may conceive the darkness of those regions to be relieved by a thin luminous equatorial cincture reposing on the bosom of the sky, which, while it may bear a faint and shadowy resemblance to the bright arch which sheds its lustre over the more favoured hemisphere, shines with a chastened light, subdued to a character of far less resplendent magnificence and beauty.

"From the preceding investigations it appears that the nature of the envelope is intrinsically the same, whether the luminous surface is supposed to be a sphere or a spheroid. In order to illustrate the formulæ, the planet may be assumed to be spherical; and, using the elements given by Sir J. Herschel in the 'Outlines of Astronomy,' page 315, we find, for the interior limiting circle of the inner ring, $\sqrt{r^2-k^2}=43308$ miles, $\frac{\sqrt{r^2-k^2}}{k}=1\cdot0942$; and for the exterior limiting circle of the outer ring, $\sqrt{r^2-k^2}=78831$ miles,

$$\frac{\sqrt{r^2-k^2}}{k}=1\cdot9917;$$

from these data the limiting envelope cones may be readily delineated.

"The fainter illumination which belongs to the external ring has been attributed to the presence of a dense atmosphere; it is possible that this effect may be also due, in part, to the greater distance of this ring from the planet, in consequence of which, light of diminished intensity is reflected in the ray-surfaces; although it is to be remembered, that the same cause will slightly augment the illumination arising from the direct rays of the sun."—Pp. 75–77.

We trust that the important question of refracted ray-surfaces and caustics will not escape Mr. Childe's attention. It would form an admirable subject for another volume.

L. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 317.]

June 18, 1857.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"Researches on the Phosphorus-Bases." By Dr. Hofmann and A. Cahours.

In a note on the action of chloride of methyle upon phosphide of calcium, communicated more than ten years ago to the Institute of France, M. Paul Thenard pointed out the existence of a series of bodies which correspond to the compounds of phosphorus with hydrogen, which may, in fact, be viewed as phosphoretted hydrogens, the hydrogen of which is replaced by an equivalent quantity of methyle.

It is now many years since M. Paul Thenard abandoned the study of the phosphorus-compounds, for the first knowledge of which we are indebted to him. The unfinished state in which these researches remained, and the rich and abundant harvest collected since that period, in all the neighbouring fields of science, necessitated a revision of the subject. The discovery of methylamine, dimethylamine, and trimethylamine, and of the corresponding terms in the ethyle- and amyle-series, had shown that the hydrogen in ammonia may be replaced by binary molecules, such as methyle, ethyle, amyle and phenyle, the newly-formed compounds retaining the basic character of the original ammonia molecule; whilst the production of triethylestibine and triethylarsine had furnished the proof that the total replacement of the hydrogen in the indifferent antimonietted and arsenietted hydrogens exalts the chemical character of these compounds in a most remarkable manner, the methylated and ethylated bodies exhibiting basic characters scarcely inferior to those of ammonia itself. It remained, therefore, to be investigated whether phosphorus, which by its chemical tendencies stands between nitrogen and arsenic, would exhibit a similar deportment. It had to be ascertained in what manner the gradual entrance of binary molecules in the place of the hydrogen in phosphoretted hydrogen would change the character of the original compound. Again, the discovery of the tetrethylated ammonium-bases had opened a new field of research, in which the corresponding terms of the antimony- and arsenic-series were rapidly brought to light. It was indeed possible to predict with certainty, that an appropriately selected method would lead to the production of the analogous derivatives of phosphoretted hydrogen. The time for resuming the study of the phosphorus-bases had in fact arrived.

We have been engaged for a considerable time in the investigation of this subject, and now beg to offer to the Royal Society in the following pages a short abstract of our results.

In the first place, we have endeavoured to obtain the bases corresponding to phosphoretted hydrogen, by a method analogous to that followed by M. Paul Thenard, simply substituting for the chloride of methyle the iodide, and for the phosphide of calcium the more energetic phosphide of sodium. After many unsuccessful experiments, however, we were compelled to abandon this plan, which is quite uncertain, unproductive, and highly dangerous.

The question then resolved itself into the discovery of a method which would yield us the desired substances conveniently, without danger, in considerable quantity, and in a state of absolute purity.

It appeared that the action of terchloride of phosphorus on zinc-methyle, zinc-ethyle, &c., the remarkable substances discovered by Dr. Frankland some years ago, would enable us to attain the contemplated result. Experiment has fully confirmed this anticipation.

The bases $Me_3 P$ and $E_3 P$, the products of this reaction, which we propose to call respectively *trimethylphosphine* and *triethylphosphine*, remain united with chloride of zinc, and a simple dilution with an alkali is all that is necessary to liberate them.

$$3ZnCl, Me_3 P + 6KO = 3KCl + 3KO, ZnO + Me_3 P$$
$$3ZnCl, E_3 P + 6KO = 3KCl + 3KO, ZnO + E_3 P$$

They are obtained in this way as volatile oils, of a peculiar and strongly marked odour, and possessing distinctly basic properties.

We limit ourselves in this abstract to giving a synoptical table of the several compounds which have been the subject of our researches, the preparation and properties of which, together with the analytical details, are fully given in the complete paper which accompanies this note.

Methyle-series.

Trimethylphosphine	$Me_3 P$
Platinochloride of trimethylphosphine	$Me_3 P, HCl, PtCl_2$
Binoxide of trimethylphosphine	$Me_3 P O_2$
Bisulphide of ditto	$Me_3 P S_2$
Biselenide of ditto	$Me_3 P Se_2$
Iodide of phosphomethylium	$Me_4 P I$
Platinochloride of ditto	$Me_4 P Cl, PtCl_2$
Aurochloride of ditto	$Me_4 P Cl, AuCl_3$
Iodide of phosph'ethyltrimethylium	$(Me_3 E) P I$
Platinochloride of ditto	$(Me_3 E) P Cl, PtCl_2$
Iodide of phosph'amyltrimethylium	$(Me_3 Ayl) P I$
Platinochloride of ditto	$(Me_3 Ayl) P Cl, PCl_2$

Ethyle-series.

Triethylphosphine	$E_3 P$
Platinochloride of triethylphosphine	$E_3 P, HCl, PtCl_2$
Binoxide of ditto	$E_3 P O_2$
Bisulphide of triethylphosphine	$E_3 PS_2$
Biselenide of ditto	$E_3 P Se_2$
Iodide of phosphethylium	$E_4 P I$
Platinochloride of ditto	$E_4 P Cl, PtCl_2$
Aurochloride of ditto	$E_4 PCl, AuCl_3$
Iodide of phosphomethyl-triethylium	$(MeE_3) P I$
Platinochloride of ditto	$(MeE_3) PCl, PtCl_2$
Iodide of phosph'amyltriethylium	$(Ayl E_3) P I$
Platinochloride of ditto	$(Ayl E_3) PCl, PtCl_2$

On glancing at the phosphorus-compounds noticed in the preceding abstract, a comparison of these substances with the corresponding terms of the nitrogen-, arsenic-, and antimony-series is unavoidably forced upon us. Whether we consider the composition, or whether we review the properties of these groups, the most striking analogies, indeed an almost perfect parallelism, cannot be mistaken; similar formulæ, similar modes of combination, similar decompositions.

This analogy is particularly manifest in the compounds belonging to the *ammonium-type.* In these remarkable bodies, nitrogen, phosphorus, arsenic, and antimony appear to play absolutely the same part. It is more especially in the oxides of these compound metals that the analogy of composition induces a perfect identity of properties, and, indeed, of very salient properties, which may be traced in almost every direction. If we were satisfied with the study of the reactions of these bodies, we should never suspect, in compounds possessing such a close similarity of properties, the presence of elements so dissimilar as nitrogen, phosphorus, arsenic, and antimony; they might, moreover, be confounded with potassa and soda, by which they are scarcely surpassed in alkaline power.

Only the deportment of the hydrated oxides, under the influence of heat, distinguishes the derivatives of nitrogen from the corresponding terms of the phosphorus-, arsenic-, and antimony-series.

If we regard, on the other hand, the compounds belonging to the *ammonia-type,* we observe that the electro-positive character of the substances gradually rises in intensity from the nitrogen- to the antimony-compounds. Thus, trimethylamine and triethylamine are not capable of uniting with oxygen, chlorine, bromine, and iodine; a power which the corresponding terms of the phosphorus-, arsenic-, and antimony-series possess in a high degree.

Triethylamine unites with the acids, producing compounds of the ammonium-type, and containing

$$E_3N, HCl$$
$$E_3N, HSO_4$$
$$E_3N, HNO_6$$

The corresponding compounds in the arsenic- and antimony-series do not exist; at all events chemists have not yet succeeded in preparing them. Triethylarsine and triethylstibine combine only directly with oxygen, chlorine, sulphur, &c., producing saline bodies which have the composition respectively,—

$$E_3AsO_2 \;\ldots\ldots\ldots\ldots\; E_3SbO_2$$
$$E_3AsCl_2 \;\ldots\ldots\ldots\ldots\; E_3SbCl_2$$
$$E_3AsS_2 \;\ldots\ldots\ldots\ldots\; E_3SbS_2$$

In the phosphorus-series, lastly, the two classes are represented. Triethylphosphine not only forms compounds analogous to the salts of triethylamine, but also the terms corresponding to the binoxides of triethylarsine and triethylstibine. We have in the first place the terms

$$E_3P, HCl$$
$$E_3P, HSO_4$$
$$E_3P, HNO_6,$$

and in the second place compounds of the formula

$$E_3\,PO_2$$
$$E_3\,PCl_2$$
$$E_3\,PS_2$$

The phosphorus-compounds accordingly hold a position intermediate between the nitrogen-compounds on the one hand, and the arsenic- and antimony-series on the other. It cannot, however, be denied that the phosphorus-bodies exhibit rather a tendency towards the arsenic- and antimony-series, than towards the nitrogen-group.

This cannot surprise us, when we consider the close analogies which phosphorus and arsenic present in many other directions. Both phosphorus and arsenic form well-characterized polybasic acids; the acids of antimony are not yet sufficiently investigated, but the acids of nitrogen, which are better examined, are all found to be monobasic. The equivalent numbers, too, of phosphorus, arsenic, and antimony, present a remarkable connexion, the difference between those of phosphorus and arsenic, and those of arsenic and antimony being virtually the same—

Phosphorus	31	difference 44,
Arsenic	75	
Antimony	120	difference 45,

whilst the equivalent of nitrogen stands altogether apart from the rest.

The same relative position of the elements nitrogen, phosphorus, arsenic, and antimony, may also be traced in their hydrides,

$$H_3N \quad H_3P \quad H_3As \quad H_3Sb.$$

Ammonia is a powerful alkali; phosphoretted hydrogen unites only with hydrobromic and hydriodic acids, whilst in arsenietted and antimonietted hydrogen, the power of combining with acids has altogether disappeared. In these hydrogen-compounds the gradation of properties is indeed even more marked than in their trimethylated and triethylated derivatives. On comparing the terminal points of the series, ammonia and antimonietted hydrogen, we cannot fail to be struck by the dissimilarity of properties which at the first glance appears to limit the analogy of the two compounds to a mere parallelism of composition.

In the methylated and ethylated derivatives of these compounds, the intensity of the chemical tendencies in general is so much raised, that the gradation is no longer perceptible to the same extent.

"Report of Proceedings of the Astronomical Expedition to Teneriffe, in 1857." By Prof. C. Piazzi Smyth.

The Report, presented originally to the Admiralty, at whose expense and whose orders the Expedition was sent out, consists of ten parts, with their contents as follows:—

Part 1. Astronomical and Physical observations.
Part 2. Mountain Meteorological Journal.
Part 3. Reductions of above.

Part 4. Sea-level Meteorological Journal.

Part 5. Reductions of above.

Part 6. Plans, Sections, and Astronomical illustrations.

Parts 7, 8, 9. Condensed statement of results and conclusions in Astronomy, Physics, Meteorology, Geology, Botany, and Navigation.

Part 10. Photographs illustrating the botany and geology of three different levels in the Island.

The original instructions of Prof. Piazzi Smyth had been purely astronomical, and were to ascertain how much telescopic vision could be improved by eliminating the lower third of the atmosphere. In furtherance of this view, he erected the Sheepshanks equatorial of the Edinburgh Observatory on Mount Guajara in Teneriffe at a height of 8903 feet, and found the space-penetrating power extended from mag. 10 to mag. 14, and so great an improvement in definition, that a magnifying power of 240 could be used with more satisfaction on the mountain, than one of 60 in Edinburgh. After a month's experience of this station, he ascended to a higher one—the Alta Vista—at a height of 10,702 feet on the eastern slope of the Peak, and there erected the large equatorial of Cooke, lent for the occasion by Mr. Pattinson of Newcastle. The definition was here admirable, and the telescope equal, if not superior, to all the test objects it was turned upon. A comparative hypsometric estimate was not possible; for although the observer had spent an equal number of days to what he employed on the Peak, in trying to ascertain the capabilities of the instrument at the house of its hospitable owner, on several visits, the cloudy atmosphere of Newcastle had always prevented any good observations being made.

An argument in favour, however, of the increase of height was ascertained through means of radiation instruments, which indicated almost as much improvement between 10,700 and 8900 feet, as between the latter height and the sea-level. This unexpected degree is attributed by the author to the circumstance of the highest station being almost clear of certain dry, hazy, or dusty strata of atmosphere, which include and overtop the intermediate station, and are, next to the clouds themselves, the greatest obstacle to good telescopic vision of the heavenly bodies. The results with regard to the clouds were equally satisfactory, for $\frac{7}{10}$ths of the whole were found to be under the level of 5000 feet.

"On the Phenomenon of Relief of the Image formed on the Ground Glass of the Camera Obscura." By A. Claudet, Esq., F.R.S.

The author having observed that the image formed on the ground glass of the camera obscura appears as much in relief as the natural object when seen with the two eyes, has endeavoured to discover the cause of that phenomenon, and his experiments and researches have disclosed the singular and unexpected fact, that although only one image *seems* depicted on the ground glass, still each eye perceives a different image; that in reality there exist on the ground glass two images, the one visible only to the right eye, and the other visible only to the left eye. That the image seen by the right eye is the representation of the object refracted by the left side of the lens, and the image seen by the left eye is the representation of the object refracted by the right side of the lens. Consequently these two images presenting two different perspectives, the result is a stereoscopic perception, as when we look through the stereoscope at two images of different perspectives.

It appears that all the different images refracted separately by every part of the lens, are each only visible on the line of their refraction when it corresponds with the optic axis, so that while we examine the image on the ground glass, if we move the head we lose the perception of all the rays which are not corresponding with the optic axes, and have only the perception of those which, according to the position of the eyes, gradually happen to coincide with the optic axes. Consequently when we look on the ground glass perfectly in the middle, the two eyes being equally distant from the centre, the right eye sees only the rays refracted from the left of the lens, and the left eye only those refracted from the right of the lens.

If we move the head horizontally, as soon as we have deviated about 6° from the centre on the right or on the left, in the first position the right eye sees no image, and the left eye sees the image which before was seen by the right eye; in the second position the inverse takes place, and of course in both cases there cannot exist any stereoscopic illusion.

When we examine on the ground glass the image of a solid produced by the whole aperture of the lens, if we have taken the focus on the nearest point of the solid, we remark, in looking with the two eyes, that the image is stereoscopic, and as soon as we shut one eye the illusion of relief disappears instantly.

The stereoscopic effect is beautifully brought out by the image of a group of trees; and when experimenting in an operating room, it is rendered quite conspicuous if we take the image of an object having several planes very distinct, such as the *focimeter*, which the author has described in a former memoir (see Phil. Mag. for June 1851).

If without altering the focus we examine the same image with the pseudoscope, the effect is pseudoscopic. But if the focus has been set on the most distant plane of the focimeter, the effect is pseudoscopic, and it becomes stereoscopic in looking with the pseudoscope.

The image loses its relief when it is produced only by the centre of the lens. The stereoscopic and pseudoscopic effects are therefore as much less apparent as the aperture of the lens has been more reduced, and they are the more evident if the image is produced by two apertures on both extremities of the horizontal diameter of the lens. This mode of conducting the experiments presents the most decided manifestation of the whole phenomenon.

But it must be remarked, that if the image is received on a transparent paper instead of ground glass, it does not in any case present the least illusion of relief. The surface of the paper has the property of preserving to both eyes the same intensity of image from whatever direction the rays are refracted on that surface, and at whatever angle the eyes recede from the centre to examine the image. In fact, all the various images refracted through every part of the lens and coinciding on the surface of the paper, are visible at whatever angle they are examined.

The reason of this difference between the effect of the ground glass and that of the paper is, that through the surface of the ground glass, composed of innumerable molecules of the *greatest transparency*, only deprived of their original parallelism by the operation of grinding, but acting as *lenses* or *prisms* disposed at all kinds of angles, the rays refracted by the various parts of the lens continue their course in straight lines in passing through these transparent molecules, and are visible only when they coincide with the optic axes, being invisible in all other directions; that, in short, they are not stopped by the surface of the ground glass; while the paper being perfectly opaque, stops all the rays on their passage, by which the image of the object remains fixed on the surface. Each molecule of the paper becoming luminous, sends new rays in all directions; and from whatever direction we look on the paper, we always perceive at once all the images superposed, so that each eye seeing the two perspectives mingled, the process of convergence according to the horizontal distances of the same points of the various planes, cannot have its play, and no stereoscopic effect can take place, as is the case with the ground glass, which presents to each eye an image of a different perspective.

The author explains that he has ascertained these facts by several experiments, the most decisive of which consists in placing before one of the marginal openings of the lens a blue glass, and a yellow glass before the other. The object of these coloured glasses is to give on the ground glass two images, each of the colour of the glass through which it is refracted.

The result is two images, superposed on the ground glass, one yellow and the other blue, forming only one image of a grey tint, being the mixture of yellow and blue, when we look with the two eyes at an equal distance from the centre. But when shutting alternately, now the right eye and then the left eye, in the first case the image appears yellow, and in the second it appears blue.

If while looking with the two eyes (the opening on the right of the lens being covered with the yellow glass, and the opening on the left with the blue glass) we move the head on the right of about 6°, the mixture of the two colours disappears, and the image retains only the blue colour; on the other hand, if after having resumed the middle position, which shows again the mixture of the two colours, we move the head on the left of 6°, the mixture disappears again, and the image retains only the yellow colour.

This proves evidently that each eye sees only the rays which, when after having been refracted by any part of the lens, and continuing their course in a direct line through the ground glass, coincide with the optic axes, while all the other rays are invisible.

The consideration of these singular facts has led the author to think that it would be possible to construct a new stereoscope, in which the two eyes looking at a single image could see it in perfect relief, such a single image being composed of two images, of different perspectives superposed, one visible only to the right eye and the other to the left. This would be easily done by refracting a stereoscopic slide on a ground glass, through two semi-lenses separated enough to make the right picture of the slide coincide with the left picture at the focus of the semi-lenses. The whole arrangement may be easily understood; we have only to suppose that we look through a ground glass placed before an ordinary stereoscope at the distance of the focus of its semi-lenses, the slide being strongly lighted, and the eye seeing no other light than that of the picture on the ground glass. The whole being nothing more than a camera having had its lens cut in two parts, and the two halves sufficiently separated to produce at the focus the coincidence of the two opposite sides of the stereoscopic slide placed before the camera.

GEOLOGICAL SOCIETY.

[Continued from p. 326.]

March 10, 1858.—Prof. Phillips, President, in the Chair.

The following communications were read:—

1. "On the Geology of the Gold-fields of Victoria." By A. R. C. Selwyn, Esq., Geologist to the Colony of Victoria.

The author stated, that in the Colony of Victoria, from a line east of Melbourne to some distance west of that place, he has traced a succession of fossiliferous palæozoic rocks, commencing with schists, much cleaved and contorted, and containing *Lingulæ* and *Graptolites*, passing through a series of schists and sandstones with *Trilobites* and many other fossils characteristic of the lower, middle, and upper Silurian Series of Britain, and terminating with Devonian and Carboniferous rocks; and he remarks that the younger or Oolitic (?) coal-bearing beds on the west rest unconformably on the palæozoic rocks. A list of about sixty genera of Silurian fossils, including many new species, was appended.

The gold-bearing quartz-veins of the Silurian rocks appear to the author to be dependent more on their proximity to some granitic or other plutonic mass than on the age of the rocks in which they occur. Quartz-veins do not appear to traverse the Oolitic (?) coal-rocks, which are of newer date than the granites of this district.

The author's observations refer chiefly to Bendigo, Ballaarat, and Steiglitz gold-fields, where *Graptolites* and *Lingulæ* occur in the schists, which are traversed by the gold-quartz veins. The granites here do not contain gold; and, though they have altered the slate-rocks at the line of junction, yet they do not appear to have affected their general strike or dip, but appear to have themselves partaken of the movements which have placed these Silurian rocks on their present highly inclined and contorted positions, and given them their very uniform meridional direction.

Mr. Selwyn recognizes gold-bearing drifts of three distinct ages. The lowest contains large quantities of wood, seed-vessels, &c., at various depths, to 280 feet, and is associated with clays, sands, and pebbles. These are overlaid by sheets of lava. A more recent auriferous drift, containing also bones of both extinct and living marsupial quadrupeds, overlies these lavas in some places; in others it rests on the older drifts; and at Tower Hill, near Warnambool, marine or estuary beds of probably the same age are overlaid by volcanic ashes. A third and still more recent gold-drift is found on the surface, overlying indifferently any of the older deposits.

The gold is found at the base of these drifts or gravels, which are the result of the immediate waste, by atmospheric and fluviatile action, of older masses, and have not been far transported. The largest amount of gold is found in the drifts when near the Silurian schists. The author believes that there is every probability of gold-deposits existing under the greater portion of the lava-plains of the region to the westward.

Mr. Selwyn also described a cave which he had discovered in the basaltic lava of Mount Macedon, a few miles north of Melbourne, and in which he had found bones of many living species of mammals, including the "devil" of Tasmania, and the Dingo or native dog. The cave is about 1000 feet above the sea-level, and thirty miles inland.

2. "Notes on the Gold-field of Ballaarat, Victoria." By Mr. John Phillips, C.E., Surveyor in the Government Service of Victoria.

All the Victorian gold-fields are near granite, and some are on it. The granite at Ballaarat is fine and even-grained, and the schists lie against it. Between these rocks the junction is abrupt; there is little or no gneiss, and no porphyritic or other veins were observed. The schists are greenish, and are occasionally chloritic, micaceous, aluminous, and siliceous, and are traversed by quartz-veins, from less than an inch to one foot in thickness. The schists in the upper portion are more quartzose and contain oxides of iron; lower down they are more aluminous and contain pyrites. Their strike is rather uniform; nearly coinciding with the true meridian, while the cleavage and quartz-veins are not regular in strike. The workings at Ballaarat have exhibited a section of 300 feet in thickness, consisting of gravels, sands, clays, and trap-rocks. The oldest drift or gravel—a beach-like conglomerate—is found not in the deep section, but on the surface of the schist-country. It is regarded as of marine origin by the author, and is composed of quartz, and contains gold at its base. Another drift has been deposited in gullies cut through the oldest drift and deep into the schists. This also is auriferous, and is covered by an ancient humus, which, in the deep section, is found to contain stems of trees, and to be covered over by a trap-rock enclosing upright trees. This fossil wood is usually but little altered in its texture and ligneous qualities; its colour is changed from that of red birch to cocoa or lignum-vitæ. But some of it has passed into jet; and both the charred and the uncharred woods have much bright pyrites in them. The flora of this old land-surface resembles that of the present day.

This first trap is covered by green and brownish clay and sand, which are succeeded by another trap, having a line of charred vegetable matter at its base, and also having a similar covering of clay and sand. These clay and sand deposits are regarded by the author as being of lacustrine origin; the volcanic rocks having dammed up the old river-courses that formed the gully-drifts, and caused the drainage water of the region to be accumulated in lakes.

The next deposit is a coarse ochreous quartzose drift, considered by the author to be the effect of some sweeping deluge; and this is also overlaid by a third bed of trap-rock, with the charred remains of a forest intervening. This trap is covered by a mottled clay of pure quality, also regarded as lacustrine.

A fourth trap succeeds, covered by a superficial quartzose drift (of diluvial origin, according to the author), and lying on one side of the schistose hills, which are clearly denuded on the other.

In the basin of the Yarrowee, which is covered chiefly with this gravel, the author traces the run of the "gold-leads" or old gullies, which have only an approximative resemblance to the ramifications of the present river. These ancient gullies or leads had a very uniform fall, which, from the smallness of the contents of the gullies, must have been as rapid as 16 in 1000, while the fall of the present Yarrowee has only a fall of 8 in 1000.

Mr. Phillips urges that all the basin between the gold-leads may be wrought by the aid of the water power of the Yarrowee; a thousand horse-power being now allowed to run waste, which, by means of reservoirs, could be made available.

The author adds that silver-nuggets have been reported on good authority to have been found within thirty miles of Ballaarat. He further observes, that, whilst surveying the district, oscillations of the spirit-bubble indicated a rocking of the earth; and that the country in places sounds hollow, like a wooden bridge, horses even noticing it in passing.

3. "Notes on the Gold-diggings at Creswick Creek and Ballaarat." By Mr. W. Redaway.

Mr. Redaway noticed first the "bluestone" or concretionary basaltic lava at Creswick Creek, which composes also the rough bouldered surface of the country to a great extent. In the plains formed of this volcanic rock, small lakes or water-holes, from 3 to 12 feet in diameter, are in some places frequent.

At Creswick Creek the different diggings perforate varying thicknesses of the bluestone, from 17 to 20 feet. Under this is 30 feet of solid clay; then darkish-coloured quartzose gravel, with abundant remains of wood, to a depth of about 80 feet; and under this the "gutters," "leads," or "runs" of auriferous quartzose gravel—or "wash-dirt"—are met with on the surface of the slate or on pipe-clay. The pits vary considerably in the sections they afford.

The fragments of wood in the gravel are of all sizes, from tree-trunks, 3 or 4 feet in diameter, to branches and twigs; and this drift is throughout impregnated with woody particles, giving it a black appearance, especially towards the bottom. The cones of the "honeysuckle," or *Banksia*, have been found not unfrequently in this drift. These are very brittle, but the wood is often well preserved. Thin horizontal layers of very hard rock are imbedded in the gravel.

Some of the "gutters" or "leads" were traced by the author on plans showing their course beneath this drift across the present gullies and from hill to hill; especially the "Black Lead" and the "White Lead," underlying Little Hill, one of them having a branch from under Clarke's Hill, and both uniting before passing under Slaughter Yard Hill.

At Ballaarat, Mr. Redaway observed, in a pit on Sevastopol Hill, two layers of bluestone (the second bed about 80 feet thick) above the gold-drift or "wash-dirt," together with stiff clays and quartzose gravels. Here the author traced some gold-runs—the "Frenchman's Lead," "White Horse Lead," and "Terrible Lead," running parallel to each other in a direction transverse to that of the present gully, and from hill to hill. Like all other "leads," these rise generally in the neighbourhood of a quartz-vein (or "quartz-reef"), are shallow at first, 2 or 3 feet in depth, and gradually get deeper.

4. "Notes on some Outline-drawings and Photographs of the Skull of *Zygomaturus trilobus*, Macleay, from Australia." By Prof. Owen, F.R.S., F.G.S.

About a month since Prof. Owen received from Sir R. Murchison seven photographs, three of which are stereoscopic, of perhaps the most extraordinary Mammalian fossil yet discovered in Australia.

These photographs, with a brief printed notice of their subject by William Sharp Macleay, Esq., F.L.S., and some MS. notes by J. D. Macdonald, M.D., R.N., had been transmitted to Sir R. Murchison by His Excellency Governor Sir W. Denison, from Sydney, New South Wales; and by desire of Sir Roderick the Professor brought the subject under the notice of the Geological Society of London, to whom Sir Roderick desires to present the photographs on the part of His Excellency Sir W. Denison.

Professor Owen had some weeks previously received from George Bennett, Esq., F.L.S., of Sydney, outlines of the same fossil skull, made by him on the reception of the specimen by the authorities of the Australian Museum at that town; and the Professor had penned notes of his comparisons of these sketches before receiving the photographs and descriptions of the fossil skull from Sir R. I. Murchison.

This unique and extraordinary skull of a probably extinct Mammal, together with other bones, but without its lower jaw, were found at King's Creek, Darling Downs,—the same locality whence the entire skull and other remains of the *Diprotodon* have been obtained.

Mr. Macleay has described the fossil under notice as belonging to a marsupial animal, probably as large as an Ox, bearing a near approach to, but differing generically from, *Diprotodon*. He has named it *Zygomaturus trilobus*. The skull has transversely ridged molars, and a long process descending from the zygomatic arch, as in the *Megatherium* and *Diprotodon*, and exhibits an extraordinary width of the zygomatic arches. The skull at its broadest part, across the zygomata, is 15 inches wide, and is 18 inches long. In *Diprotodon* the skull is about 3 feet long by 1 foot 8 inches broad: so that while the latter must have had a face somewhat like that of the Kangaroo, the *Zygomaturus* more resembled the Wombat in the face and head.

Prof. Owen stated that, from the evidences afforded by the photographs, he finds the dentition of this upper jaw to consist of three incisors and five molars on each side, of which the first appears to be a premolar and the rest true molars, *i. e.* $i.\,\frac{3-3}{}$, $c.\,\frac{0-0}{}$, $p.\,\frac{1-1}{}$, $m.\,\frac{4-4}{}$; agreeing, in this formula, with *Macropus* and *Diprotodon*. The modifications of this dentition resemble those of the latter genus in the retention of the premolar, after the last true molar has come into its place, and in the superior size of the first, as compared with the second and third incisors. He then described in detail the sockets of the incisors, and the form and conditions of the molar teeth, which are highly characteristic of the marsupiality of this huge and most strange extinct quadruped. The cranial characters, which were next described, equally elucidate this affinity. The peculiar facial bones were then described in detail; that portion in advance of the orbits forming, as it were, a short pedunculate appendage to the rest of the skull, increasing in a remarkable manner in both vertical and lateral extent as it approaches the muzzle, but not offering any evidence of having borne a nasal horn, as thought to be probable by Mr. Macleay. The cavity of the nose is divided by a bony septum,—a character which Prof. Owen has lately found to exist also in a rare species of living Wombat—to a much greater extent than in other known marsupials. Wholly concurring in Mr. Macleay's conclusions as to the marsupial nature of the fossil in question, Prof. Owen does not think that it exhibits evidences of a generic distinction from *Diprotodon*. The Professor suggested, however, that probably the lower jaw, when found, may show some peculiarities of dentition and proportions similar to those on which he has founded the genus *Nototherium*.

LI. *Intelligence and Miscellaneous Articles.*

ON SONOROUS FLAMES.

To John P. Gassiot, Esq.

SINCE my last, I have made some pretty experiments on *sonorous flames*, of which, with other related matters, I am preparing a full account for Silliman's Journal. Among them are the following:—

1. When the flame is silent and cannot be made to sing by the external sound, I find that gently *vibrating the jet-pipe* causes it instantly to commence its musical performance. This occurs as well when the pipe does *not strike* the tube as when it does, and is the result simply of a more rapid mixing of the gas with the air, by which it is more certainly brought to the *explosive condition*.

2. When all other means of exciting the flame to sound have failed, we can at once cause the singing to begin by introducing *a current of air*, properly graduated, into the lower end of the tube. In many cases the *lightest breathing* suffices to awaken the sound.

3. By making the flame revolve rapidly in a circle within the tube, it assumes *when silent* the form, as might be expected, of a hollow cylinder with an even top, like the flame of an Argand burner; but the moment it begins to sing, this luminous cylinder becomes beautifully serrated above, and resolves itself into a series of vertical columns of flame with narrow dark intervals between, thus marking in a very striking way the intermitting nature of the explosive combustion, upon which the sound depends. With a tube 6 feet long and $1\frac{1}{2}$ inch in diameter, a flame of coal-gas $\frac{3}{4}$ of an inch long gives an effect which may be distinctly seen over a large room.

4. I find the simplest arrangement for showing the intermittence of the singing flame at a distance, is a *blackened wheel having on it a white bar* in a radial direction, which, when revolving near the sonorous flame, exhibits a *series* of white bars arranged symmetrically around the centre.

Yours truly,

Boston, U.S.
March 16, 1858.

W. B. Rogers.

NOTE ON THE MODE IN WHICH THE PHOSPHATES PASS INTO PLANTS. BY M. P. THENARD.

Phosphate of lime is soluble in aërated water, but the phosphates of iron and alumina are insoluble in that fluid. When 50 grammes of one of the soils examined by the author (soils produced by the decomposition of jurassic rocks) are placed in a Seltzer water-bottle and filled up with aërated water saturated with phosphate of lime, corked and shaken, if the water be filtered in three or four days, the lime is found under the form of carbonate, but not the smallest trace of phosphate. With artificially prepared alumina or oxide of iron in place of the natural soil, the result is identical. In all the soils examined by the author, the phosphates always had a sesquioxide (alumina or iron) for their base, and never a protoxide (lime or magnesia). And when phosphate of lime was mixed with the soils, the action of the rains and a few weeks of contact were sufficient to cause the complete disappearance of the phosphate added.

In the list of natural elements to which agricultural chemists have directed their attention, there is not one which dissolves the sesquiphosphates; so that we might logically conclude that, as the sesquioxides are only contained in plants in infinitesimal quantities, the plants ought to be almost completely destitute of phosphoric acid. There must consequently be some special arrangement for the introduction into plants of the considerable amount of phosphoric acid found in their ashes.

In studying the silicates, the author was led to think that certain silicates of lime must be far more soluble than was generally supposed; on treating a great excess of chloride of calcium dissolved in a large quantity of water, with a silicate of soda as neutral as possible, the silicate of lime formed had the power of dissolving freely in water in the proportion of 6 decigrms. per litre.

Reflecting on the insolubility of the silicates of alumina and iron and of phosphate of lime in water, and the solubility of the latter in aërated water, the author was led to suspect that it might be by means of silicate of lime and other soluble silicates that phosphoric acid is set free from its compounds with iron and alumina, and this view was confirmed by the following experiments:—

A solution of silicate of lime, mixed with phosphate of alumina, pure or containing an excess of alumina, is put into one of the common apparatus for preparing aërated water. The liquid is then saturated, in the ordinary manner, with carbonic acid, and the whole is left to digest for twenty-four hours, shaking it from time to time. If the liquid be then removed and filtered, the ordinary reactions demonstrate the presence of a considerable quantity of phosphate of lime.

In operating upon a soil instead of phosphate of alumina, taking the precaution to boil it for fourteen or fifteen hours with an excess of silicate of lime, the same result is obtained.

This experiment cannot furnish a means of determining the amount of phosphates in a soil; it is qualitative, and by no means quantitative; it is simply a moment of passage, but it is of this moment that plants appear to take advantage in order to appropriate the phosphates they require. Fortunately this moment is prolonged as long as there are silicates in solution, and as this is the case in all soils in which plants grow, the latter are always furnished with a larger or smaller quantity of protophosphates. It is a sort of change effected at the expense of the silicates, whilst the iron and alumina fix the unappropriated phosphoric acid, and retain it in the soil for the benefit of future generations of plants.

After instancing some districts in France in which the presence or absence of phosphates or silicates in the soil directly influences the selection of manures, the author states that it is especially to the silicate of lime that he attributes the property of decomposing the sesquiphosphates. The silicate of magnesia would probably have the same action; the silicates of soda and potash act upon the compounds formed by stable manure in the soil, so as to complicate the phænomenon and render conclusions more doubtful. — *Comptes Rendus*, February 1, 1858, p. 212.

ON THE MECHANICAL EQUIVALENT OF HEAT. BY P. A. FAVRE.

In the third part of my researches on hydro-electric currents[*], it was not so much my object to determine the mechanical equivalent of heat by means of a new and, as I believe, very direct method, as to supply new proofs of the existence of a single force, which, though its manifestations vary, is never subjected to such modifications as to cause us to question its fundamental identity. In this way the equivalence between the various species of manifestations may be expressed by a number derived from any one of them.

Thus, in the researches mentioned, where my chief aim was to study the correlation between chemical affinity and the other dynamic manifestations of matter, I showed that the amount of affinity may be expressed in kilogrammetres[†]. In another series of investi-

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[*] *Comptes Rendus de l'Acad. de Scien.*, July 13, 1857.

[†] One kilogramme, through one metre.

gations, performed partly in conjunction with M. J. T. Silbermann, it has already been shown that such affinity may be expressed in units of heat (*calories*).

The determination of the mechanical equivalent of heat was one of the consequences flowing from an investigation of the kind just described, inasmuch as I have succeeded in expressing in kilogrammetres the work done (*travail moteur*) by molecular attraction (chemical affinity), in the same manner as the work done by universal gravitation (weight) had already been expressed.

Thus, as announced in my last memoir, I have checked by inverse experiments the results I had obtained, and on which I reposed confidence.

In fact, I raised 426 kilogrammes 1 metre by means of 1000 heat-units furnished by affinity and put into activity by the replacement of hydrogen by zinc in hydrated sulphuric acid. I have now shown how much heat is caused by the expenditure of work due to the motive force of a previously raised weight, and I can substitute the action of weight for that of chemical affinity.

The experiments which I am about to describe present considerable analogy with those published by Mr. Joule, and which led him to numerical results not very different from those since obtained experimentally by other physicists.

In my new researches, the work done by the fall of a weight is expended upon a break, enclosed in a mercurial calorimeter (*thermomètre à calories*). In none of the experiments were less than 300 heat-units produced, this corresponds to a length of about 75 millimetres of the calorimetric tube.

The arrangement of the apparatus is as follows:—

The capacity of the calorimeter is about 5 litres. The break is placed in a box 12 centimetres in length and breadth, and 18 centims. deep. It is fastened by means of three milled-headed screws, by which it may be raised or depressed at pleasure. This break, skilfully made by M. Santi of Marseilles, is 11 centims. wide and 16 centims. deep. It consists essentially of two vertical uprights, carrying in their sockets a steel axle. This axle carries a cylindrical drum throughout its whole length, which is divided in the middle by a disc, thus forming two reels, upon each of which a separate catgut cord is wound. These cords are wrapped in opposite directions.

A weight of 10·268 kilogrammes, hung at the extremity of one of these cords, in addition to the permanent weight of 2 kilogrammes, unwinds the cord to which it is applied, and at the same time winds up the other cord, overcoming the tension in it, caused by another permanent weight of 2 kilogrammes fastened to its extremity. This tension is sufficient to ensure regularity in the unwinding. Thus, when one cord is unwound, an equal length of the other is wound up, and, by taking off the extra weight and hanging it at the end of the cord which was before being wound up, the same effect is produced, but in an opposite direction. This motion of the two weights in contrary directions is easily effected by means of two pulleys, fastened independently of the break at a height of 3·5 metres, each of which receives on its groove one of the cords as it leaves its reel. The apparatus is so arranged as to allow the weight a fall of 4·50 metres. The friction of the different parts having been reduced as far as possible, is allowed for by Coulomb's laws.

On one end of the axle a copper disc is fastened, 8 centims. in diameter and 8 millims. in thickness at its edge. When one of the cords unrolls by reason of the weight which it carries, the reel and its axle turn and give a rotatory motion to the disc.

A split ring tending to spring open and terminating at both ends in ears, clasps the circumference of the disc, while a rod bearing a screw-thread works in the lower ear of the ring, thus allowing the latter to be opened or shut at pleasure, thereby causing a frictional resistance to the rotation of the disc. This friction is made sufficiently uniform by fastening, on the inner circumference of the break-ring, five steel springs at equal distances apart, and with their convex sides turned towards the circumference of the disc. In this way, as long as the ring is open, the springs scarcely graze the disc, but as the ring is closed, the springs are straightened and the friction is exercised on a more extended surface. When the ring is completely screwed up, the five springs coming in contact with the disc almost throughout their entire length, arrest its motion.

In order to avoid shocks, and to ensure greater uniformity of motion, on the extremity of the axis opposite to the disc a toothed wheel is fastened, which works in an endless screw turning on a vertical axis. This axis also carries a toothed wheel which works in a pinion, the upper extremity of whose axis is provided with a ball governor.

In order to avoid loss of heat by conduction, the key with which the experimenter opens and shuts the break is made of ivory.

The distance of fall of the additional weight was at least 12 metres. It fell with great regularity and without the least shock, its velocity being on the average 20 millims. per second. It is necessary that the fall be corrected for the elongation which the cord undergoes, under the influence of the additional weight. This correction may be performed with great ease and precision.

The box containing the break is perfectly enclosed by means of an obturator of cork 15 millims. thick. This obturator only allows the ivory key to pass out, and the cords whose alternate unrollings effect the rotation of the axle of the break.

The length of the mercurial column, equivalent to one heat-unit on the calorimetric tube, was fixed when the break, fastened in its place, made part of the mass of the calorimeter.

The greatest pains were taken to avoid all influences tending to render too small the number expressing the heat produced by the destruction of the motive power. In fact, in experiments of this kind, the tendency is always to diminish this number, consequently to increase the mechanical equivalent of heat.

Having, therefore, adopted all possible precautions, I found by experiments agreeing very well with one another, the number 413·2, which differs very little from the number 426 which had to be checked.—*Comptes Rendus de l'Acad. de Scien.*, Feb. 15, 1858.

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LII. *On the Optical Properties developed in Transparent Bodies by the Action of Magnetism. By* M. VERDET[*].

PART III[†].

AFTER having determined the laws according to which the optical properties developed by magnetism in transparent bodies vary with the magnitude and direction of the magnetic forces, I undertook to investigate how these phænomena are influenced by the nature of the transparent body. This I have done by measuring the rotation of the plane of polarization, effected by bodies of very different nature, when placed under analogous circumstances, between the poles of an electro-magnet. Former labours of physicists have afforded me very scanty information on this subject. Previous to my undertaking this investigation, the only data which science possessed, consisted in a small number of numerical determinations contained in the memoirs of MM. Bertin, Edmond Becquerel and Wiedemann, and in the much more numerous and varied experiments of Prof. Faraday, which latter, however, were intended to show the general nature of the phænomena without attempting their measurement. From so small a number of facts, it was impossible to deduce any general law with certainty; and I shall show that, in reality, those laws which have been sought to be established are inexact.

In order to study the question, I have merely had to apply to a sufficient number of substances the methods which I de-

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[*] Translated from the *Annales de Chimie et de Physique* for February 1858, by Dr. F. Guthrie.

[†] See the two former parts of this research, *Ann. de Chim. et de Phys.*, s. 3. vol. xli. p. 370, and vol. xliii. p. 37; the principal results contained in these memoirs will be found abridged in the ninth volume of the present series of the Philosophical Magazine, p. 481.

scribed in my first memoir, adopting, however, certain modifications, the effect of which has been to cause either an increase in the magnitude of the phænomena to be observed, or a diminution in the duration of the experiment, without in the least impairing the exactness of measurement. In place of the great armatures made use of in my former researches, I have employed much smaller ones, consisting of iron plates 8 millims. thick, in the form of regular hexagons whose sides are 13 millims. long*. The effect of this has been, all other conditions being the same, to increase considerably both the magnetic and optical actions; and even when these two actions have not been constant throughout the whole thickness of the substance placed between the armatures, yet such variation has not been attended by any inconvenience. For when any two transparent bodies of the same thickness have been placed successively in the same position between the armatures of the electro-magnet, the several corresponding layers of both have been subjected to equal magnetic actions, and have therefore exercised optical actions proportional to the specific optical actions of the two bodies. Hence it is clear that the sums of these elementary optical actions, that is, the total observed rotations, bear the same proportion to one another as if the magnetic, and consequently also the optical, action had been invariable throughout the whole magnetic field. Now it is clear that this proportion is the only thing with the determination of which we are concerned.

The intensity of a current, and consequently also that of an electro-magnet, never remains constant during a series of experiments extending over several days, or even hours; and still greater variations are produced when the liquids of the battery are renewed, or the number of its elements altered. In order that my determinations might be independent of the effect of these variations, recourse was had to two methods. In the first, every measurement of optical action was both preceded and followed by an estimation of the magnetic force midway between the armatures of the electro-magnet; the mean of these two values was the magnetic action with which the optical action was compared. Experiment showed that for every substance this relation is constant, as long as the variations in strength of the electro-magnet are confined within certain limits;—limits moreover which may be much more widely apart than those bounding the variations occurring in my actual experiments†. Hence, by a

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* These are the hexagonal armatures attached to all Ruhmkorff's machines, and which are mentioned in the first part of my researches.—*Ann. de Chim. et de Phys.*, sér. 3. vol. xli. p. 409.

† Thus, on passing the current, first of twenty and then of ten of Bunsen's elements, through the wire of the electro-magnet, I obtained magnetic actions represented by the numbers 100·7 and 78·3. The corresponding

simple proportion, it was easy to reduce all the observed rotations to one constant magnetic action.

This plan of correction necessitated two systems of measurement, optical and magnetic. In consequence of the great number of determinations which I had to make, it became essential to shorten the time required for each experiment; I soon, therefore, had recourse to a method equally exact, and much more expeditious than that of correction; namely, direct comparison between the optical actions of bodies and that of distilled water. To effect this comparison, on some occasions the measure of the rotation produced by the transparent substance was taken between two measures of the rotation produced by an equal thickness of distilled water; sometimes, when the battery had been recently put together, and showed an action of almost perfect uniformity, the operation was still further abridged in the following manner. Distilled water was first submitted to the action of the electro-magnet, and the azimuth of the plane of polarization of the emergent light determined. The water was then replaced by the substance under investigation, and the azimuth of the plane of polarization again determined. The direction of the current being then reversed, the consequent displacement of the plane of polarization was measured. Finally, distilled water having been again placed between the armatures, a fourth and last azimuth was determined. Half the difference between the two azimuths belonging to the water, and half the difference between the two derived from the transparent substance, were considered as exactly expressing the corresponding rotations caused by the same electro-magnetic force*.

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rotations of the plane of polarization effected by a thickness of 20 millims. of sulphide of carbon were 4° 16′ and 3° 18′. The proportion between the rotation (expressed in minutes) and the magnetic action was therefore 2·542 in the first, and 2·529 in the second case, showing that this relation may be considered constant. Moreover, the difference between 100·7 and 78·3 far exceeds the variation which the strengh of an electro-magnet can undergo in a series of experiments extending over several days, if the nitric acid of the battery remain unchanged. I never allowed this variation to attain the tenth part of the initial power of the magnet.

The empirical result just mentioned could not be deduced from the proportion between the rotation and the magnetic action the existence of which I proved in my first memoir, because we are not at liberty to assume that, when the electro-magnet varies in power, the action which it exercises at different points of the interval between the armatures varies in the same proportion.

* The following are the complete elements of a comparison, effected by the second process, between water and a solution of sulphate of nickel. The two liquids were employed in thicknesses of 40 millims.; the distance between the hexagonal armatures was 50 millims., and the battery consisted of twenty Bunsen's elements.

Distilled water being placed in the apparatus, and the electro-magnet

The azimuth of the plane of polarization was always determined by observing the passage-tint (*teinte de passage*). When the body under examination is colourless or faintly coloured, it is known that the passage-tint indicates the plane of polarization of the middle yellow rays; but the case is quite different with a strongly coloured body, and grave errors are incurred if, to obtain the relation between the optical action of such a body and that of water, we compare without correction the two deviations of the passage-tint. This source of error was got rid of

being set to work, I observed the following azimuths of the passage-tint:—

50° 38′
50 44
50 42
50 40
Mean . . 50 41

The water being replaced by sulphate of nickel, the azimuths—

51° 26′
51 23
51 20
51 26
Mean . . 51 23 45″

were obtained. On reversing the current, the azimuths of the passage-tint from the sulphate of nickel were,—

46° 37′
46 36
46 33
46 31
Mean . . 46 34 15″

Finally, the sulphate of nickel being replaced by distilled water, the last series of azimuths were observed:—

47° 10′
47 12
47 13
47 15
Mean . . 47 12 30″

Hence we get the following values for the rotations:—

Distilled water . . .	1° 44′ 15″
Sulphate of nickel . .	2 24 45

In order to deduce from these numbers the true relation between the optical actions of sulphate of nickel and distilled water, they must be submitted to two corrections which will be described in the sequel.

It is to be observed, that in taking the mean of the azimuths derived from distilled water, and the mean of those from the sulphate of nickel, one ought in both cases to get the azimuth of the primitive plane of polarization, and that consequently, if the experiments have been well performed, these two mean values should be equal. Their actual values are 48° 56′ 45″ and 48° 59′, the difference between which comes within the possible errors of observation.

by examining the passage-tint from distilled water, through a thickness of the coloured substance under investigation, equa to that afterwards employed in acting on the light. The luminous pencil which fell upon the analyzer had thus the same composition as in the case of the coloured substance. The passage-tint in both experiments corresponded therefore to the plane of polarization of the same colour of the spectrum, and the relation of the deviations of this tint gave the relation between the optical actions.

Finally, the particular nature of the substances which I examined rendered another correction necessary. In order that my experiments might be confined to perfectly definite bodies and such as may be obtained in a state of perfect purity, I have only examined liquids*; and these of course had to be introduced into cases closed at their ends with transparent plates. If care be taken to choose plates of glass free from flaws, and to close the cases in such a manner that the pressure upon the plates is sufficiently small and uniformly distributed upon their whole circumference, then the plates, in their ordinary state, have no action upon polarized light; but under the influence of magnetism they exercise a sensible action, which assists the action of the liquid, and which must therefore be taken into account. For this purpose, before employing a case in my experiments, I examined it, successively, empty and full of distilled water, so as to determine the relation between the rotation produced by the action of the plates and the total rotation produced by the simultaneous action of the plates and the water. It is easy to understand how the knowledge of this relation enables us to correct all the observations made with the same case. The above determination was, moreover, repeated as often as I had occasion to alter the relative positions of the case and the armatures of the electro-magnet. In my experiments, twice the rotation of the plane of polarization due to the action of the glass plates varied from 30′ to 1°, according to the nature of the plates and their distances from the armatures†.

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* The only solid bodies suitable for experimenting on are glasses,—bodies whose composition is too ill defined for much interest to attach to their action. The singly-refracting crystals which might be employed are so difficult to obtain in pure and flawless pieces of any thickness, that I have made no use of them in this part of my investigation.

† It may perhaps excite surprise that I have sought to measure with any degree of accuracy such feeble rotations as these. In order to show that this method admits of exactness, I shall give the elements of a determination made with plates enclosing a case 44 millims. in thickness. The distance between the armatures of the electro-magnet was 54 millims., and the current was produced by twenty of Bunsen's elements. The current having been made to pass in succession in two opposite directions round

The thickness of the liquids employed varied from 10 to 50 millims.

The temperature of the laboratory where the experiments were performed remained always between 12° and 18° C.

The rotation of the plane of polarization produced by a given thickness of a transparent substance subjected to a magnetic action of given strength, is a physical constant as characteristic of the substance as its refractive index or dispersive power. It is therefore advisable that some name be given to this constant: the term "magneto-rotary power" seems to me to be applicable. I shall make constant use of this expression in the sequel. Representing the magneto-rotary power of distilled water by unity, the rotary powers of other substances will be represented by their relations to this unit. I may add, in order to give an idea of the magnitude of the phænomena observed, that when my electro-magnet was excited by a recently fitted up battery of twenty Bunsen's elements, a thickness of 40 millims. of distilled water, placed between the armatures, which were 50 millims. apart, produced a rotation of about 1° 5′.

My first task was to test experimentally certain laws which

the electro-magnet, the following two series of azimuths of the passage-tint were observed:—

	346° 31′		347° 18′
	346 32		347 18
	346 30		347 11
	346 30		347 16
	346 30		347 10
	346 27		347 16
	346 28		347 12
	346 28		347 16
Mean .	346 29 30″	Mean .	347 15

Whence 45′ 30″ is derived as the rotation due to the action of the glass plates. The agreement among the individual observations is sufficient to guarantee the accuracy of this result. I did not consider it essential always to determine each azimuth eight times; in general, four determinations seemed to me sufficient.

In my former researches, in operating upon bisulphide of carbon, this correction was omitted; for, having in fact no other purpose beyond the determination of how the action of the sulphide of carbon varied with the magnitude and direction of the magnetic force, it was a matter of indifference to me that the action actually measured was the sum of two distinct actions subject to the same law of variation. Moreover, by the arrangement of the apparatus, the correction in point would fall within the class of observation errors; for, by reason of the great size of the armatures and their distance from the glass plates, the magnetic force exerted upon these plates was much less than in my later experiments, where a great magnetic energy was concentrated in small armatures, brought as closely as possible to the bodies under examination.

had been thought to follow from the observations of MM. Bertin and Edmond Becquerel.

In his *Traité de l'Electricité,* M. De la Rive, after having described the principal experiments bearing on the magnetic rotations of the plane of polarization, and having developed certain theoretical ideas, makes use of the following words*:—

"Thus the magnetic force only acts upon the æther by the intervention of particles, and when the æther has a certain density due to the action exerted upon it by the particles between which it is placed; the magnetic action being more intense according as the density of the æther is greater. Now, inasmuch as the density of the æther does not depend exclusively upon that of the body, that is, upon the approximation of the particles composing it, but also upon the nature of these particles, it follows that it is not always the denser body which is the most refrangent, and which therefore shows the greatest magneto-rotary polarization. Experiment completely confirms this view; and on glancing at the Table (still very incomplete) of the coefficients of magnetic polarization, we are struck with the fact that the substances in this Table follow almost the same order as in the Table of their refractive powers. New investigations are requisite to establish fully the analogy which I have just pointed out, and especially to determine the nature of the modifications which the æther experiences under the influence of magnetism."

This remark of M. De la Rive attracted my notice during my former researches, as being worthy of a more attentive investigation. The simplicity of the laws governing the magneto-rotary powers inclined me, for a while, to the opinion which refers these phænomena to the direct action of the magnetic forces upon the æther rather than upon the ponderable matter present, and I therefore thought it highly probable that a relation might exist between the magneto-rotary power and the refractive index, which latter, as we know, may be taken as a measure of the square root of the density of the æther. It will be seen immediately that my conjecture has not been confirmed by experiment.

In fact, I have determined the magneto-rotary powers and the mean refractive indices of a considerable number of liquids, consisting for the most part of more or less concentrated saline solutions†. From this preliminary examination all magnetic liquids were omitted, which, according to the experiments of MM. Bertin and Edmond Becquerel, seemed to be excluded from the law enunciated by M. De la Rive. The indices of re-

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* Tome 1er, p. 555.

† Almost all the solutions employed were prepared and tested under the direction of M. Debray, in the chemical laboratory of the École Normale supérieure.

fraction were measured by Babinet's goniometer, the liquids being enclosed in an excellent glass prism, whose refracting angle was 43° 41′, constructed by M. Brunner.

This series of experiments showed me that there is no simple relation between the refractive indices and the magneto-rotary powers. This will be seen by referring to the following Table, where the results of my experiments are given, the several liquids being arranged according to their progressive refractive indices.

TABLE I.

Liquids.	Refractive Indices.	Magneto-rotary Powers.
Distilled water	1·334	1·000
Solution of borate of soda	1·341	1·000
,, chloride of calcium	1·354	1·085
,, carbonate of potash	1·355	1·050
,, nitrate of lead	1·355	1·000
,, chloride of magnesium	1·357	1·127
,, chloride of ammonium	1·359	1·184
,, protochloride of tin	1·364	1·348
,, chloride of zinc	1·368	1·341
,, chloride of ammonium	1·370	1·371
,, carbonate of potash	1·371	1·087
,, chloride of calcium	1·372	1·230
,, protochloride of tin	1·378	1·525
,, chloride of zinc	1·394	1·507
,, protochloride of tin	1·424	2·047
,, nitrate of ammonia	1·448	0·908
Chloride of carbon, $C^2 Cl^4$	1·466	1·264

It is seen from this Table that the order of the refractive indices is quite different from that of the magneto-rotary powers. Thus, merely to adduce the most striking cases, nitrate of ammonia, whose magneto-rotary power is the most feeble of all those given, has one of the highest refractive indices. Again, chloride of carbon, which is more refractive than any of the solutions of protochloride of tin examined, has a less magneto-rotary power than the least concentrated of these three solutions. The solutions again of chloride of calcium, carbonate of potash, and of chloride of ammonium, whose refractive indices are almost equal, have very widely different magneto-rotary powers.

The law enunciated by M. De la Rive is therefore not supported by experiment, and the magneto-rotary powers of bodies do not appear to depend exclusively upon the constitution of the æther which they contain. The question naturally suggests itself, whether such rotary powers do not rather depend upon the diamagnetic properties of bodies,—whether, for instance, as some physicists have imagined, they are not greater according as the diamagnetism of the bodies increases. Although I have not made any direct experiments on this point, yet it will be seen in the sequel, that, in the prosecution of my researches, the question has been solved indirectly and in the negative.

In the preceding Table several series of numbers are given, belonging to different solutions of the same salt in different states of concentration. This gives rise to the following important observation. If we suppose that in a solution the molecules of the water and those of the salt exert their actions upon the polarized light independently of one another, and if, in accordance with this hypothesis, and taking account of the composition and density of the solution, we calculate the action exerted by the dissolved salt, we find, for the different solutions of the same salt, numbers proportional to the quantity of salt contained in one unit volume of the solution. The above hypothesis is accordingly supported by experiment, and the salt dissolved in water behaves like sugar dissolved in an *active* liquid. The constant quotient obtained by dividing the rotation due to the salt by the quantity of salt contained in a unit volume of the solution (density of salt in solution), may receive the name "*molecular magneto-rotary power.*" In the following Table I have arranged the results given by solutions of protochloride of tin, chloride of zinc, and chloride of ammonium.

TABLE II.

Salt dissolved.	Density of Solution.	Weight of Salt contained in unit of		Magneto-rotary Power.	Rotation due to		Molecular Magneto-rotary power of Salt dissolved.
		Weight of Solution.	Volume of Solution.		Water.	Salt.	
Protochloride of tin ..	1·3280	0·302	0·401	2·047	0·927	1·120	2·79
Protochloride of tin ..	1·1637	0·170	0·198	1·525	0·966	0·559	2·81
Protochloride of tin ..	1·1112	0·120	0·133	1·348	0·978	0·370	2·71
Chloride of zinc	1·2851	0·266	0·342	1·507	0·943	0·564	1·65
Chloride of zinc	1·1595	0·150	0·174	1·241	0·985	0·276	1·59
Chloride of ammonium	1·0718	0·247	0·265	1·371	0·807	0·564	2·13
Chloride of ammonium	1·0493	0·129	0·135	1·184	0·914	0·270	2·00

No similar calculations were made with the two solutions of chloride of calcium and the two of carbonate of potash which I examined, because the magneto-rotary powers of three of these solutions differed so slightly from that of water, that it was impossible to make use of them to calculate the exact values of the molecular powers.

Hence, *when a salt dissolves in water, the water and the salt each contributes its own proper magneto-rotary power to the solution, and the rotation produced by the solution is the sum of the separate rotations due to the molecules of the two substances.*

It is very probable that this law holds good with all solutions of every kind, and with mixtures formed of liquids which have no chemical action upon one another. But as yet I have not made any experiments to confirm this. I have limited myself to showing that the general bearings of the phænomena are the same; that, for instance, those salts whose aqueous solutions have a high magneto-rotary power, behave in the same manner when dissolved in alcohol or æther. This I have shown in the case of several metallic chlorides which are soluble in these liquids, namely, the bichlorides of mercury and of tin, and the chlorides of cobalt and nickel, which latter two, though magnetic, behave like diamagnetic bodies.

Most salts impart to their aqueous solutions magneto-rotary powers superior to that of water. Sometimes, however, the reverse is the case; the salt contained in a given volume of the solution exerting upon polarized light a less amount of action than that effected by the water which it displaces. Thus the solution of nitrate of ammonia given in Table I., consists of 43 parts of water and 57 of the nitrate: its density being 1·2566, it follows that a unit volume contains 0·6660 of water and 0·5906 of the nitrate. The rotation being only 0·908ths of that of distilled water, may be regarded as the sum of a rotation 0·666 effected by the water, and one of 0·242 produced by the nitrate: the last number gives the small value 0·401 as the molecular rotary power. Similar phænomena would occur on mixing water with a liquid whose magneto-rotary power is less strong, such as alcohol, æther, or wood-spirit.

According to M. Bertin, solutions of the protosulphate of iron approach, in respect to their weakness in magneto-rotary power, to solutions of nitrate of ammonia. M. Edmond Becquerel made a similar observation with solutions of the protochloride of iron, and thought himself entitled to draw the general conclusion, that the rotation of the plane of polarization produced by the influence of magnetism varies in inverse proportion to the magnetic power of the body*. The experiments cited by M. Edmond Becquerel himself do not admit of so absolute a conclusion. In fact, from his memoir we see that, if the rotation of water be represented by 10, the rotation of two unequally concentrated solutions of protochloride of iron are represented by 9 and 3, and that of a solution of sulphate of nickel by 13·55; in other words, among the magnetic solutions examined, two

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* See *Annales de Chimie et de Physique*, 3 sér. vol. xxviii. p. 334.

produce a more feeble rotation than that of water, while the third exerts a more powerful one*. Nevertheless the extreme weakness of the magneto-rotary power of concentrated solutions of protochloride of iron, taken in conjunction with the observations of M. Bertin on the protosulphate of iron, appeared to me to indicate the existence of a particular mode of action, at least with compounds of iron, if not with all magnetic compounds, worthy of more minute examination.

Such examination has formed the principal object of my investigation, the results being those which I have obtained on examining in succession the compounds of the different magnetic metals.

Iron.—When a salt of the protoxide of iron is dissolved in water, the solution has always a magneto-rotary power weaker than that of water, and less according as the solution is stronger. But more than this: if, by taking account of the density and composition of the liquid, the rotation be calculated which would be produced by the water alone contained in a given volume of the solution, a number is found always greater than the observed one. It would therefore appear as if the salt of iron dissolved, exercised on the polarized light an action opposite to that of water. Thus a solution of protochloride of iron of density 1·2922, and containing 72·2 per cent. by weight of water and 28·3 of protochloride, effects under the influence of magnetism a rotation of 0·581, if the rotation produced by water be taken as unity. It follows from the above numbers, that one unit volume of the solution contains a proportion of water equal to 0·9265 of the weight unit, and that consequently if the salt dissolved were completely passive, the rotation should have been precisely 0·9265. Inasmuch as it is considerably less, it is natural to suppose that the salt dissolved effects a rotation opposite to that of water, and equal in absolute value to the difference between 0·926 and 0·581, that is, equal to 0·345.

All the salts of protoxide of iron which I have examined have furnished me with similar results. Taken in conjunction with the law established above concerning non-magnetic solutions, it is clear that salts of protoxide of iron, when subjected to magnetic influence, exert an action on polarized light contrary to that exercised by water, sulphide of carbon, glass and most trans-

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* M. Edmond Becquerel does not appear to have considered the influence which the coloration of liquids exerts upon the determination of the passage-tint. This influence, which is almost nothing in the case of salts of protoxide of iron, is very great with salts of nickel,—a fact which made me at first imagine that, by allowing for it, the salts of nickel would be brought under the general law enunciated by M. Becquerel. This is by no means the case; we shall subsequently see that solutions of nickel salts have really a rotary magnetic power greater than that of water.

parent substances. In order, however, that no doubt might remain on this point, I repeated on unequally concentrated solutions of iron salts, the experiments which had been performed on zinc and tin salts. For this purpose choice was made of two protosalts of iron,—the protosulphate and the protochloride,—which may readily be obtained free from all traces of peroxide. M. Deville furnished me with a sufficient quantity of the sulphate, prepared with the greatest care, from which two solutions were made, the one containing 17·4 per cent., and the other 10·5 per cent. of *anhydrous sulphate*. The densities of these solutions were respectively 1·1932 and 1·1135, and their magneto-rotary powers 0·740 and 0·838 respectively. On applying the same hypothesis and the same calculations to these solutions, which were given above in the case of protochloride of tin and chloride of zinc, two nearly equal values are obtained for the molecular-rotary power of protosulphate of iron, viz. −1·24 and −1·35. These values, having the opposite sign to that of water, and being sufficiently concordant, support the hypothesis. Experiments on the protochloride of iron led to the same result. The solution mentioned on page 419, made by dissolving pianoforte wire in pure hydrochloric acid, gave the number −0·94 as the molecular power. A dilute solution, prepared by adding pure water to the preceding one, and containing only 16 per cent. of protochloride, gave the number −0·82*.

It is therefore proper to distinguish two kinds of action exerted by transparent bodies subjected to magnetism upon polarized light. I shall call the rotary power of water and of most other non-magnetic transparent substances, *positive*; that of protosalts of iron and of bodies whose influence is in the same direction, *negative*. These terms are preferable to the expressions *direct* and *inverse*, which I employed in a previous publi-

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* The results of these experiments are worthy of consideration in another point of view. It is seen that the observed phænomena were represented numerically under the supposition that the solutions of the sulphate were formed of water and anhydrous sulphate, and by ascribing to the water and the salt actions contrary in direction and proportional to the densities which the two bodies had in the solution. If, on the contrary, the solutions are supposed to consist of water and of crystallized sulphate, containing 7 atoms of water, two values, viz. —0·2 and —0·3, are found for the molecular power, which are much more discordant than those got on the previous supposition. Similarly, on the supposition that the solutions of the protochloride contain the salt crystallized with 4 atoms of water, the numbers —0·51 and —0·13 are found for the molecular power of the body: two values which are quite irreconcileable. It hence appears to follow with some probability, that in both cases the salt dissolved is not the crystallized sulphate or chloride, but the anhydrous sulphate and anhydrous chloride. I imagine that the same kind of experiment might be applied to the solution of certain analogous chemical questions.

cation, because they have the advantage of reminding us of the direction of rotation. Water, sulphide of carbon, glass and other bodies whose magneto-rotary power is *positive,* turn the plane of polarization in the direction in which the positive current of electricity traverses the conducting wire of the electro-magnet: protosalts of iron and analogous bodies turn the plane of polarization in the direction in which the negative current passes through the same wire. It is scarcely necessary to mention, that in the numerical Tables these two kinds of rotary powers are denoted by + and −.

However concentrated the solutions were of the protosalts of iron, I only observed a greater or less diminution in the rotation of the plane of polarization: a solution of the protochloride, concentrated to the crystallizing point, showed a magneto-rotary power absolutely nothing; but I have not succeeded in effecting an entire reversion of the rotation. I had hopes of succeeding in this, and of thus directly demonstrating the negative rotary powers of protosalts of iron, in seeking for some solid compound rich in the protoxide of iron, and either uncrystallized or belonging to the cubic system of crystallization. Red garnet, which may contain as much as 30 per cent. of protoxide of iron, appeared suitable for the verification of this conjecture; but among numerous pieces examined, not one was found sufficiently free from flaws for experiment.

The magneto-rotary power of salts of the peroxide of iron, like that of the protoxide salts, is negative; but it is much greater and more easily shown than that of the protosalts. Perchloride of iron especially gives very remarkable results. A very dilute aqueous solution of this salt has a magneto-rotary power less than that of water. According as the solution is concentrated the rotary power diminishes, becomes nothing, and finally changes sign: after the change of sign the power increases up to the maximum of concentration. Thus, a solution near the maximum, containing 40 per cent. of perchloride, exerts an action on polarized light contrary to that of water, and from six to seven times as great; that is, nearly equal to the action of pieces of Faraday's heavy glass which I have examined.

Negative rotations may be still more readily obtained by substituting for water other solvents which, by themselves, have less action than water on polarized light, or which dissolve a greater quantity of the salt. Alcohol and æther satisfy the first condition, and are consequently very well adapted for showing the negative rotary power of persalts of iron. Thus, on dissolving in 80 parts of æther 20 parts of crystallized chloride of iron, prepared by the direct action of chlorine on iron, I obtained a strongly coloured solution whose negative rotary power was very well marked. With about half the quantity of salt, I obtained a solution almost entirely destitute of action on polarized light. Alcoholic solutions gave me similar results. But the best solvent which I have found is wood-spirit; this, by itself, has scarcely any action on polarized light, and although it dissolves a considerable quantity of the perchloride, yet remains much more transparent than water, æther or alcohol containing the same quantity of salt. Thus, on dissolving 55 parts of the crystallized perchloride in 45 parts of wood-spirit, a liquid is obtained which, on account of its transparence, is suitable for exact experiments, and whose action on polarized light is negative in sign, and equal in absolute value to almost twice that of heavy glass, or three times that of sulphide of carbon*.

Of all bodies hitherto examined, this solution is the one which, under the influence of magnetism, effects the greatest deviation in the plane of polarization. In my apparatus, a thickness of 10 millims. placed between the hexagonal armatures, which were 27 millims. apart, on being subjected to the action of magnetism, developed by twenty Bunsen's elements, gave a rotation of 6° 31′ 30″†. The greatness of this result induced me to choose the solution of perchloride of iron in wood-spirit, in order to determine whether the negative magneto-rotary power of iron salts varied with the magnitude of the magnetic action according to the same laws which occur with the positive rotary power of ordinary substances. For this purpose I compared the rotation produced by a thickness of 1 centim. of the solution with that produced by an equal thickness of sulphide of carbon; the magnitude of these rotations being made to vary either by varying the strength of the electro-magnet, or by altering the

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* From experiments upon these various liquids, I obtained several values for the molecular magneto-rotary power of perchloride of iron; but my results were rather discordant. In fact, it is known that on treating crystallized perchloride of iron with water, æther, alcohol, or wood-spirit, something more than a simple solution takes place. A considerable elevation of temperature is always observed, which is sure evidence of the occurrence of some more or less complex chemical change. With alcohol, æther and wood-spirit, M. Kuhlmann has long since shown that a definite crystallizable compound is formed between the perchloride and its solvent. The hypothesis therefore on which the calculation of the molecular powers was based, ceases here to be admissible.—See Memoir of M. Kuhlmann, *Mémoires de la Société des Sciences de Lille*, vol. xvi. 1839.

† This number is corrected for the influence of the glass plates closing the case, and for the influence of the colour of the solution upon the position of the passage-tint. The rough difference between the two azimuths of this tint, corresponding to the two opposite directions of the current, was 9° 40′.

size, form or distance of the armatures. The following Table contains the results of the experiments*:—

TABLE III.

I. The great armatures, 140 millims. in diameter and 50 millims. thick, being employed.	
Distance between the armatures 50 millims.	
Battery of twenty elements.	
Rotation produced by sulphide of carbon .	+0° 44′.
Rotation produced by the solution . . .	−2° 4′ 45″.
Proportion between the first and second rotations	−2·83.
II. The hexagonal armatures being employed.	
Distance between the armatures 65 millims.	
Battery of twenty elements.	
Rotation produced by sulphide of carbon. .	+0° 55′ 15″.
Rotation produced by the solution . . .	−2° 28′ 30″.
Proportion between the two rotations . .	−2·83.
III. The hexagonal armatures being employed.	
Distance between the armatures 27 millims.	
Battery of ten elements.	
Rotation produced by sulphide of carbon .	+1° 43′ 15″.
Rotation produced by solution	−4° 54′.
Proportion	−2·84.
IV. The hexagonal armatures being employed.	
Distance between the armatures 27 millims.	
Battery of twenty elements.	
Rotation produced by sulphide of carbon .	+2° 22′ 45″.
Rotation produced by the solution . . .	−6° 31′ 30″.
Proportion	−2·74.
Mean value of the ratios of the rotations .	−2·82.

Hence we see that the value of the ratio between the two rotations is the same in all the experiments. Consequently the negative magnetic rotation of the plane of polarization, like the positive rotation, varies in proportion to the magnetic action. It seemed to me highly probable, by reason of this conformity, that it also varied directly with the cosine of the angle contained between the direction of the magnetic action and that of the ray of light, but I have not performed any experiments on this subject. A thickness of 1 centimetre of the solution of perchloride of iron in wood-spirit, placed upon the apparatus described in

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* The numbers in this Table are corrected for the influence of colour, and for that of the glass plates of the case.

my second memoir*, gave rise to rotations of too feeble a kind: with a thickness of 3 or 4 centimetres the solution was not sufficiently transparent for accurate observations.

Nitrate of the peroxide of iron, subjected to the action of magnetism, exerts an action on polarized light in the same direction as the perchloride, but of less energy. An aqueous solution of this salt has a less magnetic power than water: when the solution is quite concentrated, the rotary power is almost nothing, but there is no change of sign in the rotation.

The two prussiates of potash, or double cyanides of iron and of potassium, seemed to me to be worthy of a special study. In fact, we know from the experiments of Faraday† and Plücker‡, that the yellow prussiate is diamagnetic, and the red prussiate magnetic. I have found that the magneto-rotary power of the yellow prussiate is positive and moderate, while that of the red prussiate is negative and very great. 15 parts of the red prussiate dissolved in 85 parts of water, give a liquid whose magneto-rotary power is negative, and in absolute value nearly double that of water. Inasmuch as it will be seen in the sequel that there are diamagnetic compounds of magnetic metals whose magneto-rotary power is negative, it is probable that the positive power of the yellow prussiate is not owing to the diamagnetism of this compound, but to the fact that the physical properties of the iron it contains are as completely masked as the chemical ones.

Nothing is easier than to prepare an aqueous solution of perchloride of iron or red prussiate of potash, suitable for showing, even on the lecture-table, the negative action of iron salts on polarized light. Nevertheless it would be advantageous for this purpose, and interesting for other reasons, to possess solid transparent bodies having the same properties as solutions. I must confess that my search for such bodies has hitherto been fruitless. Some very transparent crystals of iron-alum, 1 centim. in thickness, given me by M. Deville, showed so high a degree of lamellar polarization, that I could make no use of them. Several fragments of iron-glass which I tried, were either not sufficiently transparent, or not sufficiently free from flaws. I hoped to be more fortunate with the glasses of phosphate of lime, which M. Margueritte has recently shown to be capable of taking up the most varied metallic oxides. M. Margueritte was kind enough to have several pieces made for me, containing from 5 to 20 per cent. of peroxide of iron. But none of these pieces, even after a very prolonged re-annealing, were found sufficiently free from flaws for experimenting on. It is probable that among the

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* See *Annales de Chimie et de Physique*, 3 sér. vol. xliii. p. 37.

† Phil. Trans. for 1846.

‡ Poggendorff's *Annalen*, vol. lxxiv.

numerous iron-based glasses examined by M. Matthiessen*, some might be found of a suitable nature, but I have not been able to obtain permission to examine them.

No better success attended me in seeking to prepare an easily fusible compound of iron of sufficient transparence in the liquid state for the study of its action on polarized light, without the necessity of dissolving it. The properties of the double chloride of aluminium and sodium induced me to imagine that the corresponding chloride of iron and sodium, if it existed, might be suitable. This compound actually exists, and may be easily prepared by heating together 33 parts of perchloride of iron and 12 parts of chloride of sodium; it melts easily over an alcohol lamp, but in the liquid state is quite opake.

Nickel.—All the salts of nickel which I have tried have a positive magneto-rotary power, so that their solutions exert a greater action on polarized light than that of the water which they contain. This positive rotary power is sufficiently marked, and comparable with that of zinc and tin salts. It is particularly necessary, in determining the power of nickel, to take account of the influence which the colour of the emergent light exerts upon the position of the passage-tint. Thus, in light which has passed through 40 millims. of a moderately strong solution of chloride of nickel, the red, orange, violet, and indigo are almost extinguished, the blue and the yellow are considerably weakened, and the maximum intensity corresponds with the green rays. Hence it follows that the passage-tint is much more deviated than if the light passed without alteration through the chloride.

Cobalt.—The magneto-rotary power of salts of cobalt is positive, but weaker than that of nickel compounds, and rather difficult to be shown, because no considerable quantity of cobalt salts can be dissolved in water without greatly diminishing the transparence of the liquid. The colour of the emergent light exerts an opposite action on the position of the passage-tint to that which occurs in the case of nickel salts. Red being the dominant colour, the deviation of the passage-tint is diminished in such a manner, that if the necessary correction were neglected, the magneto-rotary power of cobalt salts would appear negative and very weak.

Manganese.—Salts of the protoxide of manganese have an inconsiderable positive magneto-rotary power; but, as their solutions are quite colourless, the action is very easily recognized.

Salts of the sesquioxide of manganese have so high a colouring power that they cannot be employed in experiment. But in the laboratory of the Collége de France, I found a compound probably corresponding to these salts, namely, the double cyanide

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* *Comptes Rendus des Séances de l'Académie des Sciences*, vols. xxiv. and xxv.

of manganese and potassium, $K^3 Mn^2 Cy^6$, which gave me a remarkable result. This salt, when dissolved in water, so diminishes its rotary power, that the salt itself must be regarded as having a negative rotary power. Is the same the case with the other salts of the sesquioxide of manganese? I am not prepared to answer this question decisively, but the excess of negative power of persalts of iron over protosalts inclines me to answer in the affirmative. However this may be, and remaining strictly within experimental limits, we see that manganese occupies in one respect an intermediate place between iron on one side, and nickel and cobalt on the other. That which is the rule with iron salts appears to be the exception with those of manganese, and *vice versâ*.

The interesting property of the double cyanide of manganese and of potassium induced me to examine the double cyanide of cobalt and potassium in the hope of meeting an analogous behaviour; but it was found to have a positive magneto-rotary power of rather feeble amount. This double cyanide is even diamagnetic.

Chromium.—The salts of the protoxide of chromium are so difficult to prepare, and especially to keep pure, that I did not subject them to experiment. The salts of the sesquioxide have so great a colouring power, that it is impossible to form an aqueous or alcoholic solution containing a few per cent. without destroying all transparence. But chromic acid and the chromates are adapted for exact experiments. Neutral chromate of potash, which is very soluble in water, gives clear yellow solutions whose coloration admits of all requisite experimental exactness, and only exercises a very insignificant influence on the position of the passage-tint. Bichromate of potash, although less soluble in water, gives solutions of deeper colour, but still very transparent, and requiring no correction in determining the position of the passage-tint. Chromic acid, on the contrary, gives solutions of a very deep red, whose colour exerts a great influence on the position of the tint, but which, in thicknesses of from 1 to 2 centimetres, are sufficiently transparent.

Solutions of these three substances have given me the following results:—

Table IV.

Nature of body dissolved.	Density of solution.	Weight of substance dissolved in		Magneto-rotary power of solution.	Rotation due to		Molecular magneto-rotary power.
		Weight-unit of solution.	Volume-unit of solution.		Water.	Substance dissolved.	
Neutral chromate of potash.	1·3598	0·369	0·504	0·76	0·86	−0·10	−0·20
Bichromate of potash	1·0786	0·101	0·109	0·89	0·97	−0·08	−0·73
Chromic acid..................	1·3535	0·341	0·470	0·31	0·88	−0·57	−1·21

Thus the two chromates of potash and chromic acid have a negative magneto-rotary power, and the absolute value of this power increases with the quantity of chromic acid. The negative power of the neutral chromate is feeble, but quite unmistakeable; that of the bichromate is considerably stronger, while that of chromic acid is comparable with the power of the protosalts of iron.

The experiment with the neutral chromate of potash is especially worthy of attention. We know, in fact, that while chromic acid and the bichromate of potash are magnetic, the neutral chromate is diamagnetic*, and it is rather surprising that the magneto-rotary power of a diamagnetic body should be negative. We shall meet with other analogous examples in the sequel.

The five metals whose compounds I have just considered have long been considered as magnetic by all physicists. This is not the case with those which follow, which have only been recognized as magnetic since the investigations of Faraday, and some of which indeed I have now examined for the first time. It has therefore been essential for me to verify the magnetic character of these bodies before studying the magneto-rotary power of their compounds. In this verification I have followed the rule laid down by Faraday, which consists in regarding as doubtful the magnetism of every metal whose apparent magnetism is only feeble, and which does not give rise to any magnetic compound, especially to no magnetic oxide. The quantity of a very magnetic metal (iron, nickel, or cobalt), the presence of which it is necessary to admit in order to account for the magnetism of a feebly magnetic body, is so small as to escape all analytical test, and against which no process of purification is a sufficient guarantee. The case is quite different with a salt or an oxide; for if we wish to explain the magnetism of such a body by the accidental presence of a salt, or oxide of iron, or some analogous compound, we should be obliged to assume the presence of a quantity sufficiently great to be easily detected by analysis, because the magnetism of such a salt or oxide is incomparably less than that of the corresponding metal. Hence it is that we see some metals, apparently feebly magnetic, give rise, on oxidation or solution, to diamagnetic compounds.

Titanium.—Mr. Faraday classed titanium among magnetic bodies. I have verified his observations with some pieces of pure titanium, with which I was furnished by M. Deville. The magnetism of titanium appeared to me to be greater than that of pure chromium, and too great to be attributed to impurities which had escaped analysis. Amongst the compounds of this

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* See memoir of Faraday referred to before.

metal, titanic acid is magnetic, and bichloride of titanium *diamagnetic**.

Nevertheless I examined the magneto-rotary power of bichloride of titanium, which is, as is well known, at ordinary temperatures a transparent colourless liquid. It was found to be *negative*, and greater in absolute power than the magneto-rotary power of water.

Two points are noticeable in this experiment. In the first place, it shows that the phænomena under consideration depend very little upon any chemical analogies which may subsist among the several bodies. Chemists generally consider titanium as the analogue of tin, and especially regard the chlorides of these two metals as bodies of entirely comparable nature. On the other hand, nothing is more dissimilar than the behaviour of the two bodies when they are placed between the poles of an electro-magnet, and examined with respect to their action on polarized light. In the second place, by virtue of the liquidity and transparence of the bichloride of titanium, its negative rotary power may be shown directly, without the intervention of a solvent; and thus an objection, which had been made to my former experiments, is removed. Some persons considered the magnetic action of certain iron solutions to be due to the fact that the molecules of the magnetic compound in solution, being magnetized by the electro-magnet, exercised a magnetic action contrary to that of the electro-magnet itself, upon the neighbouring atoms of the solvent. The experiments with solvents so varied as those which I employed, and exercising, by themselves, so inconsiderable an action, appeared greatly to invalidate this view; while my experiments upon chloride of titanium completely refute it, by showing the existence of a *diamagnetic* liquid whose rotary power is *negative*†.

Cerium.—The magnetism of cerium was discovered by Faraday, and is not more difficult to confirm than that of chromium or of manganese. As I had no metallic cerium at my disposal, I examined two perfectly pure salts of this metal, a sulphate and a chloride, which were prepared by MM. Damour and Deville in the course of an investigation on cerium and the metals which accompany it in its minerals‡. Both salts showed a strong

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* In the note presented to the Academy of Sciences, July 8, 1857, I declared myself unable to determine whether the bichloride of titanium were magnetic or diamagnetic. By employing, however, the ingenious process devised by M. Quet, for the study of the effect of magnetism on liquids, I have been enabled to solve the question.

† The objection in question was only made to me verbally, and has never been mentioned in any publication relating to the subject of my researches.

‡ MM. Damour and Deville had not determined the exact composition of these two salts at the time when they furnished me with them. They

magnetic action, and both in aqueous solution evinced a negative magneto-rotary power. The sulphate gave a tolerably transparent reddish solution, whose action on polarized light was markedly less than that of water. However, as I did not analyze this solution, I am only able to regard the negative character of the action which I attribute to it on polarized light as simply probable. With the chloride no doubt can exist. A concentrated solution of this salt, placed between the poles of an electro-magnet, exerts an action on polarized light almost equal in absolute value to that of water, and contrary in direction. The perfect limpidity of the solution makes the verification of these phænomena very easy.

Uranium.—Mr. Faraday allows some doubt to rest upon the position which uranium should occupy among the magnetic or diamagnetic metals. He found, in fact, that the protoxide was slightly magnetic, and the peroxide non-magnetic. The red and the black oxides of uranium, prepared by heating at a higher or lower temperature crystals of the nitrate purified by repeated crystallizations, are magnetic. This experiment is rendered quite conclusive from the fact that the nitrate of uranium itself is diamagnetic. The nitrate of uranium is the only compound of uranium whose magneto-rotatory power I have determined; and the result I obtained is remarkable. An aqueous solution of this body, under the influence of magnetism, exerts a less action on polarized light than the water which it contains, which leads us to regard the magneto-rotary power of the nitrate as negative. This conclusion is confirmed by the study of its solutions, which may be formed with alcohol and æther. The nitrate of uranium therefore furnishes us with a third example, to be placed by the side of the neutral chromate of potash and the bichloride of titanium*. The absolute value which my experiments show to belong to the negative action of the nitrate of uranium is, however, very small.

Lanthanium.—As far as I am aware, the magnetism of lanthanium has not hitherto been examined. Some perfectly pure carbonate of lanthanium, furnished me by M. Deville, was strongly magnetic. This is sufficient to show that lanthanium, like its analogue cerium, is to be classed among the magnetic metals. On treating the carbonate with pure hydrochloric acid, I obtained a solution whose magneto-rotary power was less than

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were only assured that the salts did not contain the slightest trace of any other metal besides cerium.

* The existence of three diamagnetic compounds whose magneto-rotary power is negative, clearly demonstrates what I before asserted, namely, that no simple relation exists between the diamagnetic capacity of bodies and their magneto-rotary power.

that of water. It is therefore probable that the magneto-rotary power of the salts of lanthanium is negative; but as I did not analyze the solution, this is not perfectly certain.

Molybdenum.—Some metallic molybdenum which was furnished me by M. Debray is magnetic; and as this property also exists in molybdic acid, purified by repeated distillations, it cannot be attributed to the presence of foreign substances. Molybdenum, therefore, must be added to the list of magnetic bodies. The soluble molybdates which I had at my disposal—the molybdates of soda and ammonia—are diamagnetic. Their magneto-rotary power is positive, but small.

Aluminium.—M. Deville has placed aluminium among the feebly magnetic metals. The analogy between aluminium and iron has caused this result to be regarded as very probable. Nevertheless I have not been able to find any compound of this metal which was not diamagnetic. Even alumina, when quite pure, is repelled by an electro-magnet. Having in fact obtained some perfectly pure and strongly diamagnetic nitrate of alumina from the École Normale, I got from it, by calcination, some anhydrous alumina which was also strongly diamagnetic. I determined, moreover, the magneto-rotary powers of alum, sulphate of alumina, chloride of aluminium, and of the double chloride of aluminium and sodium, and I found them positive. In the cases of the chloride and of the double chloride, the rotary forces were considerable.

Finally, I made use of the means offered me by the chemical collections of the École Normale and the Faculté des Sciences, for subjecting to magnetic action compounds of a certain number of rare metals, which have of late years been submitted to new and profound study. The subjects of these experiments were zirconium, glucinum, lithium and tungsten. All the pieces of these several metals, which were furnished me by MM. Debray, Troost, and Riche, with the exception of a piece of glucinum, appeared sensibly attracted by the poles of an electro-magnet. But all their pure compounds, especially their oxides and chlorides, are incontestably diamagnetic, and all their compounds, whose liquidity or solubility allowed of an examination of their optical properties, had a positive magneto-rotary power. Magnesium behaved in the same manner. A piece of distilled magnesium given me by M. Troost appeared magnetic; but pure magnesia, which was prepared by calcining pure and diamagnetic nitrate, was diamagnetic. The magneto-rotary power of all magnesian salts is negative.

From these experiments it seemed to me to be probable that aluminium, zirconium, lithium, magnesium, and tungsten are really diamagnetic. One can scarcely understand, in fact, how diamagnetic oxides can result from the union of magnetic metals with a magnetic gas, oxygen. However, we know so little as yet about the true cause of magnetism, that I do not consider myself entitled on this account alone to speak positively.

To recapitulate. All diamagnetic substances which do not contain any magnetic metal have a positive rotary power. This is not the case with substances containing magnetic metals; and according to all the observations which have hitherto been made, magnetic metals may be classed in three divisions, having for their types, iron, nickel, and manganese respectively. The magneto-rotary power of all compounds of iron is negative, with the exception of the ferrocyanides, in which, as is known, the properties of the iron are completely disguised. The magneto-rotary power of all compounds of nickel is positive. With iron are to be placed titanium, cerium, lanthanium, and probably also chromium and uranium. With nickel are to be placed cobalt and molybdenum. Manganese represents an intermediate type, the magneto-rotary powers of its compounds being sometimes positive, sometimes negative. It is possible that chromium and uranium belong rather to the manganese group than to that of iron.

Moreover, no relation seems to exist between the positive or negative magneto-rotary power and any property whatever of the metals. It is not the magnitude of the magnetic power which determines the separation of the magnetic metals into the three preceding classes; because iron and nickel, the most strongly magnetic of all the metals, are the types of two opposite classes. Chemical analogy is equally unavailable as a rule. If we see without surprise cobalt grouped with nickel, chromium with iron, lanthanium with cerium, and manganese serving as a transition between these two opposite classes, we must be astonished to see titanium and aluminium separated so completely from tin and iron respectively.

Another hypothesis concerning the anomalous nature of the phænomena was suggested to me by the old experiments of M. Plücker. It will be remembered that this physicist obtained mixtures of magnetic and diamagnetic bodies capable of being repelled by the poles of an electro-magnet of certain strength, and attracted by those of a weaker one. He thence inferred that magnetic attraction varied with the intensity of the electro-magnet, according to a law different from that of diamagnetism*. It occurred to me that something similar might occur with the magneto-rotary power; that, for instance, the magneto-rotary power of nickel salts might be positive with a certain strength of

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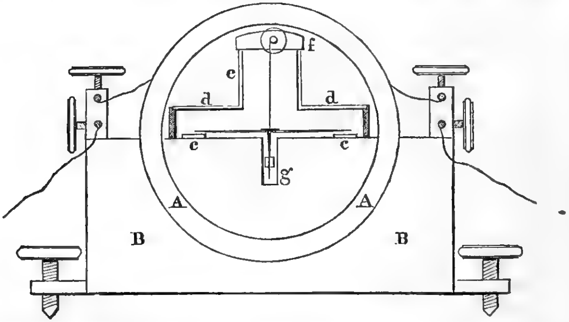
* M. Plücker subsequently recognized the inaccuracy of this interpretation of his experiments.

magnetic action, negative with a very different strength, and nothing for an intermediate one. Such might be the case without a change of sign occurring within the limits of ordinary magnetic forces; but it is highly probable that under these circumstances, and within the limits of experiment, the salts of nickel would at least be observed to depart sensibly from the general law of variation of magneto-rotary powers. In order to test this, I compared the rotation produced by a solution of sulphate of nickel with that produced by water under very various magnetic actions; and as I found the proportion between the two rotations absolutely constant, I was obliged to abandon my hypothesis.

LIII. *On an improved Galvanometer.*

By J. P. Joule, *LL.D., F.R.S. &c.**

THE important experiments required in carrying out the gigantic projects of electric telegraph engineers having rendered a delicate and portable galvanometer an essential piece of apparatus, I am induced to hope that the following description of one recently made from my design will interest at the present time. In the figure, A A represents the frame on which the wire forming the coil is wound. It is inserted in a semicircular groove cut in the block of wood B B. Another similar block, not shown in the figure, is fastened to the first by hasps, so as to hold the coil firmly in its place. *c c* shows the section of the graduated circle, enclosed in a box whose glass lid *d* is fitted with a glass chimney *e*, surmounted by a cap and roller *f*. Over this roller is thrown the filament of silk which supports the needle (a piece of magnetized sewing-needle a quarter of an inch



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* Communicated by the Author.

long), and the glass index to which the needle is attached. A small piece of fine copper wire attached to the needle hangs within a groove cut in the wooden block. By means of a hole pierced horizontally from the back of the instrument, entering about midway down the groove, a stud can be made to press the fine copper wire against the anterior wooden block (that not seen in the figure). By doing this the needle and pointer (previously let down by turning the roller so as to slacken the silk filament) are held securely. The instrument can then travel safely, and the experimenter is saved the otherwise inevitable trouble of suspending the needle afresh at the journey's end.

In the instrument I have got fitted for Mr. Gordon, 2798 yards of No. 40 silked copper wire are wound on a reel 4 inches in diameter, the object being to obtain an effect accurately measurable with a very small quantity of current. But coils of different lengths and sizes of wire can be readily attached to the instrument. Although the silk filament is only 1½ inch long, and the needle is little more than a quarter of an inch long, the torsion is so trifling that an entire twist of the filament deflects the needle only one degree from the magnetic meridian. The resistance presented by the air to the motion of the glass pointer, stops the oscillations of the needle in about half a dozen seconds.

LIV. *On the Assaying of Coals by the Blowpipe. By* E. J. Chapman, *Professor in University College, Toronto*[*].

THE blowpipe had been employed with great success for nearly a century in the examination of minerals and chemical products, with a view to distinguish these numerous bodies from one another, and also to ascertain their general composition, when Edward Harkort of Freiberg first applied it to the quantitative investigation of certain silver ores and furnace products. Plattner, who had worked with Harkort, subsequently extended this application to the assaying of various metallic substances, and added in no small degree to the utility of the instrument, by the invention of new methods of research, and many new appliances, published collectively in his well-known *Probirkunst mit dem Löthrohre*[†]. No one, however, has yet attempted to employ the blowpipe in the practical examination

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[*] Communicated by the Author.

[†] This work reached in 1853 its fourth edition. Harkort's earlier publication (1827), of which, however, merely the first part was issued, bore a similar title. For all that concerns the history and general application of the blowpipe, the reader may consult the fourth edition of the standard work by Berzelius, as translated by Whitney. A new edition of this work, incorporating the various tests and discoveries published since the death of its distinguished author, is much required.

of coals, an application peculiarly fitted to it; since, in travelling, and at other times when only the blowpipe apparatus can be conveniently made use of, determinations of the kind in question are often desirable. Having had some experience in the use of the instrument, I have attempted to supply this deficiency; and thinking the subject of sufficient interest to obtain a place in the pages of the Philosophical Magazine, I have embodied in the present paper the results of my investigations. The subject may be conveniently considered under the following heads:—(1) Coal in its different aspects; (2) Instruments and appliances; (3) Operations.

§ 1. *Varieties of Coal.*

Without attending to minor distinctions or points of merely local value, we may arrange all varieties of coal, so far as regards practical purposes, under the following subdivisions:—

1. Anthracites.
2. Anthracitic or Dry Coals.
3. Caking or Fat Coals.
4. Cannel or Gas Coals.
5. Brown Coals or Lignites.

These varieties pass by almost insensible transitions into one another. Thus, the cannel coals are related to the lignites by the different kinds of jet, some of which are referable to the one and some to the other subdivision. Between the caking and the cannel coals there are also various links; whilst the anthracitic or dry coals, on the other hand,—passing by excess of bitumen into the caking coals, and by a diminution of bituminous matter into the anthracites—serve to connect the first and third divisions. The typical or normal specimens of each of these five varieties, however, are sufficiently well marked.

1. *Anthracites.*—The true or normal anthracites possess a brilliant sub-metallic lustre, a degree of hardness varying from 3·0 to 3·25*, and a specific gravity of at least 1·33. A specimen from Pennsylvania gave 1·51; another specimen, 1·44; one from the department of the Isère in France, 1·56; and three from Wales yielded respectively 1·33, 1·37, 1·34. It should be stated, however, that many of the Welsh specimens belong strictly to the division of anthracitic coals, rather than to that of the true anthracites. The normal anthracites exhibit also a black or greyish-black streak; and all are good conductors of electricity. The latter character may be conveniently shown by

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* Hausmann in his *Handbuch der Mineralogie*, gives 2·5 as the extreme hardness of all coals; but this is evidently erroneous, as many specimens, not only of anthracite, but of common and cannel coals, scratch calcareous spar.

the method first pointed out by Von Kobell. A fragment placed in a solution of sulphate of copper (blue vitriol) in contact with a strip of zinc will become quickly coated with a deposit of metallic copper, a phænomenon not exhibited in the case of common coal. Deducting ash and moisture, true anthracites present, as a mean, the following composition:—Carbon, 92½; Hydrogen, 3½; Oxygen (with trace of nitrogen), 4. All yield an amount of coke equal to or exceeding 89 per cent. The coke is frequently pulverulent, never agglutinated.

The comportment of anthracite before the blowpipe has not hitherto been given in detail. It is as follows:—*per se*, the assay quickly loses its metallic brilliancy. After continued ignition, small white specks of ash appear on its edges. In borax it dissolves very slowly, with constant escape of bubbles. It is not attacked by salt of phosphorus; the assay works to the top of the bead and slowly burns away. In carbonate of soda it effervesces, scintillates, and turns rapidly in the bead, and the soda is gradually absorbed. In the bulb-tube a little water is always given off, but without any trace of bituminous matter.

As regards their geological position, the true anthracites belong chiefly to the middle portion of the palæozoic series, below the carboniferous formation; or otherwise they constitute the under portion of the coal-measures. Frequently also anthracites occur in the vicinity of erupted rocks, and amongst metamorphic strata, as manifest alterations of ordinary coal.

2. *Anthracitic Coals.*—These are often confounded with the true anthracites, into which indeed, as already stated, they gradually merge. Normally they differ from the true anthracites in being non-conductors of electricity, in burning more easily and with a very evident yellow flame, in yielding a small quantity of bituminous matter when heated in a tube closed at one end, and in furnishing an amount of coke below 80 per cent. The coke is also in general more or less agglutinated, although it never presents the fused, mammillated appearance of that obtained from caking coal. The mean composition (ash and moisture deducted) may be represented as follows:—Carbon, 89½; Hydrogen, 5; Oxygen (with trace of nitrogen), 5½: or Carbon, 89; Hydrogen, 5; Oxygen (with trace of nitrogen), 6.

3. *Caking Coals.*—These are often termed technically "fat coals." They constitute the type-series of the coals, properly so called. All yield a fused and mammillated coke, varying in amount from 65 to 70 per cent. Spec. grav. = 1·27 to 1·32. Commonly mixed with thin layers of strongly soiling "mineral charcoal" or fibrous anthracite. Mean composition (ash and moisture excluded):—Carbon, 87·9; Hydrogen, 5·1; Oxygen (with nitrogen), 7·0.

4. *Cannel or Gas Coals.*—These coals, at least in normal specimens, do not fuse or "cake" in the fire. They give off a large amount of volatile matter, frequently more than half their weight; hence their popular name of "gas coals." They soil very slightly, or not at all. The coke obtained from them is sometimes fritted, and partially agglutinated, but never fused into globular, mammillated masses, like that obtained from the caking coals. It varies in amount from 30 to 60, or, in typical specimens, from 55 to 58 per cent. Mean composition (normal cannel):—Carbon, 80 to 85; Hydrogen, 5·5; Oxygen (with nitrogen), 9 to 12·5.

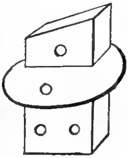
5. *Lignites or Brown Coals.*—These coals, of tertiary age, differ greatly from one another in external aspect. Some of the so-called jets, passing into the cannel coals, are black, lustrous, and non-soiling; whilst other varieties are brown, and of a ligniform or stratified structure, or, otherwise, earthy and loosely coherent. All, however, are partially soluble in caustic potash, communicating to it a dark brown colour. The coke—usually of a dull charcoal-like aspect, or in sharp-edged fragments retaining their original form—varies from 25 to 50 per cent. Its separate fragments are rarely agglutinated, except in the case of certain varieties (as the lignites of Cuba, and those from the freshwater deposits of the Basses Alpes in France) which contain asphaltum. All the typical varieties of lignite, as pointed out by Cordier, continue to burn for some time, in the manner of "braise," or ignited wood, after the cessation of the flame occasioned by the combustion of their more volatile constituents; whereas with ordinary coal, ignition ceases on the flame going out. The mean composition of lignite may be represented by—Carbon, 67 to 75; Hydrogen, 5; Oxygen (with nitrogen), 20 to 30.

All the different kinds of coal enumerated above contain a variable amount of moisture, and of inorganic matter or "ash." The moisture rarely exceeds 3 or 4 per cent., although in some samples of coal it is as high as 6 or 7, and even reaches 15 or 20 per cent. in certain lignites. The amount of ash is also necessarily a variable element. In good coals it is under 5, frequently indeed under 2 per cent. On the other hand, it sometimes exceeds 8 or 10, and in bad samples even 15 or 20 per cent. The ash may be either argillaceous, argillo-ferruginous, calcareous, or calcareo-ferruginous. The ferruginous ashes are always more or less red or tawny in colour, from the presence of sesquioxide of iron, derived from the iron pyrites (FeS^2) originally present in the coal. If much pyrites be present, the coal is not available for furnace operations, gas making, engine use, &c., owing to the injurious effects of the disengaged sulphur. Calcareous ashes are more common in secondary and tertiary coals than in those of the palæozoic age. For methods of ascertaining the nature and amount of ash, pyrites, &c., see under § 3 below.

§ 2. *Instruments and Appliances.*

The instruments, &c. employed in these examinations are the following:—a blowpipe, blowpipe-lamp, and small spirit-lamp of the ordinary construction; together with the usual accessory instruments and reagents which always accompany the blowpipe. These require no special description. The blowpipe may be of any form; but for the purpose of heating small platinum vessels in these experiments, it is convenient (although not absolutely necessary) to add to it an extra jet with an orifice rather larger than usual. The blowpipe-lamp should also be furnished with a broad wick-holder of the pattern recommended by Plattner (fig. 1), in place of the flat wick-holders formerly in use. In heating crucibles it is advisable to turn the wick-holder so as to make the upper surface slope towards the right-hand instead of towards the left, the flame being then directed upwards, against and around the bottom of the crucible. Or, to avoid the trouble of changing the position of the wick-holder, the operator may turn the lamp itself, placing it with its fore part away from him.

Fig. 1.



In addition to the above, the following appliances, of more special use, are also needed:—(1) Two platinum capsules as thin as they can be obtained. One about half an inch in diameter, provided with a small ear or handle; and the other of somewhat smaller dimensions, and without any projecting ear. The smaller capsule, reversed, fits into the larger one, the two then constituting a closed vessel. Those which I employ, weigh together less than 42 grains. (2) A small platinum crucible with a lid. I make use of two small but deep platinum spoons; one with, and the other without a handle. The latter must be the larger of the two. Its edges must be bent slightly inwards so as to allow it when reversed to be fitted closely over the smaller spoon, and thus to constitute a lid. The long handle of the spoon crucible should be bent as shown in fig. 2. The object of this is to enable the crucible to retain an upright position when placed on the pan of the balance. The figure shows the exact size and form of the crucible employed by me. Its weight is a little under 36 grains. In some spoons a slight notch must be cut in the lid to admit the passing of the handle. (3) A pair of steel tongs or forceps to hold the platinum vessel during ignition. These tongs should be so constructed as to remain closed except

Fig. 2.

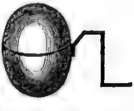
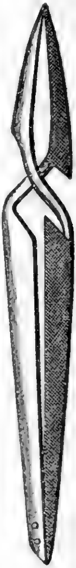


Fig. 3.



when subjected to the pressure of the fingers. I give a figure of the kind that I employ, because it is much more convenient than the forceps commonly put up in blowpipe cases; or indeed, than any that I have found described in works on the blowpipe, or in the catalogues of the instrument makers. In using these tongs, the left-hand need only be employed. They open by the pressure of the forefinger and thumb upon their sides. (4) A balance. The most convenient kind of balance for use in these operations is that first contrived by Lingke of Freiberg, for Plattner's assaying experiments. It is figured and described in detail in the fourth edition of Plattner's *Probirkunst mit dem Löthrohre.* This balance takes to pieces, and packs with its weights, forceps, &c., into receptacles cut for it in a small box of peartree wood, the size of a thin octavo volume. It can be fitted up ready for use in the course of a few minutes; and its delicacy is very great. That which I employ, when loaded with 3 grammes, a greater weight than it is ever required to carry, turns readily with less than half a milligramme, or the 0·0077th of a grain. It is convenient to have counterpoises for the platinum vessels described above, as the weights belonging to the balance only range from a gramme downwards. A small platinum capsule forms the best kind of counterpoise. It can be trimmed down by a knife or pair of scissors, until brought, after repeated trials, to the proper weight. In spare places in the box containing the balance that I use, I have cut out receptacles for the two platinum vessels and their counterpoises, and I recommend other operators to do the same, because these platinum vessels are of frequent use in various experiments unconnected with the present inquiry; as in ascertaining the amount of water in minerals, and so forth.

§ 3. *Operations.*

In the examination of coals, the following operations are necessary:—(1) The estimation of the water or hygrometric moisture present in the coal; (2) the estimation and examination of the coke yielded by the coal; (3) the estimation and examination of the ash or inorganic matters present in the coal; and (4) the estimation of the sulphur, chiefly contained in the coal as $Fe\,S^2$.

To these might be added, the determination of the heating powers of the coal; but this operation, at all times one of more or less uncertainty, cannot be performed by the blowpipe in a satisfactory manner. This, however, is really of little consequence, as apart from the doubtful character of the experiment even when conducted on the large scale, the relative heating powers of different samples of coal may generally be estimated sufficiently near for practical purposes by a comparison of the amount of coke, ash, and moisture. The litharge test commonly resorted to for the determination of the calorific power of coals, when properly considered, is of very little actual value. Take, for example, the respective results furnished by good wood-charcoal and ordinary coke. These results are closely alike, or rather in favour of the charcoal; and yet experience abundantly proves the stronger heating powers of the coke. It is impossible to raise the temperature of a furnace with charcoal to anything like the same degree as that obtainable by the employment of coke. Besides which, in practice it is not, as a general rule, the absolute calorific powers of a coal that constitute its availability for ordinary operations, because a coal—such, for instance, as a brown coal rich in bitumen—may possess heating powers of considerable amount, but only of short duration; and in cases of this kind the litharge test becomes again unsatisfactory. Thus the lignites of the department of the Basses Alpes, already alluded to, yield with litharge from 25 to 26 of lead; whilst many caking coals, practically of much higher heating powers, yield scarcely a greater amount. For these reasons, whilst seeking to discover a satisfactory method of ascertaining directly by the blowpipe the heating power of coals, I leave the subject out of consideration in the present paper.

Estimation of Moisture.—This operation is one of extreme simplicity. Some slight care, however, is required to prevent other volatile matters from being driven off during the expulsion of the hygrometric moisture. Seven or eight small particles, weighing together from 100 to 150 milligrammes, are to be detached from the assay specimen by means of the cutting pliers, and carefully weighed. They are then to be transferred to a porcelain capsule with thick bottom, and strongly heated for four or five minutes on the support attached to the blowpipe-lamp, the unaided flame of the lamp being alone employed for this purpose. It is advisable to place in the capsule, at the same time, a small strip of filtering or white blotting-paper, the charring of which will give indications of the temperature becoming too high. The coal, whilst still warm, is then to be transferred to the little brass capsule in which the weighings are performed, and its weight ascertained. In transferring the coal from one vessel to the other, the larger pieces should be removed by a pair of fine brass forceps, and the little particles or dust afterwards swept into the weighing capsule by means of the camel's-hair pencil or small colour-brush belonging to the balance-case. The weighing capsule should also be placed in the centre of a half-sheet of glazed writing-paper, to prevent the risk of any accidental loss during the transference. After the weighing, the operation must always be repeated, to ensure that no further loss of weight occur. In place of the blowpipe-lamp, the spirit-lamp may be employed for this operation; but with the former there is less danger of the heat becoming too high. By holding a slip of glass for an instant, every now and then, over the capsule, it will soon be seen when the moisture ceases to be given off. It should be remarked, that some anthracites decrepitate slightly when thus treated, in which case the porcelain capsule must be covered with a small watch-glass.

Estimation, &c. of Coke.—In this operation the small crucible is employed. Particles are detached from the assay specimen as before, by the cutting pliers, and about 100 or 150 milligrammes taken for the experiment. The weighing is performed in the crucible itself, this being placed in the little weighing-capsule, with its handle-support projecting over the side. The crucible, with its cover on, is then brought gradually before the blowpipe to a red heat. The escaping gases will take fire and burn for a few seconds on the outside of the vessel, and a small amount of carbonaceous matter may be deposited upon the cover. This, however, rapidly burns off on the heat being continued; and as soon as it disappears, the crucible is to be withdrawn from the flame, cooled quickly, and weighed always with its cover on. The loss, minus the weight of moisture as ascertained in a previous experiment, gives the amount of volatile or gaseous matter. The residue is the coke and its contained ash. The coke should be examined by a magnifying glass, and its general aspect and characters noted down. As already explained, some coals yield a swollen, semi-fused, and agglutinated coke, with a mammillated surface and metalloidal aspect. Others produce a slightly fritted and partially agglutinated coke; others again, an unfused coke retaining the form of the coal fragments subjected to the assay; others a pulverulent, or a strongly-soiling coke, and so on. It is sometimes desirable to take the specific gravity of the coke.

Estimation of Ash.—The platinum capsule is employed for this operation. The coal must be reduced to a coarse powder, and about 150 milligrammes weighed out for the experiment. The weighing may be effected in the platinum capsule in which the experiment is to be performed. The weight ascertained, the platinum capsule is to be fixed in an inclined position above the spirit-lamp, and heated as strongly as possible. If the wick of the spirit-lamp be pulled up sufficiently, and a very thin capsule, as already directed, be employed, a temperature sufficiently high to burn off the carbon from most coals is in this manner attainable. The lid of the capsule must be placed above the coal-powder until combustion cease, that is to say, until the gaseous products be driven off, and only the uninflammable carbon and ash remain; as, otherwise, a portion of the powder might very easily be lost. Some of the anthracites also decrepitate on the first application of the flame, although rarely perhaps when the coal is in the form of powder; still it is advisable in all cases to keep the assay covered until the flame cease. During the after-combustion, the powder or small particles must be gently stirred and carefully turned over, and if agglutinated, broken down by a light steel spatula, or better still, by a small spatula of platinum, made by inserting a strip of stout platinum foil (an inch long) into one of the ivory or wooden handles intended to hold platinum spoons. These handles are quite useless for the latter purpose, or at least are far inferior to the steel forceps described above. With the forceps, for example, the spoons can be taken up and disengaged in an instant, and without the intervention of the right hand. Whilst the spoons, also, are still red-hot, the forceps may be laid down without the spoons coming in contact with the table. Figure 4 shows the form and size of the spatula that I employ. A is the ivory handle; C the piece of stout platinum foil fitting into a slit in A; and B the metal ring which keeps the two together. The platinum, it should be remarked, must be sufficiently stout to resist bending; and its point must be kept quite bright and smooth by occasional polishing on a smooth part of the agate mortar which always accompanies the blowpipe. If by the method of procedure just described, the carbonaceous matter be not finally burnt off, the flame of the blowpipe—using the oil-lamp, or spirit-lamp with the wick well up—may be employed to accelerate the process. The operator, however, must be careful to keep the capsule inclined away from the flame, in order to avoid the loss of any portion of the fine light ash. Finally, when the ash ceases to exhibit in any of its parts a black colour, the lid of the capsule is to be cautiously replaced, and the whole cooled and weighed[*].

Fig. 4.

Nature of the Ash.—As already remarked, the ash or inorganic

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[*] If the ash be very ferruginous, the results thus obtained, to be exact, will require correction: the original iron pyrites of the coal being weighed as sesquioxide of iron. In ordinary cases, however,—in assays as distinguished from analyses,—this may be fairly neglected.

When, also, the ash is calcareous, and in considerable quantity, it should portion of the coal may be either argillaceous—consisting, in that case, esentially of a subsilicate of alumina—or calcareous; and in either case ferruginous also. If free from iron, the ash will be white or pale grey; but if iron be present, it will exhibit a yellowish, brown, or red colour, according to the amount of iron contained in it. The iron is, of course, in the state of sesquioxide, derived, except perhaps in a few rare instances, entirely from the iron pyrites or bisulphide of iron originally present in the coal. I have found, from numerous trials, that the well-known salt of phosphorus test, so useful in general cases for the detection of siliceous compounds, cannot be safely resorted to for the purpose of distinguishing the nature of the coal ash obtained in these experiments. This is owing to the small quantity of the ash, and to the extremely fine state of division in which it is obtained. Argillaceous ashes dissolve in salt of phosphorus with as much facility as those of a calcareous nature, and without producing the characteristic silica skeleton, or causing the opalization of the glass. With calcareous ashes also, the amount obtained is never sufficient to saturate even an exceedingly minute bead of borax or salt of phosphorus, and hence no opacity is obtained by the flaming process. The one kind of ash may be distinguished, however, from the other, by moistening it, and placing the moistened mass on a piece of reddened litmus paper. Calcareous ashes always contain a certain amount of caustic lime, and thus restore the blue colour of the paper. These calcareous ashes also sometimes following contain sulphate of lime*. For the detection of the latter, the well-known test may be resorted to. The ash is to be fused with carbonate of soda and a little borax on charcoal in a reducing flame, and the fused mass thus obtained is to be moistened and placed on a bright silver coin, or on a piece of glazed card; when, if sulphate of lime were present in the ash, a brown or black stain will be produced by the formation of sulphide of silver or of lead. In testing earthy sulphates generally by this process, a little borax should always be added to the carbonate of soda, in order to promote the solution of the assay, and the more ready formation of an alkaline sulphide. If oxide of manganese be present in the ash, by fusion with carbonate of soda and a little borax, we obtain the well known bluish-green manganate of soda technically termed a turquoise-enamel.

Estimation of Sulphur.—The method of detecting the presence of sulphur in coal, is the same as that just pointed out for the detection of sulphate of lime in the ash. The actual estimation

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be moistened with a drop of a solution of carbonate of ammonia, and gently re-heated previous to weighing.

* The ashes of a lignite from Grosspreisen yielded Erdmann,—Carbonate of lime, 30·93; sulphate of lime, 36·42; lime, 17·22; sesquioxide of iron, 20·67; alumina, 1·23; soda, 1·86; potash, 1·67.

of the sulphur is a much more troublesome operation. A process given by Berthier, in his *Traité des Essais par la voie sèche,* consists in boiling the ferruginous ash in hydrochloric acid, which dissolves out the sesquioxide of iron, and then calculating the sulphur from the loss. One hundred parts, for example, of sesquioxide of iron correspond to 70·03 of metallic iron; and hence to 150·24 of iron pyrites, or to 80·21 of sulphur. But this method, besides requiring a larger quantity of ash than can be conveniently prepared in these blowpipe examinations, exacts that the other portion of the ash be not attackable by the acid,—a condition which of course does not obtain in the case of calcareous ashes. For this reason, the process recommended by Rose and other chemists is preferable, although somewhat beyond the range of blowpipe examinations. About 200 milligrammes of the coal in fine powder are to be intimately mixed with 8 parts of nitrate of potash, 4 of carbonate of potash, and 16 of common salt, and the mixture fused in a platinum crucible over the spirit-lamp, with the wick well pulled up, or, better still, over a double-current or Berzelius's lamp. The fused mass is then to be dissolved out in boiling water to which a few drops of hydrochloric acid have been added, and the sulphuric acid thrown down by chloride of barium. By dividing the precipitate thus obtained (after filtration, careful washing, and ignition) by 7·25, we get the amount of sulphur.

As the above process, although simple enough in the performance, is scarcely available when the operator is away from home, I have attempted to hit upon a more ready method, and one more properly within the legitimate pale of blowpipe experimentation, of ascertaining approximatively the amount of sulphur in coal samples. After various trials, I have found the following process sufficiently exact for all ordinary cases, because, as a general rule, we merely require to know here, if the coal under examination be slightly, moderately, or highly sulphurous. It consists essentially in comparing the intensity of the stain produced on silver foil by an alkaline sulphide of known composition with that formed by an alkaline sulphide obtained from the assay-coal. For this purpose, mixtures must first be made of a coal free from sulphur, with such proportions of iron pyrites as correspond respectively to a per-centage of 2, 4, 6, 8, and 10 parts of sulphur. These proportions are the following:—Coal, 96·26; pyrites, 3·74 = sulphur 2 per cent. Coal, 92·50; pyrites, 7·50 = sulphur 4 per cent. Coal, 88·76; pyrites, 11·24 = sulphur 6 per cent. Coal, 85; pyrites, 15 = sulphur 8 per cent. Coal, 81·27; pyrites, 18·73 = sulphur 10 per cent. Separate portions of each of these mixtures are to be fused in a platinum spoon with three parts of a mixture of five parts of carbonate of soda with one part of borax (mixed beforehand, and kept for these experiments in a receptacle of its own); and the fused mass is then to be dissolved out in a measured quantity of water. A single drop of the solution is afterwards to be placed on a piece of silver foil (formed for example by beating out a small coin), and suffered to remain upon it for thirty seconds. The silver, wiped dry, is finally to be marked on the back with the per-centage of sulphur —2, 4, &c.—contained in the prepared coal. When employing this method for the estimation of sulphur, the coal under examination is to be treated in an exactly similar manner, and the stain produced by it on a piece of clean foil, compared with the test stains on the separate silver plates.

Finally, when the iron pyrites in the coal is not in a state of semi-decomposition, the amount of pyrites, and consequently the amount of sulphur, may be arrived at far more nearly than might at first thought be supposed, by the simple process of washing in the agate mortar. Each single part of pyrites, it will be remembered, corresponds to 0·53 of sulphur. A large piece of the assay coal should be taken, and broken up into powder, and a couple of trials should be made on separate portions of this. About 500 milligrammes may be taken for each trial, and washed in three or four portions. In the hands of one accustomed to the use of the mortar in reducing experiments, the results, owing to the lightness of the coal particles, and the consequent ease with which they are floated off, come out surprisingly near to the truth. In travelling, we may dispense with the washing bottle, by employing, in its place, a piece of straight tubing drawn out abruptly to a point. This is to be filled by suction, and the water expelled with the necessary force by blowing down the tube. A tube 6 inches long and the fourth of an inch in diameter will hold more than a sufficient quantity of water to be used between the separate grindings. The mortar should be very slightly inclined, and the stream of water must not be too strong; otherwise, and especially if the coal be ground up too fine, portions of the iron pyrites may be lost. The proper manipulation, however, is easily acquired by a little practice.

LV. *On the Evidence which the Observed Motions of the Solar Spots offer for the Existence of an Atmosphere surrounding the Sun. By* R. C. Carrington, *Esq*[*].

THE protuberances observed around the sun's periphery during total solar eclipses, and the excess of light and heat radiated from the centre of the disc as compared with the parts

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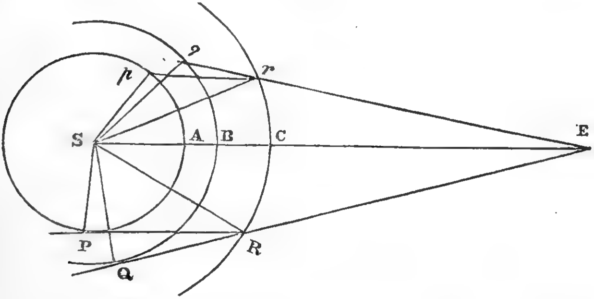
[*] From the *Monthly Notices of the Royal Astronomical Society*, April 9, 1858.

near the circumference, tend so strongly to force upon us the conviction of the existence of an atmosphere of no inconsiderable amount, that it is of some importance to inquire whether the motions of the spots, in passing over the disc, present any favourable or unfavourable indications.

The inquiry presents difficulties of two sorts, arising, first, from the little suitability of solar spots in general as data in any matter of delicacy, from their constant liability to change of form; and secondly, from our entire ignorance of the law of density, which should be assumed for the supposed atmosphere under the peculiar conditions of temperature to which it must be subject. The first of these difficulties we must surmount as well as we can by selection of the spots for discussion: the second, I shall make free to evade on the present occasion by the old assumption of homogeneity, proposing only to inquire what sort of result will be yielded by that hypothesis, which in the case of the earth's atmosphere is well known to afford a close representation of the observed actual effects on a ray of light.



Let P A p be a central section of the sun's actual photosphere by a plane passing through E, the centre of the earth. Let R C r be the corresponding section of the atmosphere, supposed homogeneous. Let $SP = r$; $SR = r(1+h)$; refractive index $= m$. If a ray of light from the sun's actual limb reach the earth by the path P R E, it will appear to have come from the limb of a concentric sphere (the apparent photosphere), whose section is Q B q, and whose radius $QS = m \cdot PS$; since $\sin QRS = m \cdot \sin PRS$. A spot at p on the actual photosphere will appear in the direction E r q, or will be seen at q on the apparent photosphere, under an apparent angle B S q from the centre of the disc, instead of A S p the true. The position-angles of spots will be unaffected.

$$\text{Let } \rho = q\text{SE}; \quad \rho' = \text{SE}q = m\,.\,(\text{R})\,.\,\sin(\rho+\rho')$$

$$= 16' \times \sin\rho, \text{ nearly}$$

$$\sin \text{S}rq = \frac{\text{S}q}{\text{S}r}\,.\,\sin \text{S}qr = \frac{m}{1+h}\,.\,\sin(\rho+\rho')$$

$$\sin \text{S}rp = \frac{1}{m}\,.\,\sin \text{S}rq = \frac{1}{1+h}\,.\,\sin(\rho+\rho')$$

$$\sin \text{S}pr = (1+h)\,.\,\sin \text{S}rp = \sin \text{S}qr$$

$$\therefore\ p\text{S}q = prq; \text{ and } \text{AS}p = \rho + \text{S}rq - \text{S}rp.$$

Let $\text{AS}p - \rho$ be denoted by (ρ),

$$(\rho) = \sin^{-1}\frac{m}{1+h}\,.\,\sin(\rho+\rho') - \sin^{-1}\frac{1}{1+h}\,.\,\sin(\rho+\rho')\,.\,.\ (1)$$

If m be but little greater than unity, this expression may be written with advantage in a different form.

$$\text{Let } a = \frac{m}{1+h}\,.\,\sin(\rho+\rho') = \frac{1}{1+h}\sin(\rho+\rho') + \frac{m-1}{1+h}\,.\,\sin(\rho+\rho')$$

$$= b + (m-1)\,.\,b.$$

Since $\sin^{-1} a - \sin^{-1} b = \sin^{-1}(a\sqrt{1-b^2} - b\sqrt{1-a^2})$, we find, after some reductions, neglecting powers of $(m-1)$ above the second,

$$(\rho) = \sin^{-1}\,.\,\left\{(m-1)\,.\,\frac{b}{(1-b^2)^{\frac{1}{2}}} + \frac{1}{2}\,.\,(m-1)^2\,.\,\frac{b^3}{(1-b^2)^{\frac{3}{2}}}\right\}.$$

Accordingly, if $\sin\tau = \frac{1}{1+h}\,.\,\sin(\rho+\rho')$,

$$\sin(\rho) = (m-1)\,.\,\tan\tau + \frac{1}{2}(m-1)^2\,.\,\tan^3\tau, \text{ very nearly.}\quad .\ (2)$$

The atmosphere speculated on will bear a bold assumption, and we will accordingly assume its height $h = \frac{1}{4}$. sun's radius, and compute the values of (ρ) for the three following values of m . . . 1·005, 1·010, and 1·020; corresponding roughly to the refractive index of common air, of which the density has been increased respectively 17, 34, and 69 times. Either of the formulæ above given may be used for the purpose. The first is the more readily worked, and has been used in forming the following Table.

TABLE I. ($h=\frac{1}{4}$).

ρ.	1·005.	1·010.	1·020.	$10' . \frac{\tan \tau}{\sin \rho}$.
0°	0′·0	0′·0	0′·0	8′·0
10	+2·4	+4·9	+9·7	8·1
20	4·9	9·8	19·6	8·3
30	7·5	15·0	30·2	8·7
40	10·3	20·7	41·5	9·3
50	13·4	26·9	53·9	10·1
60	16·7	33·4	67·0	11·1
70	19·8	39·6	79·8	12·1
80	22·2	44·4	89·6	13·0
90	+23·1	+46·2	+93·4	13·3

As a first series of observations for discussion, I have selected that of a small and well-defined spot observed in August and September 1854, under favourable circumstances, which in both its passages over the disc passed very near to the centre, and which, from August 8 to August 20, exhibited no marked deviation of its nucleus from a circular form. On its second appearance, it formed one of a small group; and after September 10 ceased to be distinguishable. In the Table which follows are given in the first column the fractions of the days of observation (in civil reckoning); in the second and third, p the angles of position and ρ the angles from centre of apparent photosphere; in the fourth and fifth l and λ the heliographical longitudes (from node) and latitudes, deduced from the elements (for 1854·0) N=74° 30′, I=7° 10′, as adopted provisionally at Redhill.

From comparison of the longitudes of the two passages at and about the dates at which the spot passed the centre, it was found that 25^{d}·240 was the period of sidereal rotation for this spot. This value has accordingly been used in computing the dates in the sixth and last column, which are those of the spot's passing the meridian through node given by each day's observation.

1854.	p.	ρ.	l.	λ.	t.
Aug. 8·525	98° 23′	77° 48′	162° 30′	N 6° 32′	22^{d}·372
10·504	101 8	51 43	190 43	6 33	·374
11·537	102 40	37 54	205 39	6 25	·359
13·525	107 19	11 35	234 4	6 3	·354
14·494	265 46	1 16	247 51	6 9	·357
16·574	286 27	28 27	277 16	5 48	·374
18·600	289 41	55 16	306 15	5 50	·369
19·470	290 58	66 30	318 25	5 50	·385
20·502	292 33	79 41	333 44	5 49	·344
Sept. 4·525	107 44	77 52	188 40	N 5 39	22·297
6·519	110 31	52 30	216 11	5 54	·362
7·513	111 43	39 34	230 11	6 8	·374
10·531	244 43	0 45	273 28	6 36	·358

As the parallel on which this spot was situated passed nearly through the centre of the disc, and its latitude was small, the differences of heliographical longitude of the spot and the centre of the sun's disc were on each day nearly equal to ρ, and were amenable to sensibly the same corrections for solar refraction. The heliographical longitudes will accordingly receive the same corrections, and the times t the equivalents in time with opposite signs. Taking $t=$Aug. 22·365 as the concluded mean, the next Table exhibits first the uncorrected discordances of t; and next, these discordances corrected on three suppositions indicated.

Disc^{s}.	$m=1·0025$		$m=1·0050.$		$m=1·0100.$	
d	d	d	d	d	d	d
+·007	+·012	+·019	+·025	+·032	+·050	+·057
+·009	+·008	+·017	+·016	+·025	+·033	+·042
+·006	+·006	·000	+·012	+·006	+·024	+·018
−·011	+·002	−·009	+·004	−·007	+·007	−·004
−·008	·000	−·008	·000	−·008	−·001	−·009
+·009	−·004	+·005	−·008	+·001	−·016	−·007
+·004	−·009	−·005	−·017	−·013	−·035	−·031
+·020	−·011	+·009	−·022	−·002	−·045	−·025
−·021	−·013	−·034	−·026	−·047	−·052	−·073
−·068	+·012	−·056	+·025	−·043	+·050	−·018
−·003	+·008	+·005	+·017	+·014	+·033	+·030
+·009	+·006	+·015	+·012	+·021	+·025	+·034
−·007	·000	−·007	·000	−·007	·000	−·007
±·182		±·189		±·226		±·355

The first hypothesis leaves the resulting sum of discordances much as it was before correction, but the second and third sensibly increase it, in other words, do harm instead of good. The sums on the three hypotheses, however, indicate that a smaller value of m would probably give a better result. Treating the discordances by equations of condition and the method of least squares, we find $m=1·002003$ as the most probable value, the sum of the discordances being in that case reduced to ±176. The observations accordingly afford very slender evidence *in favour* of an atmosphere for which $h=\frac{1}{4}$, $m=1·0020$, but will not bear one for which m much exceeds 1·005.

We will now take a different case, in which no assumption of elements of the plane of rotation need be made. If two spots are passing over the disc together, and solar refraction be insensible, the distance between their centres in arc of a great circle should be constant from day to day, or affected only by the independent motion of the spots. On the other hand, if the observed angles are affected by refraction, the mutual distance will appear to vary. The independent motions of spots being notoriously less in latitude than in longitude, whether arising from change of form or translation, it appears at first sight best to select a pair not differing much in longitude, and differing widely in latitude. I find such a series recorded in my books for June 22, 1854, and the five subsequent days, after which the southern spot underwent such a change of form as renders the remaining positions unfit for our present purpose.

If ρ_1, ρ_2 be the observed distances from centre, and p_1, p_2 the observed position-angles, the mutual distances ϕ result from the well-known expression

$$\cos \phi = \cos \rho_1 \,.\, \cos \rho_2 + \sin \rho_1 \,.\, \sin \rho_2 \,.\, \cos (p_1 - p_2).$$

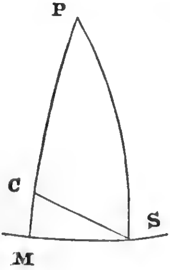
The following Table gives these observed and computed angles, uncorrected for solar refraction.

1854.	ρ_1.	ρ_2.	$p_1 - p_2$.	ϕ.	Disc^s.
June 22	54° 20′	67° 42′	28° 5′	27° 49′	− 3′
23	42 2	55 46	32 18	27 39	−13
24	29 20	43 37	41 26	27 49	− 3
25	16 51	32 14	62 38	28 17	+25
26	8 13	22 24	125 35	27 57	+ 5
27	13 52	17 17	125 20	27 39	−13

The next Table gives the same angles corrected for solar refraction on the hypothesis $h=\frac{1}{4}$, $m=1{\cdot}010$.

1854.	ρ_1.	ρ_2.	$p_1 - p_2$.	ϕ.	Disc^s.
June 22	54° 50′	68° 20′	28° 5′	28° 0′	− 4′
23	42 24	56 17	32 18	27 52	−12
24	29 35	44 0	41 26	28 2	− 2
25	16 59	32 30	62 38	28 31	+27
26	8 17	22 35	125 35	28 10	+ 6
27	13 59	17 25	125 20	27 52	−12

On comparing the individual discordances in the two tables, we here perceive that correction for refraction affects the angles ϕ by an almost constant amount, and that, consequently, the case taken for discussion is less suitable than it appeared at first sight, and is, in fact, of little or no use for the purpose. Still I have retained it in this paper, from its informing us of one important circumstance, that the latitudes of spots receive nearly constant corrections for solar refraction, and consequently that elements of the plane of the sun's rotation deduced from uncorrected angles will be sensibly unaffected with error.



Let us see how this arises. If P be the sun's pole, C the apparent centre of the disc, S a spot, SM its parallel, CS$=\rho$,

$$d\lambda=(\rho)\,.\sin \mathrm{CSM}=\frac{m-1}{\sin 1''}\,.\tan\tau\,.\,\frac{\sin \mathrm{CM}}{\sin\rho},\ \text{nearly};$$

$$\text{or } d\lambda \text{ varies nearly as } \frac{\tan\tau}{\sin\rho};\ \text{since CM is constant.}$$

I have given the values of this ratio for convenience in the last column of Table I. for $h=\frac{1}{4}$, by which it appears that they may be considered constant in connexion with the unavoidable errors of observation, and absolutely vary very slowly for most values of ρ.

We are compelled to recur again to longitudes. If a pair of well-defined spots could be found differing little in latitude and about 30° in longitude, which had twice crossed the disc together under favourable circumstances of observation, I should select them for our next discussion; but the conditions are such as are not satisfied by any series I have yet succeeded in obtaining.

Accordingly, that the conclusions of this paper may not rest on a single series of observations, although satisfactory in themselves, I will, lastly, take another series of deduced longitudes for examination, which were obtained at nearly the same time in the same year, 1854. During its first passage, the spot exhibited a circular and well-defined nucleus, which appeared to attain its greatest actual expansion about the time it first passed the centre. At its reappearance, all trace of penumbra was gone, the nucleus was much reduced, and was an exceedingly sharp object for observation. After Sept. 1 it was no longer to be seen.

1854.	*p.*	*ρ.*	*l.*	λ.	*t.*
July 29·565	114° 45′	68° 28′	164° 42′	S. 11° 40′	d 12·268
30·518	118 12	56 2	178 37	11 30	·234
Aug. 1·496	133 17	31 53	207 2	11 12	·221
2·555	153 25	21 37	221 46	11 8	·251
6·586	259 47	43 6	278 46	10 56	·281
7·517	265 31	54 35	292 3	10 42	·281
8·525	269 29	66 32	305 34	10 26	·341
Aug. 25·508	123 2	69 47	189 24	S. 10 14	12·229
26·535	126 44	57 24	203 23	10 20	·276
27·564	132 9	44 25	218 10	10 3	·268
28·528	141 24	33 5	231 56	10 13	·267
29·526	159 58	22 55	246 19	10 23	·256
30·522	195 9	17 34	260 29	10 23	·259
31·546	235 23	21 5	275 6	10 19	·258
Sept. 1·517	256 34	30 5	288 54	S. 10 8	12·262

The period $25^{d}\cdot240$ for the sun's sidereal rotation satisfies best the above longitudes, and has been used, as in the first example, in computing the dates t. In the first column of the next Table are given the quantities (ρ); in the second the corrections to l on hypothesis $m=1\cdot010$, computed by a fresh reduction with the same elements used before; in the third, the uncorrected discordances of t; in the remainder as indicated.

$(\rho.)$	$dl.$	Disc^{s}.	$m=1\cdot0025.$		$m=1\cdot0050.$		$m=0\cdot0100.$	
′	′	d	d	d	d	d	d	d
+38	+38	+·004	+·011	+·015	+·022	+·026	+·045	+·049
+32	+32	−·030	+·010	−·020	+·019	−·011	+·038	+·008
+16	+14	−·043	+·004	−·039	+·008	−·035	+·016	−·027
+11	+ 7	−·013	+·002	−·011	+·004	−·009	+·008	−·005
+22	−21	+·017	−·006	+·011	−·012	+·005	−·025	−·008
+30	−29	+·017	−·009	+·008	−·017	·000	−·034	−·017
+37	−37	+·077	−·011	+·066	−·022	+·055	−·043	+·034
+40	+40	−·035	+·012	−·023	+·023	−·012	+·047	+·012
+32	+32	+·012	+·010	+·022	+·019	+·031	+·038	+·050
+23	+22	+·004	+·007	+·011	+·013	+·017	+·026	+·030
+17	+15	+·003	+·004	+·007	+·009	+·012	+·018	+·021
+11	+ 7	−·008	+·002	−·006	+·004	−·004	+·008	·000
+ 8	− 3	−·005	−·001	−·006	−·001	−·006	−·003	−·008
+10	− 7	−·006	−·002	−·008	−·004	−·010	−·008	−·014
+15	−13	−·002	−·004	−·006	−·007	−·009	−·015	−·017
Sums: 1st Series		±·201		±·170		±·141		±·148
— 2nd Series		±·075		±·089		±·101		±·152
— Both ...		±·276		±·259		±·242		±·300

The discordances of the first series are larger than those of the second, as was to be expected from the spot being of greater size, and offering a less definite centre for observation. It will be necessary to have regard to this circumstance in considering the uncorrected and corrected sums. The first hypothesis on the whole improves the result; the second, while it diminishes the total, sensibly increases the sum of the second series; and the third, judging principally by the second series, shows decided signs of over-correction. If we now treat the discordances as before by equations of condition, the method of least squares gives by the first series $m=1\cdot008176$, and by the second, which is to be preferred, $m=1\cdot001998$; by the whole $m=1\cdot005684$. If we hold by the evidence of the second series, we have $1\cdot0020$ as the most probable value of m, precisely the same which resulted from our former discussion. If it be thought better to include the first series, we find a somewhat larger value. As a general conclusion we may say that the motions of the solar spots in longitude will be more nearly proportional to the time, if we apply corrections to the angles ρ differing little from half of those in the first column of our Table I.; that their evidence is on the whole in favour of an atmosphere causing refractions of about half the amount there tabulated, but is opposed to refractions much exceeding those in amount.

As a definitive result, the conclusion will probably be considered as entitled to little weight; yet it is such as would agree pretty well with the idea that the red protuberances are cloudy masses of great tenuity held in suspension, as well as with the absorptive action on light and heat, which has been previously noticed by observers.

It is at least satisfactory to find that the motions of the spots offer no objections to the admissibility of an atmosphere, such as common air of a uniform density increased from 8 to 10 times, and extending to a distance from the surface equal to one-fourth the radius, but rather favour the assumption of an equivalent atmosphere, while they suffice to assign quickly a superior limit to its amount.

The observations discussed, not having been corrected for terrestrial refraction, it is desirable to point out that the angles ρ are sensibly independent of correction from that source, being derived from the expression

$$\frac{r}{\mathrm{R}} = \sin\,(\rho + \rho'),$$

where r and R are respectively the linear distances of spot and limb from centre of disc, which, as is well known, are by refraction contracted in very nearly the same proportion.

The principal source of error which may affect our result is the omission of the effect of perspective on the apparent centre of a spot's nucleus, according as we adopt the conclusions of Dr. Wilson or Sir W. Herschel on that subject. But if we exclude observations very near the limb, and take only spots which exhibit penumbra on the side next the centre, our conclusions will not be affected by Dr. Wilson's hypothesis; excepting, it is true, that the radius of the disc employed should be diminished by the depth of the spot's cavity. On Sir W. Herschel's hypothesis (the word is perhaps objectionable), it would similarly be necessary to diminish the radius employed by the distance between the surfaces of the photosphere and the cloudy stratum. But if a spot be taken, as in the second series of our second spot, which exhibits no penumbra, no such objection will lie. The remaining difficulty is inherent, the little permanence of such spots.

LVI. *Chemical Notices from Foreign Journals.*
By E. Atkinson, *Ph.D.*

[Continued from p. 303.]

THREE gases are known which have the composition C^2H^3Cl. One is prepared by the action of hydrochloric acid on wood-spirit; another was prepared by Bunsen by heating basic perchloride of kakodyle; and a third was obtained by Kolbe and Varrentrapp, by the action of light on a mixture of equal volumes of chlorine and marsh-gas. The first has always been considered to be true chloride of methyle. Bunsen's gas, although it has similar physical properties, was held by Gerhardt to be marsh-gas, C^2H^4, in which one equivalent of hydrogen was replaced by chlorine.

Baeyr* endeavoured to decide this question by determining the coefficient of absorption† of the various gases. The gas from kakodyle had for water, the coefficient of absorption—

At 7° . . .	5·304
At 14° . . .	4·172
At 20° . . .	3·462
At 25° . . .	3·034

Below +6° C. the gas forms a solid colourless hydrate, crystallizing in rhombic prisms, which has some similarity with hydrate of chlorine. Chloride of methyle from wood-spirit forms likewise the same hydrate, and has the same coefficient of absorption; these two gases are therefore identical. The hydrate forms a very convenient means of obtaining the pure gas, as otherwise it always contains traces of oxide of methyle.

The absorptiometric determinations of the body from marsh-gas gave not entirely accordant results, which probably arose from its containing a little hydrogen. The coefficient for 14° was found to be in the mean 0·08, or one-fiftieth that of the others. As, further, this gas forms no hydrate, it is certain that it is a different body from the others, which are chloride of methyle.

Berthelot‡ has recently effected the synthesis of wood-spirit by means of this substitution-product of marsh-gas. He states that, under the influence of suitable agents, it fixes the elements of water, loses hydrochloric acid, and becomes changed into wood-spirit,

$$C^2H^3Cl+2HO-HCl=C^2H^4O^2.$$

To obtain the gas, closely stoppered flasks of 1 litre capacity,

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* Liebig's *Annalen*, August 1857. † Phil. Mag. vol. ix. p. 116.

‡ *Comptes Rendus*, November 30, 1857. *Annales de Chimie et de Physique*, January 1858.

containing a mixture of equal volumes of chlorine and purified marsh-gas, are exposed to the action of feeble solar light. When the flasks are opened over mercury, and a little potash added, they are reduced to one-half. The residue contains chloride of methyle mixed with marsh-gas and hydrogen. The chloride of methyle is separated by agitation with glacial acetic acid, in which it dissolves. The acid, when boiled and afterwards supersaturated with caustic soda, gives off the gas which it dissolved; the gas is collected over mercury, and purified by digestion with a little potash. Berthelot states that it is soluble in one-fourth its volume of water.

By acting upon this gas with a mixture of sulphuric acid and sulphate of silver, sulphomethylic acid is readily formed. The simultaneous action of both these agents is necessary, as neither separately has any action at 100° C. From sulphomethylic acid, the sulphomethylate of baryta, and from it benzoate of methyle, oxalate of methyle, or wood-spirit, may be readily prepared.

The biatomic alcohol glycol* bears the same relation to chloride of æthylene that ordinary alcohol does to chloride of æthyle:—

$$\underset{\text{Alcohol.}}{C^4H^6O^2}+HCl=H^2O^2+\underset{\text{Chloride of æthyle.}}{C^4H^5Cl}.$$

$$\underset{\text{Glycol.}}{C^4H^6O^4}+2HCl=2H^2O^2+\underset{\text{Chloride of æthylene.}}{C^4H^4Cl^2}.$$

By the action of pentachloride of phosphorus on glycol, Wurtz† has obtained chloride of æthylene. When the two substances are brought together, a brisk action takes place, hydrochloric acid is evolved, and there is obtained a liquid which is a mixture of oxychloride of phosphorus and chloride of æthylene. By washing with water, drying over chloride of calcium, and rectification, the latter body is obtained in a state of purity.

The reaction is thus:—

$$\underset{\text{Glycol.}}{C^4H^6O^4}+\underset{\text{Pentachloride of phosphorus.}}{2PCl^5}=2HCl+\underset{\text{Oxychloride of phosphorus.}}{2PO^2Cl^3}+\underset{\text{Chloride of æthylene.}}{C^4H^4Cl^2}.$$

Chloride of æthylene is the hydrochloric æther of glycol.

Berthelot found that olefiant gas, by the intervention of sulphuric acid, combined with the elements of water to form alcohol. This reaction could not, however, be applied to the preparation of the higher homologues of alcohol, from the corre-

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* Phil. Mag. Supplement, January 1857.

† *Comptes Rendus*, vol. xlv. p. 228.

sponding homologues of olefiant gas, inasmuch as these hydrocarbons exert a reducing action on the acid at the temperature necessary for the reaction. Berthelot has, however, lately* found that certain homologues of olefiant gas combine with the hydrogen acids, and that in this way the corresponding alcohols may be prepared.

Propylene gas, C^6H^6, heated with concentrated hydrochloric acid for several days to 100° C., is completely absorbed with the formation of chloride of propyle,—

$$\underset{\text{Propylene.}}{C^6H^6} + HCl = \underset{\text{Chloride of propyle.}}{C^6H^7Cl.}$$

By treating this chloride of propyle with solution of potash, propylic alcohol, $C^6H^8O^2$, is obtained. In like manner amylene, $C^{10}H^{10}$, unites, though more slowly and less completely, with hydrochloric and hydrobromic acids to form chloride and bromide of amyle, from which amylic alcohol is obtained.

Caprylene, $C^{16}H^{16}$, and æthalene, $C^{32}H^{32}$, also combine with these acids, although the action, especially of the latter substance, is still slower and less complete.

Couper† describes two bromine substitution-products of benzole, obtained by bringing bromine vapour into contact with boiling benzole.

Bromobenzole (bromide of phenyle), $C^{12}H^5Br.$, boils at 150° C. It is a colourless liquid, with an odour resembling benzole. It does not solidify at −20° C. Heated with fuming nitric acid, there is formed a crystalline nitro-substitution product of the formula $C^{12}H^4(NO^4)Br$, which melts at 90° C., but cannot be distilled without decomposition.

Bromobenzole dissolves in fuming sulphuric acid; the solution, on standing, deposits crystals of sulphobromobenzolic acid. This acid is very deliquescent; it forms with ammonia an almost insoluble crystallizable ammonia salt of the composition

$$C^{12}H^5Br\,2SO^3\,NH^3.$$

By the continued action of excess of bromine on bromobenzole, hydrobromic acid is evolved, and *dibromobenzole*, $C^{12}H^4Br^2$, is formed in crystals, which are purified by recrystallization from æther. It forms beautiful oblique rhombic prisms, which melt at 89° C., and distil without decomposition at 219° C.

Buff and Wöhler, in continuing their researches on siliciuretted hydrogen‡, with a view of finding a method for the preparation

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* *Comptes Rendus*, vol. xliv. p. 1350.

† Ibid. vol. xlv. p. 230. Liebig's *Annalen*, November 1857.

‡ Phil. Mag. August 1857.

of this remarkable body without the aid of the galvanic current, have described more fully* the series of new silicon compounds, of which a short notice has already appeared in this Journal.

Hydrochlorate of Protochloride of Silicon, Si^2Cl^3+2HCl.—This is prepared by passing dry hydrochloric acid gas over crystallized silicon heated in a glass tube just below redness. To the anterior part of the tube a U-shaped tube-receiver is attached, which is placed in a freezing mixture of ice and salt, and with which is connected a gas-delivery tube dipping in ice-cold water. The tube must not be heated too high, otherwise ordinary chloride of silicon would be formed. The hydrochloric acid is readily decomposed, and inflammable siliciuretted hydrogen continually bubbles through the condensing water; at the same time some uncondensed protochloride is carried away in the stream of gas, which, decomposing with the water, forms a new oxide of silicon. In order to prevent the latter from decomposing, it is necessary to keep down the temperature of the water.

After the operation is concluded, the protochloride is found in the U-tube. It is turbid, and contains several substances. On distilling it, it begins to boil at 28°—30° C., and the temperature gradually rises to 60°, the greater part passing over at 40°—43° C. This is the new substance.

The protochloride of silicon is a colourless mobile liquid, of an irritating odour, fuming strongly in the air, and covering everything with a white incrustation. It boils at 42° C., and its specific gravity is 1·65. It does not conduct the galvanic current. Its vapour is as inflammable as that of æther, and it burns with a feeble greenish flame, emitting vapours of silica and hydrochloric acid. When mixed with excess of oxygen in a eudiometer, and the electric spark passed through the mixture, it explodes with great violence, covering the inside of the tube with silica. The residual gas fumes strongly, and contains chloride of silicon and hydrochloric acid. When its vapour is passed through a red-hot tube, it is decomposed into ordinary chloride of silicon and hydrochloric acid. With water it decomposes instantaneously into hydrochloric acid and into a new oxide of silicon, very different in appearance from ordinary silicon. The gaseous protochloride is also readily absorbed by alcohol and æther without separation of the oxide.

The numbers obtained in the analyses show that the substance contained some ordinary chloride, from which indeed it is exceedingly difficult to free the protochloride, as their boiling-points only differ by 17 degrees. Buff and Wöhler supposed at first that the formula of the body was Si^2Cl^5, as the two equivalents of hydrogen, which only amount to 0·9 per cent., scarcely

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* Liebig's *Annalen*, October 1857.

alter the relation between the chlorine and the silicon. But the deportment of the body to aluminium showed the true state of the case. When the vapour of the protochloride is passed over fusing aluminium, it is decomposed with the greatest ease, a quantity of hydrogen is liberated, chloride of aluminium sublimes, and the rest of the aluminium is covered with a crust of black crystalline silicon. From the composition of the protochloride as finally adopted, it is clear why it is not formed when ordinary chloride of silicon is passed over heated silicon.

Hydrobromate of Protobromide of Silicon, $Si^2 Br^3 + 2HBr$.—This is formed under similar circumstances, and has similar properties to the protochloride.

Hydriodate of Protiodide of Silicon, $Si^2 I^3 + 2HI$ is formed in the same manner as the foregoing compounds. It is a dark-red brittle mass, which fumes strongly in the air, is readily fusible, and solidifies on cooling to a crystalline mass. Heated more strongly it boils and distils over. It is only slowly decomposed by water. It is soluble in bisulphide of carbon, with a blood-red colour, and may be obtained in dark-red crystals on concentrating and cooling the solution.

Hydrated Oxide of Silicon, $Si^2 O^3 + 2HO$.—This is formed by decomposing any of the preceding compounds by water, but it is easiest obtained as a bye product in the preparation of the protochloride.

After filtering off from the water it is washed with a little cold water, and dried at first by pressing between blotting-paper and then over sulphuric acid.

Thus prepared it forms a snow-white, light, amorphous powder. It is dissolved by caustic and carbonated alkalies, and even by ammonia, with violent disengagement of hydrogen. It can be heated to 300° C. without losing water, and without being changed, but at a higher temperature it takes fire and burns with a phosphorescent light, hydrogen being liberated, which inflames with a violent explosion.

It burns in oxygen with a brilliant light; it also burns when heated in a covered crucible, but the residual silica is more or less brown from amorphous silicon. In fact, the hydrate when heated gives off siliciuretted hydrogen, though unfortunately only at a temperature at which the latter is decomposed. When the hydrate is heated in a tube, a gas is disengaged which fumes in the air, but on account of the admixed hydrogen does not spontaneously inflame. When inflamed, however, it burns, forming silica.

The hydrate is slightly soluble in water; and the solution of it, obtained in the preparation of the protochloride, being filled with bubbles of hydrogen, has all the appearance of being in a state of fermentation.

The solution of the hydrate has a powerful reducing action, precipitating metallic gold and palladium from their solutions, and forming with silver solution silicate of the suboxide of silver. It also reduces copper salts; and from selenious, telluric, and sulphurous acids it liberates selenium, tellurium, and sulphur.

The analyses lead to the formula $Si^2O^3, 2HO$; and the substance can have no other composition, if the protochloride have the formula $Si^2Cl^3, 2HCl$. But this is still an open question. Buff and Wöhler have reason to believe that there is a still lower chloride and a corresponding oxide, but all their endeavours to decide this point were fruitless. They hope to resume it when they possess more silicon.

It was an old observation of Schafheutl, that the residue from the solution of cast iron in acid, after being well washed out, liberated hydrogen when treated with ammonia. The discovery of the new oxide of silicon rendered it probable that this residue contains the hydrated oxide of silicon. A recent investigation by Wöhler* showed that this is the case; and it seems therefore that siliciuret of iron, when dissolved in acid, does not form silica, but this new hydrate,—a deportment shared by siliciuret of manganese, as has been noticed by Wöhler.

Dumas' method of taking vapour densities, so very accurate and easy of execution, is admirably fitted for all substances of moderately low boiling-points. But many chemical substances, and especially inorganic compounds, volatilize at such high temperatures that an oil-bath cannot be used, and even glass vessels soften. In any general method for the determination of the vapour densities of substances with high boiling-points, two questions demand attention: first, the manner in which the heat is to be applied; and second, the nature of the vessels in which the substance is to be volatilized. Troost and Deville† have described a method which introduces important modifications in both these points.

They find that a constant temperature is obtained by the evaporation of substances of moderately low boiling-points, provided care be taken to exclude the influence of the heat from the source of heat, and that of the surrounding air. In the arrangement which Deville and Troost describe, these sources of error are excluded. The substances used for determinations, in which glass vessels can be employed, are mercury and sulphur; the former of which boils at 350° C., and the latter at 440° C.

The apparatus consists of a mercury bottle cut off near the neck, so as to form a cylinder closed at the bottom. In the interior are two rings provided with holes, between which the glass globe is held firm about 6 or 8 centimetres above the bottom of

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* Liebig's *Annalen*, December 1857.

† *Comptes Rendus*, vol. xlv. p. 821. Liebig's *Annalen*, February 1858.

the cylinder. Small cylindrically bent pieces of sheet metal, affixed parallel to the sides of the cylinder, form additional layers of vapour surrounding the central space, which exclude all influence from the external source of heat, and also from the surrounding air. The upper part is closed by a lid of cast iron provided with two holes, through one of which passes the finely drawn-out neck of the cylinder, and through the other the tube of an air-thermometer, which does not need to be graduated, as it only serves to show if the temperature in the interior be constant. An iron tube, 2 centimetres in diameter, is screwed as high as possible in the side of the cylinder, so that its aperture shall be at least 8 centimetres above the base of the neck of the globe. If sulphur be used, this tube may conveniently terminate in another tube, in which the sulphur may condense and flow out without inflaming. The operation, according to the authors, is easy of execution. In general it is necessary to evaporate 2 pounds of sulphur, or 2–4 pounds of mercury in each operation. The experiment is concluded when no more vapour rises from the neck of the globe, which is kept warm by hot charcoal.

Deville and Troost give some vapour densities determined by this method.

They found that the vapour density of pure chloride of aluminium in mercury vapour at 350° C. was 9·35, and in sulphur vapour at 440° C. 9·34. The density calculated for a condensation to two volumes is 9·31. They found for the density of perchloride of iron, which has the same condensation, 11·39 in sulphur vapour. The density calculated for the formula $Fe^2\,Cl^3$ is 11·25. The vapour density of chloride of mercury calculated for the formula $Hg^2\,Cl$, and for a condensation to four volumes, is 8·15. Mitscherlich had found the number 8·35. Deville and Troost find the number 8·21.

An example is given of the use of a knowledge of the vapour densities of substances whose formulas cannot be considered to be fixed. They found for the vapour density of chloride of zirconium determined in sulphur vapour, the number 8·15. The formula of this substance is generally considered to be $Zr\,Cl^3$, and its equivalent 174·5. On this assumption its theoretical vapour density would be 12. But assuming for it the formula $Zr\,Cl^2$, its theoretical density would be 8·02, with which the results of experiment closely agree.

In a subsequent communication, Deville and Troost promise to communicate the results of experiments which now occupy them, in which the vapour of zinc is used as a mode of heating; and for vessels, globes of porcelain are employed, the necks of which terminate in a point fine enough to be closed in a moment by the oxyhydrogen blowpipe.

LVII. *On the Observation of Atmospheric Electricity.*

By F. Dellmann*.

PROFESSOR W. THOMSON has had the goodness to notice my method for the observation of atmospheric electricity; at his desire I have rewritten the following for this periodical.

The apparatus, consisting of the collector and the measurer, has received a new construction at my hands, for the special purpose of attaining the greatest possible accuracy of result.

The essay by Duprez, which obtained the prize awarded by the Brussels Academy in 1844, convinced me completely that the collector must be moveable; because a fixed one cannot be relied upon for sufficient insulation, and because its insulation can never be properly checked. The moveable apparatus which I have constructed may at any time be used as a fixed one, and can be immediately rendered moveable again. By its means I have been enabled to show that the fixed apparatuses are subject to two hitherto neglected sources of error; viz. first, that they become charged too slowly, inasmuch as they require at least from 20 to 25 minutes before they are completely charged; secondly, that the action of the atmospheric electricity upon the collectors is never pure, but accompanied by the action upon the part of the apparatus connecting the collector with the measurer.

My collector has the following form:—A brass sphere, of about 5 inches in diameter, is screwed upon a brass wire of about 2 lines in thickness and 15 inches in length. The lower end of this brass wire, carrying the sphere, is fastened into a foot of shell-lac of about 9 lines in thickness, and is provided, at a height of about 4 inches, with an enveloping ring of shell-lac of the same thickness. The collector can thus be placed in an insulated state in a cylindrical brass case, into the middle of which a bottom is soldered. In this way the case may be placed with its lower half enveloping the top of a pole of pine wood, which may be raised or lowered by means of a cord passing over a couple of pulleys. The pole passes through two iron rings fastened to two iron arms, about 4 feet apart, fixed into the wall of the house. The top of the pole is cased with iron for a length of about 9 inches. A vertical slit, of about 2 inches in length and 2 lines wide, is cut through the top of the pole and its iron casing, immediately beneath the place to which the lower enveloping half of the brass case reaches. A brass ruler passes through the slit, and may be turned like a lever in a vertical plane about an iron peg which passes horizontally through it. On to the one, and somewhat longer, arm of this lever a brass

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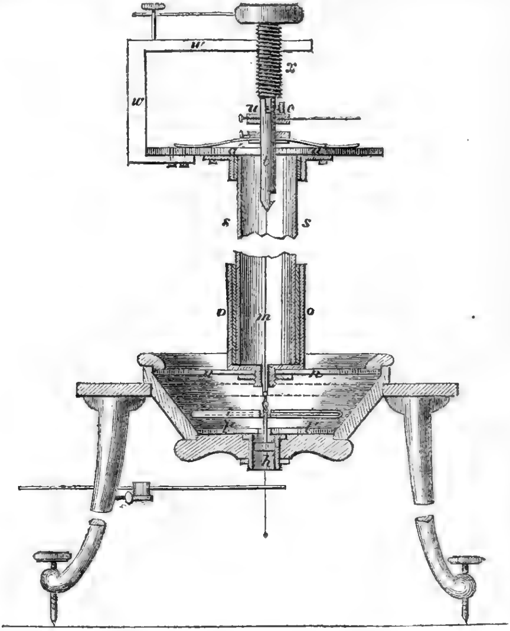
* Communicated by Prof. W. Thomson.

rod is fastened, which reaches so far upwards that a crosspiece fastened to it and directed towards the carrier of the collector, touches the carrier immediately above the case, when the shorter arm of the lever is depressed. The shorter arm of the ruler is bored through at its extremity, and a thin brass wire passes through the hole and is carried downwards. The brass wire carries a brass handle at its lower extremity for the convenience of taking hold of it, and is so long that when the sphere is raised above the roof of the house, the handle reaches to the window from which the sphere was placed upon the pole. On pulling the handle, the crosspiece mentioned strikes upon the carrier of the sphere, whereby the latter becomes charged. On letting go the handle, the angular lever falls back, because it is heavier on the side on which the crosspiece is situated. The sphere is then lowered, removed from the pole, and the electricity which it has brought with it given, by means of a transferrer, to the measurer. As the sphere acquires its electricity in the way described, it must always bring down the opposite electricity to that of the atmosphere. A measurement is completed in a few seconds. By placing the sphere, during the interval between two measurements, upon a tripod in a room, it may be retained for a long time in an insulated state; and inasmuch as it can easily be taken with its carrier out of the case in which it stood upon the pole, any possible want of insulation may be checked and corrected. The sphere, while being charged, is remote from the observer, so that his presence cannot exert any such influence as must have been the case with the observations made at Brussels and Munich. When the sphere is elevated, the whole apparatus is fixed in its position by means of a windlass and break, which are placed by the drum upon which the cord is wound. In this condition the apparatus is a stationary one, but it may be converted into a moveable one by lowering the break.

The torsion of a glass thread is employed to measure the electricity, for there is no force so constant and reliable for this purpose as the elasticity of glass. The glass thread *m* (see woodcut) is stuck with shell-lac upon the end of a brass rod, *t*, about 2 lines in thickness, so as to be in the same straight line with the axis of the rod. Fastened to the rod with a screw, there is an index, *u*, which moves above a graduated circle, *a'*. The brass cylinder supporting the plate on which is the graduated circle, is firmly pressed upon a glass tube, *s*, of tolerable thickness, which is provided with an external ring of leather for this purpose. The lower end of the glass tube is also furnished with a leather collar, by which it is firmly fastened into a second brass cylinder, *o*, which passes through a thick piece of plate glass, *n*. This glass plate forms the cover to a jar-shaped vessel of thick brass. The lower end of the very fine glass thread reaches into this vessel, and carries a fine brass wire, *i* (the arm of the balance), about 2 inches long, in a horizontal position, above a strip of thin brass plate of the same length and about 1 line broad. The arm of the balance is hammered out flat, so that its flat faces are vertical. The strip of brass, the plane of which is also vertical, is fastened into a narrow slit on a brass wire, *h*, about half a line in thickness, which passes insulated through the bottom of the brass vessel and extends about 2 inches beyond. By means of this wire the electricity to be measured is imparted to the instrument, on which account I have given it the name of the conducting wire (*Zuleitungsdraht*). On the bottom of the inside of the brass vessel, that is, beneath the brass strip, a brass graduated circle, *l'*, is also placed, which must be sufficiently polished to form a mirror. If the upper index be turned, the arm of the balance also turns. The stationary strip of brass in the lower vessel is fixed at 0°. The instrument is adjusted when the index above, and the arm of the balance below, point to the same numbers on the two scales. When reading off the angle below, the error of parallax is avoided by making the image



and the object, that is the arm of the balance, coincide. The instrument may readily be adjusted by turning round the higher divided circle. The glass plate covering the brass jar is surrounded with leather, so that it can be arrested by means of a screw passing through the brass case. By slackening this screw the instrument may also be adjusted, but not so certainly as by turning the higher graduated circle; because by screwing up the screw, the glass plate may too readily be displaced. On charging the measurer, the arm of the balance must rest upon the brass strip and form a right angle with it; for this to be effected, it must be turned to 90° and then lowered. The elevation and depression of the arm of the balance is effected by turning a screw, x, which passes through a bracket, w, attached to the upper index-plate. The end of this screw, formed into a peg, enters the top of the brass rod upon which the glass thread is fastened, and is held in its place by a transverse screw, c', which passes from the exterior of the rod and works into a thin part cut in the peg. By this arrangement the movement of the rod may be regulated with great accuracy.

After charging the instrument, the upper index is turned to 0°; the arm of the balance below would therefore also move to 0° if it had not become electrical. The angle which it makes with the brass strip, after it has come to rest, is the magnitude from which the electrical quantum may easily be calculated by means of a table. The tension of one element of a zinc-copper battery is taken as unity. The mean values obtained from three daily observations for two years, give two mean yearly values which nearly agree, namely 157·9 and 155·2, although the mean monthly values for the two years differ considerably. The mean monthly values, commencing with January, are for the first year,

191·5; 187·6; 150·9; 133·7; 114·2; 122·9; 124·7; 130·8; 142·4; 190·2; 172·6; 233·2:

and for the second,

169·5; 140·4; 150·6; 128·4; 114·1; 119·1; 118·7; 149·3; 154·1; 163·1; 226·2; 229·2.

Kreutznach, February 1857.

LVIII. *On the Rotation of the Electrical Light round the Pole of an Electro-magnet.* By M. A. De la Rive[*].

To Dr. Faraday, F.R.S. &c.

My dear Friend, Geneva, May 10, 1858.

Having learnt from my friend Professor Marcet that you and Mr. Gassiot have been occupied with the remarkable luminous effects produced by the electrical discharges of Ruhm-

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[*] Communicated by Professor Faraday.

korff's induction apparatus through Geisslar's tubes*, and the influence which magnetism exerts over them, I recall to your mind that I was the first to point out this influence under a form different, it is true, but more remarkable, in my opinion, than that which M. Plücker has made known. The note which the learned philosopher of Bonn has published respecting this subject induced me to repeat my experiment, and has made me feel the necessity of adding to its description some details necessary for the reproduction of the results with certainty. I have just had the opportunity, during a recent residence at Berlin, of making the experiment in the laboratory of M. Magnus, in the presence of that illustrious philosopher, and of MM. Riess, Dove, Dubois-Reymond, and Wiedemann, all of whom were witnesses of the facility with which it succeeded.

I have already described, in a letter addressed to M. Régnault, and inserted in the *Comptes Rendus* of the Academy of Sciences of Paris of 1849, vol. xxix., and also in my 'Treatise on Electricity,' vol. ii. p. 308, the apparatus intended to show the action in question. It consists of a glass balloon, associated with which is a rod of soft iron, surrounded, except at its two extremities, by a very thick insulating layer; one end of the rod is nearly in the centre of the balloon: the other issuing from the balloon through a tubulature, is external and capable of being placed upon the pole of an electro-magnet. The requisite electrical discharges are set up, when the air is sufficiently exhausted in the balloon, between the interior extremity of the iron rod and a metal ring which surrounds this rod, and outside the insulating layer, at the portion nearest to the tubulature.

The first time that I made the experiment, I employed an Armstrong's hydro-electric machine to produce the discharges; subsequently I made use of an ordinary electrical machine, but then the phænomenon was less distinct, which is due to the inferior power of the source of electricity, and especially to the less degree of continuity of the discharges. But as Armstrong's machine is not convenient to manage, I found that it might be replaced with advantage by Ruhmkorff's induction apparatus, by establishing a communication between one end of the secondary coil and the rod of soft iron, and between the other and the interior ring of copper, by means of a wire soldered to this ring, and traversing the tubulature, but still insulated from the soft iron. I should add, that it is necessary for the success of the experiment that the balloon should contain a certain quantity of vapour, of which the tension may be equal to 4 or 6 millims. of mercury; the aqueous vapour which usually remains in the balloon when

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* [Messrs. Gassiot and Faraday have worked together only on the peculiar tubes constructed under Mr. Gassiot's direction.—Ed.]

a vacuum is produced in it, having a tension of about 3 to 5 millims., is sometimes sufficient for the production of the phænomenon; but it is preferable to introduce this vapour directly, and even a vapour arising from a more volatile liquid, such as alcohol, æther, or sulphuret of carbon. Vapour of oil of turpentine has also afforded me a good result. To introduce the vapours, all that is necessary is to produce a vacuum in the balloon, and to permit the entrance of air by opening it over a bottle containing the liquid the vapour of which is to be introduced, and which is to be gently heated if, like oil of turpentine, it is not sufficiently volatile; a vacuum of about 4 or 6 millims. is then again to be produced in the balloon.

When the apparatus is thus arranged, the positive electrode of the Ruhmkorff's apparatus is connected with the rod of soft iron, and the negative one with the ring; one or more distinct luminous jets are immediately seen starting from the extremity of the rod, and forming between this and the ring, curved lines like those of the electrical egg; at the same time the upper part of the rod is covered with brilliant points, which are agitated like the particles of a boiling liquid. As soon as the rod of soft iron is magnetized by its being placed with its base upon the pole of a strong electro-magnet, the luminous jets acquire a rapid, very strongly-marked, and perfectly visible movement of rotation in one direction or the other, according as the pole of the electro-magnet is north or south. At the same time the brilliant points which were upon the summit of the iron rod disappear from thence and are driven to the edges, where they form a luminous ring, which turns like the jets and in the same direction. When the direction of the induced discharges is changed, that of the rotation is reversed.

Not to dwell upon the different details of the phænomenon, which vary with the nature of the vapours introduced into the balloon, a subject to which I shall return very shortly, I shall confine myself at present to remarking that when the rotation has continued a certain time the jets spread out, and at last form around the cylinder of soft iron a cylindrical luminous and nearly continuous coat which revolves with great rapidity, but of which the rotation is often difficult to perceive, because of the continuity of the light. To reproduce the separate jets the rotation of which is so distinct, the passage of the discharges must be stopped and vapour again introduced.

Without insisting afresh upon the analogy presented by the luminous electro-magnetic phænomena which I have just described with the aurora borealis, an analogy which I have explained in my 'Treatise on Electricity,' vol. iii. p. 292, I cannot, nevertheless, avoid taking this opportunity of recalling, in favour of this analogy, the observation which has just been made by Dr. Robinson (Phil. Mag. April 1858), that the light of the aurora borealis, like the electrical light, possesses the property of rendering those substances fluorescent which are capable of being so, a property which is due to the presence in both lights of the most refrangible rays.

Believe me, &c.,

A. DE LA RIVE.

P.S. 19*th May.*—In my letter of the 10th of May, which contained some details relative to my experiments on the rotation of the electrical light round the pole of an electro-magnet, I have omitted a very important point which I now point out. Of the vapours which I have successively introduced into the balloon, that which has given me the most brilliant, and at the same time the most constant results, is undoubtedly the vapour of sulphuric æther, at the tension of 10 to 12 millims. of mercury. Not only is the rotation very distinct in one direction or the other, according to the direction of the discharges and the nature of the magnetic pole, but the experiment may be continued for a very long time without the phænomenon being changed in appearance or rendered more difficult of perception, at the end of a certain time, by the expansion of the jets round the cylinder, as is the case with the other vapours. The streams are also very apparent and more durable. This difference is probably due to the vapour of æther in the balloon being in a state of greater tension and density than the others, and to its being less rapidly decomposed by the passage of the electrical discharges.

[It may be as well to point out that Davy first observed the effect of the magnet on the electric discharge in air either dense or rarefied (Phil. Trans. 1821, p. 427). Ritchie perhaps first observed it in water, and at a later date. All these motions and rotations obey the law enunciated by Faraday in his account of electro-magnetic rotation, Experimental Researches, 8vo, vol. ii. pp. 130, 131.—ED.]

LIX. *Description of a* Ruhmkorff's *Induction Apparatus, constructed for* JOHN P. GASSIOT, *V.P.R.S., by* Mr. RITCHIE, *Philosophical Instrument Maker, Boston, U.S.*

To the Editors of the Philosophical Magazine and Journal.

Clapham Common,
May 24, 1858.

GENTLEMEN,

THE following description of a Ruhmkorff's induction apparatus, which has been recently constructed for me by Mr. Ritchie of Boston, United States, may probably not be devoid of interest to the readers of the Philosophical Magazine.

The primary wire is No. 9 gauge, the helix 15 inches in length with three courses, the length about 150 feet; the interior bundle of wires 18 inches long, about $1\frac{3}{4}$ inch diameter of annealed iron wire of No. 16, annealed with great care, having been a fortnight in cooling.

The primary wire has a cover of gutta percha $\frac{1}{10}$th of an inch in thickness, passing through the basement to a plate of the same, to which it is united; over this gutta-percha coating a glass tube passes.

The secondary helix is divided into three bundles, each 5 inches long, wound on cylinders of gutta percha $\frac{1}{10}$th of an inch thick; the wire in the upper and lower is of No. 33 gauge, each 25,575 feet; the middle one is of No. 32, 22,500 feet, forming a total length of 73,650 feet; the stratum of wire is perpendicular to the length of the helix, and the wire is wound over with silk.

The condensers are of varnished tissue-paper of three thicknesses between each stratum of foil; there are three condensers, with surfaces of about 50, 100 and 150 feet; by means of screws these can be used separately or combined.

The contact-breaker is raised by means of a ratchet-wheel turned by the hand, which acts on a spring, so that the platina surfaces touch firmly.

The upper platina has a screw and binding nut; the hammer has also its screw and binding nut: the hammer must not bear too heavily, and care must be used to adjust the screws so that the ratchet-wheel works well.

It will be observed from the preceding description, that the manner of breaking contact, or the disruption of the primary current, is different from the plan used by Ruhmkorff or Foucault in Paris, or by Bentley or Hearder in this country; for the exhibition of the phænomena of stratification their arrangements are preferable, as in manipulating by means of a ratchet-wheel it is difficult to obtain uniformity of action; but that of Ritchie enables us to obtain the maximum effect due to the magnetization of the iron core, to produce which, time is an important element.

When the handle attached to the ratchet-wheel is turned very slowly, the contact with the primary current is prolonged, and the iron core consequently becomes highly magnetized; the suddenness of the break instantaneously developes the entire force of the induced discharge, not only giving sparks of great length, but of a very remarkably dense character, the main line of discharge being surrounded by a sort of burr; if the velocity of the rotation is gradually increased, the discharge as gradually assumes the white luminous character of a long spark taken from the prime conductor of an electric machine, while, if the velocity of rotation is still further increased, the luminous discharge in air will disappear, for there will then not be sufficient time between the make and break contact to magnetize the iron core, on which the intensity of the induced discharge mainly depends.

This form of the apparatus also enables the experimentalist to examine the character of the single discharge, and which, in respect to the phænomena of stratification, corroborates the results I have previously obtained and communicated to the Royal Society.

On examining the single discharge, as taken from Ritchie's coil by means of Mr. Wheatstone's revolving mirror, I found the direct spark was elongated but not divided: if a Leyden jar is introduced in the manner first suggested by Mr. Grove, the length of the spark is much reduced, but is increased in brilliancy, becoming intensely bright and dense. When this discharge is examined by Mr. Wheatstone's apparatus, it is instantly resolved into two distinct sparks, not elongated as in the direct discharge, but separated clear and defined. In my recent communication to the Royal Society, I have shown that when a Leyden jar is attached to the inner and outer terminal of the induction coil, excited by a single cell of the nitric acid battery, stratifications are observable in the dark portion of the discharge when taken through a vacuum tube, such stratifications being concave in opposite directions to those which emanate from the positive terminal of the coil. The division of the spark by Mr. Wheatstone's apparatus shows the cause of this appearance to be due to the double discharge.

The method adopted by Mr. Ritchie for the winding the wire on his secondary coils (first, I believe, suggested by Poggendorff), is the same as used by seamen in coiling ropes on the deck of a vessel: commencing with the inner circle, the coil gradually extends to the outer circumference, the next layer is continued from the outer to the inner, and this is repeated until the reel is completed. This method secures the secondary coil from the risk of disruption; but it becomes liable for the discharge to pass from the internal terminal of the secondary to the primary, even when protected by a glass cover and thick gutta percha. Mr. Ritchie has sent me a piece of a glass cover which was thus fractured; and having already starred, although not actually perforated, one of my glass covers, I have not incurred the risk of using the apparatus to its highest power.

The maximum effect that could be attained with this apparatus would, I conclude, be about 15 inches, the total length of the three secondary helices. With five cells of the nitric acid battery, each platina plate 4 × 8 inches, I obtained 10 inches spark from two coils, and 5 inches from one. In one instance, a 6-inch spark was obtained, but at the same time strong discharges took place from the inner portion of the coil to the glass cover; and there is little doubt that, had the experiment been continued, the glass would have been perforated. With three coils I have as yet only obtained a spark of $12\frac{1}{4}$ inches in length; but, for the reasons already named, I shall probably not venture to excite the coil to its greatest intensity.

I am, Gentlemen,
Your obedient Servant,
JOHN P. GASSIOT.

LX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 400.]

June 18, 1857.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the Development of *Carcinus Mænas*." By Spence Bate, Esq., F.L.S.

"On the Electro-dynamic Qualities of Metals:—Effects of Magnetization on the Electric Conductivity of Nickel and of Iron." By Professor W. Thomson, F.R.S.

I have already communicated to the Royal Society a description of experiments by which I found that iron, when subjected to magnetic force, acquires an increase of resistance to the conduction of electricity along, and a diminution of resistance to the conduction of electricity across, the lines of magnetization*. By experiments more recently made, I have ascertained that the electric conductivity of nickel is similarly influenced by magnetism, but to a greater degree, and with a curious difference from iron in the relative magnitudes of the transverse and longitudinal effects.

In these experiments the effect of transverse magnetization was first tested on a little rectangular piece of nickel 1·2 inch long, ·52 of an inch broad, and ·12 thick, being the "keeper" of the nickel horse-shoe (§ 143) belonging to the Industrial Museum of Edinburgh, and put at my disposal for experimental purposes through the kindness of Dr. George Wilson. Exactly the method described in § 175 of my previous communication referred to above, was followed; and the result, readily found on the first trial, was as stated.

The effect of longitudinal magnetization on nickel was first found with some difficulty, by an arrangement with the horse-shoe itself, and magnetizing helix (§ 143), the former furnished with suitable electrodes for a powerful current through itself, and the system treated in all respects (including cooling by streams of cold water) as described in § 156, for a corresponding experiment on iron. The

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* See Phil. Trans. Bakerian Lecture, "On the Electro-dynamic Qualities of Metals," Feb. 27, 1856, § 146 of Part 4 and Part 5. In the present communication that paper will be referred to simply by the sectional (§) numbers.

result, determined by but a very slight indication, was, as stated above, that longitudinal magnetization augmented the resistance.

The magnetization of the small piece of metal between the poles of the Ruhmkorff electro-magnet being obviously much more intense than that of the larger piece under the influence merely of the smaller helix, I recurred to the plan of experiment (§ 175) by which the effect of transverse magnetization on the little rectangular piece of nickel was first tested, and I had an equal and similar piece of iron, and another of brass, all prepared to be tested, as well as the nickel, with either longitudinal or transverse magnetic force.

To each of the little rectangles of metal to be tested, a thin slip of copper (instead of lead, as in the experiment of § 175), of the same breadth (·52 of an inch), to serve as a reference-conductor, was soldered longitudinally; and to the other end of the metal tested, a piece of copper, to serve as an electrode for the principal current, was soldered. The ends of a testing conductor, 6 feet of No. 18 copper wire, were soldered respectively to the last-mentioned end of the tested metal, and to a point in the reference-conductor, found so that the resistance between it and the junction of the reference-conductor with the testing conductor should be about equal to the resistance in the latter.

A single element, consisting of four large double cells of Daniell's (§ 63), exposing in all 10 square feet of zinc surface to 17 square feet of copper, was used to send the testing current through the conducting system thus composed, by electrodes clamped to the ends of the principal conducting channel, just outside the points of attachment of the testing conductor.

The electro-magnet was excited by various battery arrangements, in different experiments, at best by 52 cells of Daniell's, each exposing 54 square inches of zinc surface to 90 square inches of copper, and arranged in a double battery* equivalent to one battery of 26 elements each of double surface. By accident, only a single battery of 26 elements was used in obtaining the numerical results stated below.

The nickel was first placed between the flat poles of the electro-magnet, with its length across the lines of force, and, one galvanometer electrode being kept soldered to the junction of the nickel and the copper reference-conductor, the other galvanometer electrode was applied to the testing conductor till the point (equipotential with that point of junction) which could be touched without giving any deflection of the needle, was found. A multiplying branch, 3 feet of No. 18 wire, was then soldered with its ends $\frac{3}{8}$ths of an inch on each side of this point, and, as soon as the solderings were cool, the corresponding point on this multiplying branch was found. The magnetizing current was after that sent in either direction through the coils of the electro-magnet, and it was found that the moveable

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* This arrangement was found to give about the same strength of current through the coils of the electro-magnet, as a single battery of 52 of the same cells in series, and was therefore preferred as involving only half the amount of chemical action in each cell, and consequently maintaining its effect more constantly during many successive hours of use.

galvanometer electrode had to be shifted over about $4\frac{1}{2}$ inches on the multiplying branch, towards the end of the testing conductor connected with the nickel, that is to say, in such a direction as to indicate a *diminished resistance* in the nickel. When the same operations were gone through with the nickel placed longitudinally between the poles of the electro-magnet, the zero-point on the multiplying branch was shifted about 6 inches in the direction which indicated an *increased resistance* in the nickel.

The piece of iron similarly tested, gave effects in the same direction in each case, and the results originally obtained for iron (§§ 146, 155, 161–177) were thus verified.

No effect whatever could be discovered when the piece of brass was similarly tried. It is much to be desired that experiments with highly increased power, and with a better kind of galvanometer, should be made, to discover whatever very small influence is really produced by magnetic force on the comparatively non-magnetic metals.

The shifting of the neutral point on the multiplying branch, required to balance the effect produced by the longitudinal magnetization in the iron, was only from $1\frac{1}{2}$ to 2 inches. Three inches were required to balance the opposite effect of the transverse magnetization.

Hence, with the same magnetic force, the effect of longitudinal magnetization, in increasing the resistance, is from three to four times as great in nickel as in iron; but the contrary effect of transverse magnetization is nearly the same in the two metals with the same magnetic force. It may be remarked, in connexion with this comparison, that nickel was found by Faraday to lose its magnetic inductive capacity much more rapidly with elevation of temperature, and that it must consequently, as I have shown, experience a greater cooling effect with demagnetization* than iron, at the temperature of the metals in the experiment. It will be very important to test the new property for each metal at those higher temperatures at which it is very rapidly losing its magnetic property, and to test it at atmospheric temperature for cobalt, which, as Faraday discovered, actually gains magnetic inductive capacity as its temperature is raised from ordinary atmospheric temperatures, and which, consequently, must experience a heating effect with demagnetization, and a cooling effect with magnetization.

The actual amount of the effects of magnetization on conductivity, demonstrated by the experiments which have been described, may be estimated with some approach to accuracy from the preceding data. Thus the value of an inch on the multiplying branch would be the same as that of $\frac{1}{36}\times\frac{3}{4}$, or $\frac{1}{48}$ of an inch on the portion of the main testing conductor between its ends. The whole resistance of this $\frac{3}{4}$ of an inch of the main testing conductor, assisted by the attached multiplying branch of 36 inches, is of course less, in the ratio of 48 to 49, than that of any simple $\frac{3}{4}$ of an inch of the testing conductor; but in the actual circumstances there will be no

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* See Nichol's Cyclopædia of Physical Science, article "Thermo-magnetism."

loss of accuracy in neglecting so small a difference. Hence the effect of the transverse magnetization of the nickel was to diminish its resistance in the ratio of half the length of the testing conductor diminished by $\frac{4\frac{1}{2}}{48}$ of an inch, to that of the same increased by the same, that is to say, in the ratio of $11\frac{31}{32}$ to $12\frac{1}{32}$, or of 383 to 385. Hence it appears that the resistance of the nickel, when under the transverse magnetizing force, was less by $\frac{1}{192}$, and similarly, that the resistance, when under the longitudinal magnetizing force, was greater by $\frac{1}{144}$, than when freed from magnetic influence; and that the effects of the transverse and of the longitudinal magnetizing forces on the iron were to diminish its resistance and to increase its resistance by $\frac{1}{288}$ and $\frac{1}{500}$ respectively. The first effect which I succeeded in estimating (§ 155) amounted to only $\frac{1}{3000}$, being the increase of resistance in an iron wire when longitudinally magnetized by a not very powerfully excited helix surrounding it. In the recent experiments the magnetizing force was (we may infer) far greater.

It is to be remarked that the results now brought forward do not afford ground for a quantitative comparison between the effects of the same degree of magnetism, on the resistance to electric conduction along and across the lines of magnetization, in either one metal or the other, in consequence of the oblong form of the specimens used in the experiment. It is probable that in each metal, but especially in the nickel of which the specific inductive capacity is less than that of iron, the transverse magnetization was more intense than the longitudinal magnetization, since the poles of the electro-magnet were brought closer for the former than for the latter.

I hope before long to be able to make a strict comparison between the two effects for iron, at least, if not for nickel also; and to find for each metal something of the law of variation of the conductivity with magnetizing forces of different strengths.

"On the Electric Conductivity of Commercial Copper of various kinds." By Professor W. Thomson, F.R.S.

In measuring the resistances of wires manufactured for submarine telegraphs, I was surprised to find differences between different specimens so great as most materially to affect their value in the electrical operations for which they are designed. It seemed at first that the process of twisting into wire-rope and covering with gutta-percha, to which some of the specimens had been subjected, must be looked to to find the explanation of these differences. After, however, a careful examination of copper-wire strands, some covered, some uncovered, some varnished with india-rubber, and some oxidized by ignition in a hot flame, it was ascertained that none of these circumstances produced any sensible influence on the whole resistance; and it was found that the wire-rope prepared for the Atlantic cable (No. 14 gauge, composed of seven No. 22 wires, and weighing altogether from 109 to 125 grains per foot) conducted about as well, on the average, as solid wire of the same mass: but, in the larger collection of specimens which thus came to be tested, still greater differences in conducting power were discovered than any previously observed. It appeared now certain that these differences were owing to different qualities of the copper wire itself, and it became important to find how wire of the best quality could be procured. Accordingly, samples of simple No. 22 wire, and of strand spun from it, distinguished according to the manufactories from which they were supplied, were next tested, and the following results were obtained:—

Table of relative conducting qualities of single No. 22 Copper wire, supplied from manufactories A, B, C, D.

	Resistances of equal lengths.	Weights of seven feet.	Resistances reduced to equal conducting masses and lengths.	Conducting power (reciprocals of resistances) of equal and similar masses.
A..	100	121·2 grs.	100	100
B..	100·2	125·8 „	104·0	96·05
C..	111·6	120·0 „	110·5	90·5
D..	197·6	111·7 „	182·0	54·9

The strands spun from wire of the same manufactories showed nearly the same relative qualities, with the exception of an inversion as regards the manufactories B and D, which I have been led to believe must have arisen from an accidental change of labels before the specimens came into my hands.

Two other samples chosen at random about ten days later, out of large stocks of wire supplied from each of the same four manufactories, were tested with different instruments, and exhibited, as nearly as could be estimated, the same relative qualities. It seems, therefore, that there is some degree of constancy in the quality of wire supplied from the same manufactory, while there is vast superiority in the produce of some manufactories over that of others. It has only to be remarked, that *a submarine telegraph constructed with copper wire of the quality of the manufactory* A *of only* $\frac{1}{21}$ *of an inch in diameter, covered with gutta-percha to a diameter of a quarter of an inch, would, with the same electrical power, and the same instruments, do more telegraphic work than one constructed with copper wire of the quality* D, *of* $\frac{1}{16}$ *of an inch diameter, covered with gutta-percha to a diameter of a third of an inch,* to show how important it is to shareholders in submarine telegraph companies that only the best copper wire should be admitted for their use. When the importance of the object is recognized, there can be little difficulty in finding how the best, or nearly the best, wire is to be uniformly obtained, seeing that all the specimens of two of the manufactories which have as yet been examined have proved to be of the best, or little short of the best quality, while those of the others have been found inferior in nearly constant proportion.

What is the cause of these differences in electrical quality is a question not only of much practical importance, but of high scientific interest. If chemical composition is to be looked to for the explanation, very slight deviations from perfect purity must be sufficient to produce great effects on the electric conductivity of copper; the following being the results of an assay by Messrs. Matthey and Johnson, made on one of the specimens of copper wire which I had found to be of low conducting power:—

Copper	99·75
Lead	·21
Iron	·03
Tin or antimony	·01
	100·00

The whole stock of wire from which the samples experimented on were taken, has been supplied by the different manufacturers as remarkably pure; and being found satisfactory in mechanical qualities, had never been suspected to present any want of uniformity as to value for telegraphic purposes, when I first discovered the difference in conductivity referred to above. That even the worst of them are superior in conducting power to some other qualities of commercial copper, although not superior to all ordinary copper wire, appears from the following set of comparisons which I have had made between specimens of the No. 22 A wire, ordinary copper wire purchased in Glasgow, fine sheet-copper used in blocks for calico-printers, and common sheet-copper.

Lengths of No. 22 A, weighing 17·3 grs. per foot, used as standards.	Conductors tested.	Their weights per foot.	Lengths resisting as much as standards if of equal conductivity.	Lengths found by experiment to resist as much as standards.	Conductivity referred to that of No. 22 A as 100.
inches.		grs.	inches.	inches.	
23·8	Ordinary No. 18 wire	57·5	79·0	73·6	93·2
7·5	Slip of fine sheet-copper	37·6	16·3	9·1	55·8
15·5	Slip of common sheet-copper	51·1	45·77	15·6	34·1

To test whether or not the mechanical quality of the metal as to hardness or temper had any influence on the electrical conducting power, the following comparison was made between a piece of soft No. 18 wire, and another piece of the same pulled out and hardened by weights applied up to breaking.

Soft No. 18 copper wire.	No. 18 copper wire, stretched to breaking.	Length found equivalent by experiment.
Weight per foot, 57·5 grs. Length used, 30·8 inches.	Weight per foot, 44·8 grs. Equivalent length, if of equal conductivity, 24·0 inches.	24·0 inches.

The result shows that the greatest degree of brittleness produced by tension does not alter the conductivity of the metal by as much as one half per cent. A similar experiment showed no more sensible effect on the conductivity of copper wire to be produced by hammering it flat. There are, no doubt, slight effects on the conductivity of metals, produced by every application and by the altered condition left after the withdrawal of excessive stress*; and I have already made a partial examination of these effects in copper, iron, and platinum wires, and found them to be in all cases so minute, that the present results as to copper wire are only what was to be expected.

To find whether or not there is any sensible loss of conducting power on the whole due to the spiral forms given to the individual wires when spun into a strand, it would be well worth while to compare very carefully the resistances of single wires with those of strands spun from exactly the same stock. This I have not yet had an opportunity of doing; but the following results show that any deficiency which the strand may present when accurately compared with solid wire, is nothing in comparison with the differences presented by different samples chosen at random from various stocks of solid wire and strand in the process of preparation for telegraphic purposes.

No. 16 Solid Wire. Pairs of samples in different states of preparation, each 1000 inches long.

Resistances†.			Weights per foot.	Specific resistances reduced to British absolute measure.
			grs.	
Not covered....	E_1 ·2036 E_2 ·1995	·2015	74·6	11,850,000
Once covered ..	F_1 ·2054 F_2 ·1999	·2026	77·55	12,410,000
Twice covered..	G_1 ·1963 G_2 ·1963	·1963	77·2	11,970,000
Thrice covered..	H_1 ·1893 H_2 ·1916	·1904	77·73	11,680,000
	Means....	·1977	76·78	11,980,000

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* See the Bakerian Lecture, "On the Electro-dynamic Qualities of Metals," §§ 104, 105 and 150, Philosophical Transactions for 1856.

† These resistances were measured, by means of a Joule's tangent galvanometer with a coil of 400 turns of fine wire, in terms of the resistance of a standard conductor as unity. The resistance of this standard has been determined for me in absolute measure through the kindness of Professor W. Weber, and has been found to be 20,055,000 German units $\left(\frac{\text{metre}}{\text{seconds}}\right)$, or 6,580,000 British units $\left(\frac{\text{foot}}{\text{seconds}}\right)$. The numbers in the last column, headed "Specific resistances reduced to British measure," express the resistances of conductors composed of ten different qualities of metal, and each one foot long and weighing one grain. I is impossible to over-estimate the great practical value of this system of absolute measurement carried out by Weber into every department of electrical science, after its first introduction into the observations of terrestrial magnetism by Gauss. See "Messungen galvanischen Leitungswiderstände nach einem absoluten Maasse," Poggendorff's Annalen, March 1851. See also the author's articles entitled "On the Mechanical Theory of Electrolysis," and "Application of the Principle of Mechanical Effect to the Measurement of Electromotive Force, and of Galvanic Resistances in Absolute Units," Philosophical Magazine, December 1851.

No. 14 Gauge Strand (seven No. 22 wires twisted together). Pairs of samples in different states of preparation, each 1000 inches long.

Resistances.			Weights per foot.	Specific resistances reduced to British absolute measure.
			grs.	
Not covered....	K_1 ·1595 K_2 ·1634	·1614	115·82	14,750,000
Once covered ..	L_1 ·1037 L_2 ·1043	·1040	109·37	8,964,000
Twice covered..	M_1 ·1426 M_2 ·1424	·1425	111·95	12,590,000
Thrice covered..	N_1 ·1092 N_2 ·1085	·1088	121·30	10,430,000
	Means....	·1297	114·61	11,680,000

The specific resistances of the specimens of copper wire from the manufactories A, B, C, D, of which a comparative statement is given in the first Table above, I have estimated in absolute measure by comparing each with F_2, of which the resistance in absolute measure is 6,580,000 × ·1999, or 1,316,000. The various results reduced to specific resistances per grain of mass per foot of length are collected in the following Table, and shown in order of quality in connexion with four determinations of specific conductivity by Weber.

Specific Conductivities of specimens of Copper expressed in British Absolute Measure.

Description of Metal.	Specific resistances.
Copper wire A No. 22	7,600,000
Wire of electrolytically precipitated copper : Weber (1)	7,924,000
Copper wire B No. 22	7,940,000
Ordinary No. 18 copper wire	8,100,000
Copper wire C No. 22	8,400,000
Weber's copper wire : Weber (2)............	8,778,000
No. 14 strand specimen, once covered	8,960,000
Kirchhoff's copper wire : Weber (3)	9,225,000
No. 14 strand specimen, thrice covered	10,400,000
Jacobi's copper wire : Weber (4)............	10,870,000
No. 16 wire specimen, thrice covered	11,700,000
Ditto, twice covered	11,970,000
Ditto, not covered.........................	11,850,000
Ditto, once covered	12,410,000
No. 14 strand specimen, twice covered........	12,590,000
Slip of fine sheet-copper	13,600,000
Copper wire D No. 22	13,800,000
No. 14 strand specimen, not covered	14,750,000
Slip of common sheet-copper	22,300,000

"On the Thermal Effects of Fluids in Motion:—Temperature of a Body moving slowly through Air." By Prof. W. Thomson, F.R.S., and J. P. Joule, Esq., F.R.S.

The motion of air in the neighbourhood of a body moving very slowly through it, may be approximately determined by treating the problem as if air were an incompressible fluid. The ordinary hydrodynamical equations, so applied, give the velocity and the pressure of the fluid at any point; and the variations of density and temperature actually experienced by the air are approximately determined by using the approximate evaluation of the pressure thus obtained. Now, if a solid of any shape be carried uniformly through a perfect liquid*, it experiences fluid-pressure at different parts of its surface, expressed by the following formula,—

$$p=\Pi+\tfrac{1}{2}\rho\,(V^2-q^2),$$

where Π denotes the fluid-pressure at considerable distances from the solid, ρ the mass of unity of volume of the fluid, V the velocity of translation of the solid, and q the velocity of the fluid relatively to the solid, at the point of its surface in question. The effect of this pressure on the whole is, no resultant force, and only a resultant couple which vanishes in certain cases, including all in which the solid is symmetrical with reference to the direction of motion. If the surface of the body be everywhere convex, there will be an augmentation of pressure in the fore and after parts of it, and a diminution of pressure round a medium zone. There are clearly in every such case just two points of the surface of the solid, one in the fore part, and the other in the after part, at which the velocity of the fluid relatively to it is zero, and which we may call the fore and after pole respectively. The middle region round the body in which the relative velocity exceeds V, and where consequently the fluid pressure is diminished by the motion, may be called the equatorial zone; and where there is a definite middle line, or line of maximum relative velocity, this line will be called the equator.

If the fluid be air instead of the ideal "perfect liquid," and if the motion be slow enough to admit of the approximation referred to above, there will be a heating effect on the fore and after parts of the body, and a cooling effect on the equatorial zone. If the dimensions and the thermal conductivity of the body be such that there is no sensible loss on these heating and cooling effects by conduction, the temperature maintained at any point of the surface by the air flowing against it, will be given by the equation

$$t=\Theta\left(\frac{p}{\Pi}\right)^{\frac{\cdot 41}{1\cdot 41}},$$

where Θ denotes the temperature of the air as uninfluenced by the motion, and p and Π denote the same as before†. Hence, using for

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* That is, as we shall call it for brevity, an ideal fluid, perfectly incompressible and perfectly free from mutual friction among its parts.

† The temperatures are reckoned according to the absolute thermodynamic scale which we have proposed, and may, to a degree of accuracy correspondent with that of the ordinary "gaseous laws," be taken as temperature Centigrade by

p its value by the preceding equation, we have

$$t=\Theta\left\{1+\frac{\rho}{2\Pi}\left(\mathrm{V}^2-q^2\right)\right\}^{\frac{\cdot 41}{1\cdot 41}}$$

But if H denote the length of a column of homogeneous atmosphere, of which the weight is equal to the pressure on its perpendicular section, and if g denote the dynamical measure of the force of gravity (32·2 in feet per second of velocity generated per second), we have

$$g\rho\mathrm{H}=\Pi;$$

and if we denote by α the velocity of sound in the air, which is equal to $\sqrt{1\cdot 41\times g\mathrm{H}}$, the expression for the temperature becomes

$$t=\Theta\left\{1+\frac{1\cdot 41}{2}\cdot\frac{\mathrm{V}^2-q^2}{\alpha^2}\right\}^{\frac{\cdot 41}{1\cdot 41}}.$$

According to the supposition on which our approximation depends, that the velocity of the motion is small, that is, as we now see, a small fraction of the velocity of sound, this expression becomes

$$t=\Theta\left\{1+\cdot 41\times\frac{\mathrm{V}^2-q^2}{2\alpha^2}\right\}.$$

At either the fore or after pole, or generally at every point where the velocity of the air relatively to the solid vanishes (at a re-entrant angle for instance, if there is such), we have $q=o$, and therefore a elevation of temperature amounting to

$$\cdot 41\times\frac{\mathrm{V}^2}{2\alpha^2}\Theta.$$

If, for instance, the absolute temperature, Θ, of the air at a distance from the solid be 287° (that is 55° on the Fahr. scale), for which the velocity of sound is 1115 per second, the elevation of temperature at a pole, or at any point of no relative motion, will be, in degrees Centigrade,

$$58^\circ\cdot 8\times\left(\frac{\mathrm{V}}{\alpha}\right)^2,\text{ or }58^\circ\cdot 8\times\left(\frac{\mathrm{V}}{1115}\right)^2,$$

the velocity V being reckoned in feet per second. If, for instance, the velocity of the body through the air be 88 feet per second (60 miles an hour), the elevation of temperature at the points of no relative motion is ·36°, or rather more than $\frac{1}{3}$ of a degree Centigrade.

To find the greatest depression of temperature in any case, it is necessary to take the form of the body into account. If this be spherical, the absolute velocity of the fluid backwards across the equator will be half the velocity of the ball forwards; or the relative velocity (q) of the fluid across the equator will be $\frac{3}{2}$ of the velocity of the solid. Hence the depression of temperature at the equator of a sphere moving slowly through the air will be just $\frac{9}{4}$ of the elevation of temperature at each pole. It is obvious from this that a

the air-thermometer, with 273°·7 added in each case. See the author's previous paper "On the Thermal Effects of Fluids in Motion," Part II., Philosophical Transactions, 1851, part 2. p. 353.

spheroid of revolution, moving in the direction of its axis, would experience at its equator a depression of temperature, greater if it be an oblate spheroid, or less if it be a prolate spheroid, than $\frac{9}{4}$ of the elevation of temperature at each pole.

It must be borne in mind, that, besides the limitation to velocities of the body small in comparison with the velocity of sound, these conclusions involve the supposition that the relative motions of the different parts of the air are unresisted by mutual friction, a supposition which is not even approximately true in most cases that can come under observation. Even in the case of a ball pendulum vibrating in air, Professor Stokes* finds that the motion is seriously influenced by fluid friction. Hence with velocities which could give any effect sensible on even the most delicate of the ether thermometers yet made (330 divisions to a degree), it is not to be expected that anything like a complete verification or even illustration of the preceding theory, involving the assumption of no friction, can be had. It is probable that the forward polar region of heating effect will, in consequence of fluid friction, become gradually larger as the velocity is increased, until it spreads over the whole equatorial region, and does away with all cooling effects.

Our experimental inquiry has hitherto been chiefly directed to ascertain the law of the thermal effect upon a thermometer rapidly whirled in the air. We have also made some experiments on the modifying effects of resisting envelopes, and on the temperatures at different parts of the surface of a whirled globe. The whirling apparatus consisted of a wheel worked by hand, communicating rapid rotation to an axle, at the extremity of which an arm carrying the thermometer with its bulb outwards was fixed. The distance between the centre of the axle and the thermometer bulb was in all the experiments 39 inches. The thermometers made use of were filled with ether or chloroform, and had, the smaller 275, and the larger 330 divisions to the degree Centigrade. The lengths of the cylindrical bulbs were $\frac{9}{10}$ and $1\frac{4}{10}$ inch, their diameters ·26 and ·48 of an inch respectively.

TABLE I.—Small bulb Thermometer.

Velocity in feet per second.	Rise of temperature in divisions of the scale.	Rise divided by square of velocity.
46·9	27½	·0125
51·5	32	·0121
68·1	46½	·0100
72·7	57½	·0109
78·7	67½	·0109
84·8	74	·0103
104·5	91	·0083
130·2	151	·0089
133·2	172	·0097
145·4	191	·0090
	Mean..	·01026

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* "On the Effect of the Internal Friction of Fluids on the Motion of Pendulums," read to the Cambridge Philosophical Society, Dec. 9, 1850, and published in vol. ix. part 2 of their Transactions.

The above Table shows an increase of temperature nearly proportional to the square of the velocity.

$V=\sqrt{\frac{275}{\cdot 01026}}=163\cdot 7=$the velocity in feet per second, which, in air of the same density, would have raised the temperature 1° Centigrade.

Table II.—Larger bulb Thermometer.

Velocity in feet per second.	Rise of temperature in divisions of scale.	Rise divided by square of velocity.
36·3	18	·0125
66·6	42	·0095
84·8	57	·0079
125·6	146	·0093
	Mean..	·0098

In this instance $V=\sqrt{\frac{330}{\cdot 0098}}=183\cdot 5$ feet per second for 1° Centigrade. It is however possible that the full thermal effect was not so completely attained in three minutes (the time occupied by each whirling) as with the smaller bulb. On the whole it did not appear to us that the experiments justified the conclusion, that an increase of the dimensions of the bulb was accompanied by an alteration of the thermal effect.

Table III.—Larger bulb Thermometer covered with five folds of writing-paper.

Velocity in feet per second.	Rise of temperature in divisions of scale.	Rise divided by square of velocity.
36·3	20	·0152
51·5	43	·0162
72·6	53	·0101
118	132	·0095

The increased thermal effect at comparatively slow velocities, exhibited in the above Table, appeared to be owing to the friction of the air against the paper surface being greater than against the polished glass surface.

One quarter of the enveloping paper was now removed, and the bulb whirled with its bared part in the rear. The results were as follow:—

Table IV.—Paper removed from posterior side.

Velocity in feet per second.	Rise of temperature in divisions of scale.	Rise divided by square of velocity.
75·6	60	·0105
96·8	87	·0093

On whirling in the contrary direction, so that the naked part of the bulb went first, we got,—

Table V.—Paper removed from anterior side.

Velocity in feet per second.	Rise of temperature in divisions of scale.	Rise divided by square of velocity.
81·7	56	·0084
93·8	72	·0082

On rotating with the bare part, posterior and anterior in turns, at the constant velocity of 90 feet per second, the mean result did not appear to indicate any decided difference of thermal effect.

Another quarter of paper was now removed from the opposite side. Then on whirling so that the bared parts were anterior and posterior, we obtained a rise of 83 divisions with a velocity of 93·8. But on turning the thermometer on its axis one quarter round, so that the bared parts were on each side, we found the somewhat smaller rise of 62 divisions for a velocity of 90·8 feet per second.

The effect of surface friction having been exhibited at slow velocities with the papered bulb, we were induced to try the effect of increasing it by wrapping iron wire round the bulb.

Table VI.—Larger bulb Thermometer wrapped with iron wire.

Velocity in feet per second.	Rise in divisions of scale.	Rise divided by square of velocity.
15·36	10·25	·0434
23·04	33	·0623
30·71*	49·25	·0522
46·08	68·75	·0324
69·12	98	·0206
111·34	185	·0149
126·72	207	·0129
153·55	above 280	above ·0118

On inspecting the above Table, it will be seen that the thermal effect produced at slow velocities was five times as great as with the bare bulb. This increase is evidently due to friction. In fact, as one layer of wire was employed, and the coils were not so close as to prevent the access of air between them, the surface must have been about four times as great as that of the uncovered bulb. At high velocities, it is probable that a cushion of air which has not time to escape past resisting obstacles makes the actual friction almost independent of variations of surface, which leave the magnitude of the body unaltered. In conformity with this observation, it will be seen that at high velocities the thermal effect was nearly reduced to the quantity observed with the uncovered bulb. Similar remarks apply to the following results obtained after wrapping round the bulb a fine spiral of thin brass wire.

Table VII.—Bulb wrapped with a spiral of fine brass wire.

Velocity in feet per second.	Rise in divisions of scale.	Rise divided by square of velocity.
7·68	2·5	·0424
15·36	13·5	·0572
23·04	36·5	·0687
30·71	48	·0509
46·08	64·5	·0304
76·8	103·5	·0175
115·18	224·5	·0169
148·78	264	·0119

The thermal effects on different sides of a sphere moving through

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* The whirring sound began at this velocity. According to its intensity the thermal effect must necessarily suffer diminution; unless indeed it gives rise to increased resistance.

air, have been investigated by us experimentally by whirling a thin glass globe of 3·58 inches diameter along with the smaller thermometer, the bulb of which was placed successively in three positions, viz. in front, at one side, and in the rear. In each situation it was placed as near the glass globe as possible without actually touching it.

Table VIII.—Smaller Thermometer whirled along with glass globe.

Velocity in feet per second.	Rise in divisions of scale.		
	Therm. in front.	Therm. at side.	Therm. in rear.
3·84	·66	10	4
7·68	2·66	40	10·5
15·36	41·9	78	51
23·04	71·2	90	71·7
38·4	78·4	90	68
57·5	99·9	112	76
70·92			107

The effects of fluid friction are strikingly evident in the above results, particularly at the slow velocities of 3 and 7 feet per second. It is clear from these, that the air, after coming in contact with the front of the globe, traverses with friction the equatorial parts, giving out an accumulating thermal effect, a part of which is carried round to the after pole. At higher velocities the effects of friction seem rapidly to diminish, so that at velocities between 23 and 38 feet per second, the mean indication of thermometers placed all round the globe would be nearly constant. Our anticipation (written before these latter experiments were made), that a complete verification of the theory propounded at the commencement was impossible with our present means, is thus completely justified.

It may be proper to observe, that in the form of experiment hitherto adopted by us, the results are probably, to a trifling extent, influenced by the vortex of air occasioned by the circular motion.

We have on several occasions noticed the effect of sudden changes in the force of wind on the temperature of a thermometer held in it. Sometimes the thermometer was observed to rise, at other times to fall, when a gust came suddenly on. When a rise occurred, it was seldom equivalent to the effect, as ascertained by the foregoing experiments, due to the increased velocity of the air. Hence we draw the conclusion, that the actual temperature of a gust of wind is lower than that of the subsequent lull. This is probably owing to the air in the latter case having had its *vis viva* converted into heat by collision with material objects. In fact we find that in sheltered situations, such for instance as one or two inches above a wall opposite to the wind, the thermometer indicates a higher temperature than it does when exposed to the blast. The question, which is one of great interest for meteorological science, has hitherto been only partially discussed by us, and for its complete solution will require a careful estimate of the temperature of the earth's surface, of the effects of radiation, &c., and also a knowledge of the causes of gusts in different winds.

GEOLOGICAL SOCIETY.

[Continued from p. 404.]

March 10, 1858.—Prof. Phillips, President, in the Chair.

The following communication was read :—

"On the Gold-diggings at Ballaarat." By H. Rosales, Esq. (In a letter to W. W. Smyth, Esq., Sec. G.S.)

"By the aid of machinery, and through the alteration of the mining regulations granting extended claims, the old ground has been profitably re-worked; and, by the introduction of the frontage-system, which, according to the difficulties to be overcome, grants extensive claims on new ground, the present 'leads,' most of which are N.W. of the Gravel Pits, under the townships, are advantageously worked. The amalgamation of three or more claims is also allowed, the miners having then to put down only one shaft.

"The engines most in use are stationary, of from 15 to 20 horse-power, with winding and reversing gear. To the end of the winding-gear-shaft is attached the crank for the pump, and the motion is also taken to drive a puddling machine, which is nothing but the *arrastra* working without mercury. The depth of sinking averages about 300 feet, of which in some instances there are as much as 200 of basalt to be cut through.

"At the junctions of the Frenchman's and White Horse Leads, in the Eldorado, the remains of a tree were found in an undisturbed position, with the roots fast in the wash-dirt; and it might be interesting to you to know that at Poverty Point the deep channel, with a N.W. strike, is crossed at about 140 feet higher by the shallow channel, which has a strike of N.E. by E., and which again, in its turn, is crossed, at a level of 20 or 30 feet still higher, by the present water-course, the strike of which is W."

March 24.—Prof. Phillips, President, in the Chair.

The following communications were read :—

1. "On a protrusion of Silurian Rock in the North of Ayrshire." By J. C. Moore, Esq., F.R.S., F.G.S.

The author described the coast-section of a part of North Ayr, from Ardrossan to Goldenberry Hill, north of Portencross Castle, altogether about five miles in length; and he showed that the red sandstones (of Devonian age) that succeeded the coal-measures of Ardrossan, from which they are cut off by a trap-dyke, dip southwardly for upwards of two miles, then form a low anticlinal, and are then vertical for a short distance; after which they are abruptly succeeded by a contorted and arched mass of purple, green, and black schists of Silurian age. The schists are immediately succeeded on the north by unconformable red sandstones and conglomerates, dipping northward, cut through by greenstone, and apparently belonging to a lower stage than that of the red sandstones on the south of the axis; sandstones similar to the latter succeed, with a conformable dip, and are traversed by a porphyry which forms a massive covering above them, and constitutes the picturesque hill of Goldenberry.

The author remarked that an axis of elevation has disturbed the Old Red sandstone, Coal-measures, and Permian sandstone in the

Isle of Arran, at a point coincident with the above-described protruded mass of Silurian rock, ten miles distant; and he pointed out that the axes of the successive movements of palæozoic rocks of Scotland appeared to correspond with this and with each other, in having an E.N.E. strike.

2. "On the Rock-basins in the Granite of Dartmoor." By G. W. Ormerod, Esq., M.A., F.G.S.

The district examined by the author may be regarded as a central belt of this granitic district, occupying about one-third of Dartmoor, and extending over the parishes of Gidleigh, Chagford, Morton Hampstead, North Bovey, Withycombe in the Moor, Manaton, Ilington, and Bridford. Mr. Ormerod has examined most of the rock-basins occurring in this district himself, and made plans of them from measurement. In describing them, he divided the district into plots, by rivers and watersheds; and he gave a slight notice of each tor, if it possessed any features of interest. He also stated the measurements of the basins from N. to S. and from E. to W., for comparison; as well as their characters, depths, &c., and the direction of their longest diameter. Vixen Tor, Mis Tor, Hounter Tor, the Tolmen, Kestor Rock, the Puckie Stone, Middleton Hill, the Logan Stone, Hell Tor, Ingstone Rock, and Yes Tor were more especially described; and the heights above the sea-level,—the presence or absence of basins on the tors—and the relative shapes and sizes of the basins were shown by tables.

After observing that though probably in some cases used by the "Druids," yet the basins were certainly not formed artificially, Mr. Ormerod proceeded to explain the differences between the "rock-basins" of the tors, and the "potholes" in the granite of the bed of the Teign; and to show that atmospheric causes alone had caused the formation of the former; and he pointed out the peculiar kinds of granite that were liable to be so decomposed as to present either saucer-shaped, flat-bottomed, or other shaped basins on the surface.

3. "On the Kelloways Rock of the Yorkshire Coast." By J. Leckenby, Esq.

The author traced this interesting deposit from a short distance to the south of Gristhorpe Bay, where it is a very thin pisolitic band, to Red Cliff, where it presents upwards of 20 feet of sandstones, the upper part being fossiliferous. At other points also, near Scarborough Pier, at the Castle, and inland at Oliver's Mount, the Kelloways Rock has been found to yield numerous fossils, a list of which the author appended, together with descriptions of some new or little-known species of Ammonites from this deposit.

April 14.—Prof. Phillips, President, in the Chair.

The following communications were read:—

1. "On the occurrence of *Graphularia Wetherellii* in nodules from the London Clay and the Crag." By N. T. Wetherell, Esq., M.R.C.S.

The author, having briefly explained the form and general characters of the Pennatula-like fossil named *Graphularia Wetherellii* by MM. Milne-Edwards and Haime, described the circumstances under which he had lately found portions of the stem of this coral traversing nodules in the London Clay of Highgate. The nodules are of different sizes, and vary from a cylindrical to an ovoidal shape. Mr. Wetherell having also observed unmistakeable fragments of the same *Graphularia* holding the same relative position in nodules from the Red Crag, differing from those of the London Clay only in having been more rounded and polished by aqueous action, brought the subject before the Society as a link in the chain of evidence of the so-called "coprolitic" or "phosphatic" nodules of the Red Crag having been to a great extent derived from the destruction of the London Clay. The author also offered some observations on the structure of other nodular bodies in the London Clay and in other deposits.

2. "On the Extraneous Fossils of the Red Crag." By S. V. Wood, Esq., F.G.S.

In pursuing his investigations as to the geologic age of the Crag, the author found it necessary to decide as to what are the "derivative" fossils which are mixed with the native fossils in this deposit. This has been a matter of some difficulty. Mr. Wood enumerates the following genera that may have supplied species or specimens to the Red Crag:—*Chama, Cardita, Astarte, Cyprina, Isocardia, Limopsis, Turritella, Vermetus, Cancellaria, Terebra, Voluta,* and *Pyrula*; and he observes that out of 240 species of mollusca found in the Red Crag, and belonging truly to a modern tertiary period, forty, or perhaps fifty, might be considered as derivative fossils, though possibly some of them may have lived on from the period of the Lower to that of the Upper or Red Crag. Some few extraneous fossils found in the Red Crag appear to the author to have been casts of shells from some freshwater deposit, probably of old tertiary date, but of which no other trace has been recognized. Mr. Wood regards the relics of terrestrial mammals found in the Red Crag as extraneous to that deposit, as well as the Cetotolites or relics of whales. Remains of two species of *Hyracotherium,* originally derived from the London Clay, have occurred in this Crag; also a tooth said to be of a *Coryphodon.* The remains of *Ursus, Canis, Vulpes, Felis, Trogontherium, Mastodon, Rhinoceros, Equus, Sus,* and *Cervus* have been derived from some Upper Tertiary deposits; but Mr. Wood is uncertain whether the Crag relics of *Hippotherium* and *Hyænodon* may not have come from some Middle Tertiary beds. The remains of *Balænodon* certainly were not derived from the London Clay, as has been suggested, but, with the *Delphinus,* have come from some tertiaries of a later date. Crocodile, Turtle, and Snake remains, fossil wood, crustaceans, and mollusca have come from the London Clay. Many of the fish-remains are London Clay fossils; but some have been washed in from beds similar to those of Bracklesham; and some, including the teeth of *Carcharodon megalodon,* have apparently been derived from Middle Tertiary beds not existing now in this area. Some Chalk fossils, and several fossils from the Middle Oolites, have also been collected in the "phosphatic beds." Mr. Wood described the relative proportions in which the different derivative fossils occurred, and made some remarks on the general characters of these beds. He observes that probably no locality more nearly represents the conditions of the Red Crag Sea during the accumulation of the materials of its bed than the existing Bay of Christchurch, where the conflicting tidal currents mix the fossils of several beds with recent shells.

MANCHESTER SOCIETY.

March 9, 1858.—W. Fairbairn, F.R.S. &c., President, in the Chair.

A paper was read by the President on "Experiments to determine the strength of some alloys of Nickel and Iron, similar in composition to Meteoric Iron."

The object of the experiments in this paper was to ascertain whether an infusion of nickel, in a given proportion, would increase the tenacity of cast iron, as originally imagined from the analysis of meteoric iron, found to contain 2½ per cent. of nickel. Contrary to expectation, the cast iron, when mixed with the precise quantity of nickel, indicated by the analysis of meteoric iron, lost considerably in point of strength instead of gaining by it. Hopes were entertained that increased toughness and ductility would be the result of the mixture; but the experiments which follow clearly show that there is a diminution in place of an increase of strength.

From the first class of experiments recorded in the paper, it appears that the nickel was prepared from the ore, and melted in the crucible, as follows:—

30 lbs. of roasted ore.
5 lbs. of pure sand.
2 lbs. of charcoal.
2 lbs. of lime.

These were kept six hours in the furnace, and after being separated from the slag, the metal was cooled and remelted with ½ lb. of roasted ore and ¼ lb. of pure bottle glass; about 25 per cent. of nickel was obtained. Two and a half per cent. of this was fused with Blaenavon No. 3 pig iron, and run into ingots, or bars, which were then subjected to experiment as follows:—

Results derived from 1-inch square bars, subjected to a transverse strain 2 feet 3 inches between the supports.

Description of Iron.	Breaking weight in lbs. (B.)	Ultimate deflection in inches. (D.)	Power of resisting impact. (B×A.)	Comparative strength, Blaenavon representing 1000.
Exp. I. Bar D. Pure Blaenavon, No.3	1131	·75	848·2	1000
„ II. „ C. Blaenavon, No. 3 Nickel	875	·58	507·5	773
„ III. „ B. Pure Cast Iron, No. 1...	861	·47	404·7	761
„ IV. „ A. Cast Iron, No. 1 Nickel	637	·43	276·4	563
„ V. „ E. Pontypool, pure No. 1...	798	·36	292·1	705
Mean	860	·52	465·8	760·2

From the above there appears to be a loss of 22 to 26 per cent. as compared with the Blaenavon No. 3 iron; and in the next series of experiments made upon similar mixtures, but with perfectly pure nickel, the same indications of loss are apparent, but not to the same extent as in the preceding, as may be seen from the following results:—

Description of Iron.	Breaking weight in lbs. (B.)	Deflection in inches. (D.)	B × A, or power to resist impact.	Rates of strength, F2=1000.
Exp. VI. Bar F 1 without Notches	867	·315	273	1000 : 876
" VII. " F 2 "	989	·380	376	1000 : 1000
" VIII. " G 1 with one Notch	760	·331	231	1000 : 768
" IX. " G 2 "	899	·410	368	1000 : 908
" X. " H 1 with two Notches	746	·286	213	1000 : 754
" XI. " H 2 "	703	·290	203	1000 : 810
Mean	829	·335	280	1000 : 838

Taking the mean of these experiments, it will be observed that the loss of strength is not so great as in the former, it being about 17 per cent., or as 100:83. In the deflections and the power to resist impact, they are, however, inferior to those first experimented upon, as may be seen by the numbers, in the ratio of 465 : 280. This in some degree neutralizes the measure of strength, by a proportionate diminution of elasticity of the bars employed in the last experiments.

At the commencement of the paper, the author stated that the experiments were undertaken for the purpose of ascertaining how far, and to what extent, an admixture of nickel would improve cast iron; and that that improvement had reference, independent of other objects, to increased tenacity in the metal employed for the casting of mortars and heavy ordnance.

During the last two years innumerable tests and experiments have been made for that purpose with more or less success; but the ultimate result appeared to be, in the opinion of the author and others, that for the casting, or rather the construction, of heavy artillery, there is no metal so well calculated to resist the action of gunpowder as a perfectly homogeneous mass of *the best and purest cast iron* when freed from sulphur and phosphorus.

In the discussion which followed the reading of the paper, Mr. Calvert said that it was highly probable that nickel caused the increased brittleness of cast iron, just as carbon, phosphorus, and sulphur, but that the result with malleable iron might probably be very different; and as meteoric iron is malleable, the trial could only be complete when soft iron and nickel were united; nevertheless these experiments, as far as cast iron is concerned, were decidedly new and of great value.

LXI. *Intelligence and Miscellaneous Articles.*

ON THE TORPIDITY OF THE MARMOT. BY G. VALENTIN.

THE object of this memoir of M. Valentin is to examine the influence of the winter-sleep upon the production of glucose by the liver. During an abstinence from food of five or six months, the sugar is persistent in the liver of the Marmot; from this it follows that there is an essential difference between the true winter-sleep of the Marmot and the torpidity of the Batrachia, or the state of inanition of waking animals.

When, as is sometimes the case, the death of the animal is caused by exhaustion at the end of the winter-sleep, the liver no longer contains sugar. The same fact is observed in Hedgehogs which have died during their winter-sleep. On the contrary, when a healthy Marmot, killed at the end of its torpidity, is examined, it is found that the fresh blood of the aorta and the fresh urine will precipitate small quantities of protoxide of copper, showing that they contain glucose.

Some authors have expressed the opinion that the liquid secreted by the stomach is absorbed, and that after passing through the vena porta, it produces sugar in the liver. M. Valentin opposes this view, and cites several facts which speak against it.

The author has observed a striking difference between the sugar of the liver of Marmots in their winter-sleep and that of other waking animals; the former is not so readily destroyed by putrefaction as the latter.

In conclusion he cites an observation made upon some frogs which had passed four months of the winter in a dark cellar. They were frozen by exposure to a temperature of $+5°$ F.; the sugar of their livers did not disappear.—Moleschott's *Untersuchungen*, vol. iii.

ON THE NITRURETS OF TUNGSTEN AND MOLYBDENUM.
BY PROFESSOR WÖHLER.

If one of the chlorides of tungsten or molybdenum be placed in the closed end of a long glass tube, with dried fragments of muriate of ammonia above it, and the tube be heated to redness, first in the empty part, and afterwards at the end, so that the two salts may be volatilized and exposed to a red heat whilst mixed in a gaseous form, a complete mutual decomposition takes place, and after all excess of muriate of ammonia has been driven off, the entire inner wall of the tube is found coated with a speculum of a black, semi-metallic substance, which may be removed partly in brittle crusts, and partly in black powder. It has the same aspect with both metals, and consists either of the nitruret of the metal, or of one of the amide compounds of these metals. When heated in the air it burns into tungstic or molybdic acid. When fused with hydrate of potash it forms a great quantity of ammonia.—Liebig's *Annalen*, February 1858, p. 258.

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LXII. *On the Chemical Theory of Gunpowder.*
By R. BUNSEN *and* L. SCHISCHKOFF[*].

[With a Plate.]

ALTHOUGH the combustion of gunpowder, which produces its mechanical effect, appears, from all that we know, by no means complicated, we are still but very imperfectly acquainted with the phænomena which occur in the process. For what has been done in this important field of theoretical artillery since the first, and even now, most important investigation of Gay-Lussac, published thirty years ago, has led to such discordant results, that we cannot be considered at the present time to possess a chemical theory of gunpowder which even approximately accords with experiment.

What is considered to be the normal composition of gunpowder corresponds to a mixture of 1 atom nitre, 1 atom sulphur, and 3 atoms charcoal[†]. Assuming that the whole of the carbon is burnt to carbonic acid, and the nitrogen eliminated as such, we shall obtain, as the following equations show, 330·9 cubic centims. gas at 0° and 0·76 pressure from 1 grm. powder.

$$\left.\begin{array}{l} C^3 \\ S \\ KO+NO^5 \end{array}\right\} \begin{array}{c} 3CO^2 \\ N \\ KS \end{array}$$

$$\text{1 grm. of powder}\left\{\begin{array}{ll} \text{Nitre} & 0{\cdot}7484 \\ \text{Sulphur} & 0{\cdot}1184 \\ \text{Charcoal} & 0{\cdot}1332 \end{array}\right\}\text{gives, when exploded,}$$

$$\left\{\begin{array}{lll} \text{Sulphide of potassium} & 0{\cdot}4078 & \\ \text{Nitrogen . . . .} & 0{\cdot}1037 = & 82{\cdot}52 \text{ cubic cent.} \\ \text{Carbonic acid . . .} & 0{\cdot}4885 = & \underline{248{\cdot}40} \quad ,, \\ & & 330{\cdot}92 \quad ,, \end{array}\right.$$

This volume of gas cannot vary if carbonic oxide and nitric oxide

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[*] Translated by Dr. E. Atkinson.

[†] Taking charcoal as pure carbon.

be formed instead of carbonic acid and nitrogen. And since, with the exception of unimportant traces of hydrogen and sulphuretted hydrogen, carbonic acid, carbonic oxide, nitrogen, and nitric oxide alone occur in the gases from powder, 331 cubic centims. must be considered to be the greatest volume of gas which can in any case be formed from 1 grm. of powder of the normal composition. But the experiments of Gay-Lussac, and of most of the recent observers, give for powder exploded at the ordinary pressure, a much larger volume than could, in any case, have been obtained in accordance with these considerations. From this contradiction alone, we see how uncertain and faulty the methods and observations must have been which form the basis of most of the preceding investigations on this subject.

We have therefore endeavoured to obtain, in a manner less uncertain than that hitherto followed, an experimental foundation for a chemical theory of gunpowder.

The questions which presented themselves were the following:

1. What is the composition of the solid residue remaining after the explosion of powder?
2. Of what does the smoke consist?
3. What is the composition of the gases formed in the explosion of powder?
4. How much residue and smoke on the one hand, and how much gas on the other, are formed by a given quantity of powder?
5. How great is the heat of combustion of powder, and how high the temperature of its flame?
6. How great is the pressure of the gases if the powder explodes in the space which it occupies in the granular condition, assuming that no heat is lost by radiation or by conduction?
7. What is the theoretical work which the powder is capable of accomplishing?

Unfortunately the brief time which was appropriated to our researches, has not allowed us to consider these questions for more than one kind of powder, and only for the combustion under the ordinary atmospheric pressure. We therefore offer the following experiments, not as a complete work, but rather as an example to illustrate the methods followed by us, which may be used, with small alterations, for the investigation of combustions of powder under other conditions than those which we have chosen.

The composition of the sporting and rifle powder which was used in all our experiments is obtained from the following determinations:—

1·2580 grm. gave 0·9960 grm. nitre.

0·8897 grm. gave 0·7040 grm. nitre.

0·9783 grm. gave 0·2054 grm. of a residue insoluble in water, which, when treated with bisulphide of carbon, gave 0·1080 grm. of charcoal free from sulphur.

1·1285 grm. gave 0·2380 of the same residue, which, by oxidation with chlorate of potash and precipitation by chloride of barium, gave 0·8028 grm. sulphate of baryta.

0·1844 grm. of the charcoal free from sulphur, gave 0·0606 grm. water and 0·4658 grm. carbonic acid.

The powder consisted therefore of

	Nitre	78·99
	Sulphur	9·84
Charcoal	Carbon	7·69
	Hydrogen	0·41
	Oxygen	3·07
	Traces of ash	0·00
		100·00

In order to ascertain the qualitative composition of the solid as well as the gaseous products of the explosion of powder, we used the small apparatus, Plate III. fig. 1. *a* is a brass tube 250 millims. long and 2 millims. wide, fitting air-tight into the cork *b*, and in which the finely pulverized gunpowder is pressed down. This tube *a*, as soon as the powder is inflamed and issues from the open extremity with uniform hissing flame, is fixed air-tight in the wide glass tube *d*, by means of the cork *b*. The tube *d* is thereby heated so rapidly that it readily cracks at the place at which the flame touches it. To prevent this, a sieve-shaped, perforated tin tube, or a tube of thin glass, is so inserted as to protect the tube *d* from the direct action of the flame.

The residue and smoke of the exploded powder remain in the tubes *a* and *d*, while the gases issue from the delivery tube, and may be collected over mercury after the atmospheric air has been displaced.

The following substances may be readily detected in the solid residue obtained in this apparatus:—1, sulphate of potash; 2, carbonate of potash; 3, hyposulphite of potash; 4, sulphide of potassium; 5, hydrate of potash; 6, sulphocyanide of potassium; 7, nitrate of potash; 8, carbon; 9, sulphur; 10, carbonate of ammonia. The gaseous products of decomposition contain, on the other hand,—1, nitrogen; 2, carbonic acid; 3, carbonic oxide; 4, hydrogen; 5, sulphuretted hydrogen, and occasionally considerable quantities of (6) binoxide of nitrogen, and even protoxide of nitrogen.

To prepare the material for the quantitative determination of these substances, we have made use of another apparatus which is represented in fig. 2: *a* is a vulcanized india-rubber tube filled with about 15 to 20 grms. powder, and fitting on the brass cap of the glass tube *c*, which is about 1 metre long and 2·5 millims. wide. The cap *b* contains a stop having a small circular orifice with a sharp edge, through which the grains of powder fall in a fine stream like that of an hour-glass, when the vulcanized tube is brought into the position represented in fig. 2. The tube *a*, therefore, in being fitted on *b*, is held downwards in order to prevent the powder from prematurely falling into the glass bulb *d*. This glass bulb *d*, which is gently heated externally by means of a lamp, serves for the combustion of the powder, which takes place regularly in the form of a rhythmical flame if the stream of powder is fine, and falls continuously. Occasional interruptions, which are easily remedied by gentle motion of the india-rubber, are not injurious to the experiment.

The solid residue remains almost completely in the bulb *d*, and in its tubular continuation, while the smoke is deposited without loss in the tube $e e_1$, which is about 25 millims. in diameter, and 1 to 2 metres long, and the gaseous products escape into the air at e_1. These gaseous products cannot be collected by means of a delivery tube at the extremity e_1, for as soon as such a tube dips under a liquid, the pressure of even the smallest column causes the flame to reverberate from the bulb *d*, through the tube *c*, into the vulcanized tube, and thereby explode the whole mass of the powder. Such explosions in masses of from 15 to 20 grms. of powder, are very violent, but are quite without danger, even for the operator in charge of the tube *a*; for the vulcanized tube is immediately burst without offering any resistance. In such explosions, which also occur if the tubular continuation of the bulb is stopped up, the glass part of the apparatus is not injured.

In order, therefore, to collect the gaseous products for analysis without danger of such an explosion, we used the tube $f f_1$ connected with the aspirator *h*, and collected the gases in the tubes $s s_1$, which could be closed by means of clamps, and then hermetically sealed before the blowpipe.

The residue obtained in the bulb *d*, fig. 2, from the combustion of about 20 grms. of powder, formed a semi-fused, yellowish-gray compact mass, which dissolved readily in water, leaving a little charcoal. It was analysed according to the following methods:—

1. *Determination of the unburned Charcoal.*—7 grms. of substance dissolved in hot water gave 0·0682 grm. of charcoal collected on a weighed filter, which corresponds to 0·974 per cent. of charcoal.

2. *Determination of Sulphide of Potassium.*—The filtrate from the above was digested for two days with ignited oxide of copper in a stoppered cylinder, with occasional agitation, until the

liquid, which at first became brownish, was again colourless. The sulphurized oxide filtered off, and dissolved in fuming nitric acid, gave 0·1567 grm. of sulphate of baryta free from nitrate, which for 100 parts of substance corresponds to 2·239 sulphate of baryta, or 1·058 sulphide of potassium, or 1·077 hydrate of potash, or 0·9043 potash, or 0·3080 sulphur.

3. *Determination of Hyposulphite of Potash.*—The whole of the liquid obtained in experiments 1 and 2 was well mixed and divided into 7 equal volumes; one of which, corresponding to 1 grm. of substance, was used for this and for each of the following determinations.

The amount of hyposulphite is most readily determined by adding a standard solution of iodine to a volume of the liquid which has been mixed with solution of starch, and acetic acid added until a feeble acid reaction showed itself. If α be the quantity of iodine in grammes in a degree of the burette, t the number of burette degrees added to the solution until the blue colour manifested itself, the quantity of hyposulphite of potash is obtained from the formula $s = \alpha \frac{2\mathrm{KO\,SO^3}}{\mathrm{I}} t$, as is seen from the following equation,

$$\left.\begin{array}{r}\mathrm{KO\,S^2\,O^2}\\ \mathrm{KO\,S^2\,O^2}\\ \mathrm{I}\end{array}\right\}\begin{array}{l}\mathrm{KO,\,S^4\,O^5}\\ \mathrm{K\,I}\end{array}$$

t was 40·7; α was in this, as in all following determinations, 0·001242 grm. Hence the fluid investigated contained 0·001859 $\times$ 40·7 = 0·07566 grm. hyposulphite of potash, or 7·566 per cent., which corresponds to 3·747 per cent. potash, or 2·5452 sulphur, or 8·0434 nitre.

4. *Determination of Sulphate of Potash.*—Another volume of liquid, containing 1 grm. of residue, gave 0·7582 grm. sulphate of baryta, which in 100 parts correspond to 56·623 sulphate of potash, or 10·397 sulphur, or 30·624 potash, or 65·721 nitre.

5. *Determination of Sulphocyanide of Potassium.*—Small quantities of sulphocyanide of potassium may be determined with great accuracy by a colorometric method. For this purpose we prepared a solution which contained 0·0004884 grm. sulphocyanide of potassium in one degree of the burette, and we employed this solution in all our determinations. As a colorometric agent, we used a solution of perchloride of iron mixed with hydrochloric acid and sufficiently diluted, containing an unknown quantity of iron. Of this solution, two measures were brought into two glass cylinders of the same shape. To one of these the liquid to be tested for sulphocyanide was added, and to the other the standard solution of sulphocyanide of potassium was

added until the colour in both was the same, care being taken by the addition of water to give to both the same degree of dilution. If α_1 be the quantity in grammes of sulphocyanide contained in one degree of the burette, t_1 the number of degrees necessary to produce the same colour, the amount s of sulphocyanide is obtained from the formula

$$s=\alpha_1 t_1.$$

In this and in all the following determinations, α_1 was 0·0004884. By this method half a milligramme of the salt may be detected with certainty. The experiment gave 17·5 degrees of the burette for a volume of liquid containing a gramme of residue, which corresponds to 0·8564 per cent. sulphocyanide of potassium, or 0·4145 potash, or 0·2815 sulphur, or 0·8896 nitre.

6. *Determination of Ammonia.*—The quantity of anhydrous acid contained in a dilute hydrochloric acid solution was determined by precipitation with solution of silver. The liquid used in our experiments contained 0·002357 grm. in a cubic centimetre. Of this liquid, two equal portions, A and A_1, were measured out in a flask which held 23·81 cub. cent. One of the portions was brought into the stoppered cylinder *c*, which stands in cold water in the vessel C, fig. 3. The ammonia was distilled into this cylinder by a delivery tube from the retort *r*, where it was liberated by boiling a volume of the liquid containing a gramme of residue with caustic potash free from nitric acid. To prevent reabsorption during the boiling, the end of the delivery tube which dips in the liquid was fitted with a piece of narrow vulcanized tubing, closed by a piece of glass rod, and in the side of which a longitudinal incision had been made by means of a moistened penknife. Such an incision acts like a valve, allowing free exit to the stream of vapour, but becoming hermetically closed as soon as cooling in the retort causes a diminished pressure. As soon as the ammonia is distilled off, which is evident from the volume of the distillate, the cylinder is cooled down, and its contents transferred into a beaker. Into a similar beaker the second portion of hydrochloric acid solution is brought, and so much water added that the volume in both is the same. When both liquids are coloured by equal quantities of solution of litmus, they are estimated by adding a solution of ammonia, of which about 20 degrees of the burette are necessary to neutralize one measure of the hydrochloric acid employed. If α be the hydrochloric acid contained in one measure, t and t_1 the number of degrees of ammoniacal solution necessary to neutralize A and A_1, and x the quantity of ammonia distilled over from A, we have

$$\frac{\alpha}{t_1}(t_1-t),$$

the amount of hydrochloric acid equivalent to the carbonate of ammonia sought, from which the carbonate of ammonia is obtained from the equation

$$x=\frac{2(NH^4O), 3CO^2}{2HCl}\cdot\frac{\alpha(t_1-t)}{t_1}.$$

The experiment gave for t and t_1 the same value, which proves the entire absence of ammonia.

7. *Determination of Nitre.*—The liquid containing 1 grm. residue in which no ammonia was found, was feebly acidified with pure sulphuric acid, and treated with pure sheet zinc of the thickness of tinfoil. The liquid was kept in cold water, and sulphuric acid gradually added during several hours, so that the zinc dissolved up with the feeblest evolution of gas. When the solution of the zinc, and therewith the conversion of nitric acid into ammonia, was complete, the liquid was mixed with more potash than was necessary to redissolve the precipitated oxide of zinc, and the ammonia formed determined in the manner just described.

Retaining the same meaning for the letters used in (6), we have x_1, the quantity of nitrate sought,

$$x_1=\frac{KO, NO^5}{HCl}\cdot\frac{\alpha}{t_1}(t_1-t).$$

Experiment gave $\alpha=0{\cdot}05612$ grm., $t_1=27{\cdot}0$, $t=18{\cdot}0$, from which we get for x_1 the value 0·05185 grm.

8. *Determination of Carbonate of Potash.*—A measure of the liquid containing 1 grm. residue, was precipitated by a solution of recently fused chloride of manganese; the precipitate filtered off was dried, and treated, together with the filter, with dilute sulphuric acid in a carbonic acid apparatus. The liquid was repeatedly heated to expel the carbonic acid. The loss of weight amounted to 0·0860 grm., which corresponds to 27·016 per cent. carbonate of potash, or 2·3452 charcoal, or 18·417 potash, or 39·525 nitre.

9. *Determination of Hydrate of Potash.*—The protoxide of manganese dissolved in the carbonic acid apparatus in the last determination, gave when precipitated with carbonate of soda, 0·1654 grm. manganoso-manganic oxide. Now 3 atoms MnO, CO^2, or 3 atoms CO^2, correspond to 1 atom Mn^3O^4. The 0·086 grm. carbonic acid found in the carbonate of manganese corresponds to 0·1495 Mn^3O^4; this 0·1495 grm. Mn^3O^4, subtracted from 0·1654 grm., leaves 0·0159 grm. Mn^3O^4, which therefore was not present in the original precipitate as carbonate, but as hydrate of protoxide of manganese. Three atoms of MnO are equivalent to 1 atom Mn^3O^4, and require 3 atoms $KO\,HO$ for their precipitation; Mn^3O^4 corresponds therefore to 3 atoms

KO HO, and hence 0·0159 Mn^3O^4 corresponds to 2·239 per cent. of hydrate of potash.

Now since sulphide of potassium in the determination with oxide of copper passes into hydrate of potash, the quantity of hydrate of potash equivalent to the sulphide of potassium, which according to experiment (2) amounts to 1·077 per cent., must be subtracted from this 2·239 per cent. Hence there remains for the hydrate of potash contained in the mixture 1·202 per cent., which is equivalent to 1·0596 potash, or 2·2741 nitre.

10. *Determination of the Potash contained in the whole of the constituents.*—One measure of the liquid containing 1 grm. of residue, gave, when evaporated with sulphuric acid and strongly ignited, 1·0447 sulphate of potash, which corresponds to 56·497 per cent. of potash.

Collating the results of these experiments, we get the following as the composition of the residue:—

	I.
Sulphate of potash	56·62
Carbonate of potash	27·02
Hyposulphite of potash	7·57
Sulphide of potassium	1·06
Hydrate of potash	1·26
Sulphocyanide of potassium	0·86
Nitrate of potash	5·19
Charcoal	0·97
Carbonate of ammonia	0·00
Trace of sulphur	0·00
	100·55

The quantity of potash contained in 100 parts of the residue amounts therefore, according to the analysis, to 56·88 per cent. By direct experiment 56·50 per cent. was obtained, which differs therefore only by 0·38 from the number calculated from the analysis.

It will be seen from this analysis, that the residue consists chiefly of carbonate and sulphate of potash, and not, as is assumed in the best artillery and technical works, of sulphide of potassium, the quantity of which indeed scarcely exceeds 1 per cent.

In order to answer the second question as to the composition of the smoke, we collected and analysed the gray, porous, ammoniacal deposit formed towards the open end of the long tube $e\,e_1$, which consists of condensed smoke. As this cannot be removed from the tube without attracting moisture, it was immediately dissolved in water, the suspended charcoal filtered off and washed, and the filtrate well mixed and divided into eleven equal volumes, one of which was used to each for the following determinations:—

1. *Charcoal.*—The charcoal contained in the whole liquid weighed 0·08536 grm., and was free from sulphur. Hence 0·00775 grm. corresponds to one part of the liquid.

2. *Sulphide of Potassium.*—The solution did not blacken lead paper, and contained therefore no sulphide of potassium.

3. *Hyposulphite of Potash.*—One volume of the liquid required 11 degrees of the standard solution, corresponding therefore to 0·02045 grm. hyposulphite of potash.

4. *Sulphate of Potash.*—One part of the liquid gave 0·3650 grm. sulphate of baryta, which corresponds to 0·2726 grm. sulphate of potash.

5. *Sulphocyanide of Potassium.*—One volume of the liquid required 4·7 degrees of the standard solution, corresponding to 0·0023 sulphocyanide of potassium.

6. *Ammonia.*—Two parts of liquid gave $t_1 = 27{\cdot}0$, $t = 23{\cdot}9$, $\alpha = 0{\cdot}05612$, which corresponds to 0·0004373 sesquicarbonate of ammonia, or to 0·0002445 carbonic acid for one volume of liquid.

7. *Nitrate of Potash.*—One part of liquid gave $t_1 = 27{\cdot}0$, $t = 25{\cdot}2$, $\alpha = 0{\cdot}05612$, which corresponds to 0·010374 nitre.

8. *Carbonate of Potash.*—Two parts of liquid gave 0·0629 carbonic acid, which after subtracting the carbonic acid of the carbonate of ammonia in experiment (6), corresponds to 0·09803 grm. carbonate of potash.

9. *Hydrate of Potash.*—Two parts of liquid gave 0·1169 grm. manganoso-manganic oxide, which corresponds to 0·05845 for one part liquid. Subtracting therefrom the quantity of $Mn^3 O^4$ (0·05466), equivalent to the carbonic acid of experiment (8), there remains 0·00379 grm. $Mn^3 O^4$, which corresponds to 0·00556 hydrate of potash.

Therefore, according to these experiments, there are contained in one part of the liquid,—

Sulphate of potash	0·27258
Carbonate of potash . . .	0·09803
Hyposulphite of potash . .	0·02045
Sulphide of potassium . .	0·00000
Hydrate of potash	0·00556
Sulphocyanide of potassium.	0·00230
Nitrate of potash	0·01037
Charcoal	0·00775
Sesquicarbonate of ammonia	0·00044
Sulphur	0·00000
	0·41748

or in 100 parts of the dissolved solid substance,—

	II.
Sulphate of potash . . .	65·29
Carbonate of potash . . .	23·48
Hyposulphite of potash . .	4·90
Sulphide of potassium . .	0·00
Hydrate of potash	1·33
Sulphocyanide of potassium.	0·55
Nitrate of potash	2·48
Charcoal.	1·86
Sesquicarbonate of ammonia.	0·11
Sulphur	0·00
	100·00

As a check, a measure of the liquid was evaporated with sulphuric acid, and determined as neutral sulphate of potash. The mass obtained weighed 0·4286 grm. From the analysis, one measure ought to have given 0·4345 grm. salt. The agreement could not be expected to have been closer, and it may be regarded as a guarantee for the accuracy of the analysis.

From a comparison of this analysis (II.) with that of the residue (I.), the conclusion may be drawn that the smoke has essentially the composition of the residue; that in the former the sulphur and nitre of the powder are somewhat more completely burned to carbonate and nitrate of potash; and instead of minute quantities of sulphide of potassium, considerable traces of carbonate of ammonia are found.

In order to settle the third question, respecting the nature of the gases, we collected them for analysis by means of the apparatus previously described, fig. 2.

If the gases produced by the regular combustion of the granular powder, and issuing from the tube $e\,e_1$, be drawn through the tubes ff_1 by the mouth, a taste of almost pure carbonic acid is perceived. If they be allowed to escape through the nose, there is not the least smell of cyanogen, sulphurous acid, or binoxide of nitrogen, and scarcely perceptible traces of sulphuretted hydrogen. Mixed with air, no visible red vapours are produced. Now as a few tenths per cent. of cyanogen, or binoxide of nitrogen, or sulphurous acid, may be thus detected, it may be assumed that these three constituents are not present in the gas under investigation. Of the gaseous products which, besides these, could result from the combustion of powder, there remain therefore only carbonic acid, sulphuretted hydrogen, and traces of oxygen, carbonic oxide, hydrogen, nitrogen, and protoxide of nitrogen. The carbonic acid and sulphuretted hydrogen are first determined in the absorption-tube by potash, and the oxygen by pyrogallate of potash. The residual gas is then transferred into a eudiometer, exploded with excess of hydrogen and electrolytic detonating gas, and the oxygen remaining after the combustion, exploded by excess of hydrogen.

If the required volumes of carbonic acid, sulphuretted hydrogen, oxygen, carbonic oxide, hydrogen, protoxide of nitrogen, and nitrogen, be designated respectively by k, w, s, k_0, h, n_0, and n, we have first, if A_0 be the volume in the absorption-tube taken for analysis,

$$k+w+s+k_0+h+n_0+n=A_0;$$

k, w, and s are obtained directly by absorption. If A_1 be the volume remaining after this absorption, we have

$$k_0+h+n_0+n=A_1.$$

If A_2 volumes of the volume A_1 be taken for analysis in the eudiometer, we have, setting $\frac{A_2}{A_1}=\alpha$,

$$\alpha k_0+\alpha h+\alpha n_0+\alpha n=A_2.$$

If K be the carbonic acid formed by burning this volume A_2 with oxygen, and C the contraction observed in the combustion, we have

$$\alpha k_0=K,$$

$$\alpha h=\frac{2C-K}{3}.$$

If O be the volume of oxygen admitted into the eudiometer for the combustion of A_2, the volume of oxygen remaining after explosion and absorption of carbonic acid is $O-\frac{K+C}{3}$. Further, if V be the volume remaining after the combustion and absorption of carbonic acid of A_2+O, we obtain

$$\alpha n_0+\alpha n=V-O+\frac{K+C}{3}. \quad . \quad . \quad . \quad . \quad (\phi)$$

If the volume V be exploded with excess of hydrogen, a contraction C ensues, which is caused on the one hand by the combustion of oxygen present $=O-\frac{K+C}{3}$, and on the other, by the combustion of protoxide of nitrogen. Now as the contraction corresponding to this $O-\frac{K+C}{3}$ oxygen is $3O-C-K$, the contraction corresponding to the αn_0 protoxide of nitrogen must be $C_1-3O+C+K$. And further, since one volume of gas disappears by the explosion of one volume protoxide of nitrogen with excess of hydrogen, we have

$$\alpha n_0 = C_1 - 3O + C + K.$$

Substituting this value in the equation (ϕ), we have

$$\alpha n = V - C_1 + 2O - \tfrac{2}{3}(C + K).$$

An analysis of the gases collected in the apparatus, fig. 2, founded upon these considerations, and executed with great care, gave the following results:—

Absorption Analysis.

	Volume.	Pressure.	Temperature.	Vol. at 0° C. and 1 metre pressure.
Original volume....................	136·2	0·7359	8°·8	97·102
After absorption of carbonic acid and of sulphuretted hydrogen...	69·1	0·6760	8·0	45·382
After absorption of oxygen	68·7	0·6738	8·6	44·877

Sulphuretted hydrogen determination in the Potash Ball.*

146 divisions of the absorption-tube correspond to 30 cubic centims.

1 burette degree contained $\alpha = 0\cdot001242$ grm. iodine.

The potash ball required $t = 1\cdot8$ burette degree.

Hence the sulphuretted hydrogen contained in 97·104 volumes of gas at 0° C. and 1^{m} pressure, and absorbed simultaneously with the carbonic acid, amounted to

$$\frac{146}{30}\cdot\frac{773}{1\cdot175}\cdot\frac{112\cdot5}{1588\cdot7}\,.\,0\cdot76\,\alpha t = 0\cdot58 \text{ division.}$$

Combustion Analysis of the transferred Gas.

	Volume.	Pressure.	Temperature.	Vol. at 0° C. and 1 metre pressure.
Original volume....................	110·0	0·3569	10°·2	37·846
After admission of oxygen.........	150·6	0·3974	10·3	57·674
After admission of electrolytic detonating gas..................	189·0	0·4350	10·3	79·230
After explosion	144·2	0·3915	9·3	54·595
After absorption of carbonic acid.	135·7	0·3915	9·1	51·414
After admission of hydrogen (dry)	220·4	0·4753	10·9	100·740
After explosion (dry)...............	125·6	0·3917	9·0	47·629

We obtain from this the following elements necessary for the calculation:—

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* *Gasometrische Methoden*, by R. Bunsen, p. 93. Roscoe's translation, p. 86.

$A_0 = 79{\cdot}102$
$A_1 = 44{\cdot}877$
$A_2 = 37{\cdot}846$
$K = 3{\cdot}181$
$C = 3{\cdot}079$
$O = 19{\cdot}828$
$V = 51{\cdot}414$
$C_1 = 53{\cdot}111$

By substituting them in the formulæ, we get—

$k = 51{\cdot}140$
$w = 0{\cdot}580$
$s = 0{\cdot}505$
$k_0 = 3{\cdot}772$
$h = 1{\cdot}176$
$n = 40{\cdot}063$
$n_0 = -\ 0{\cdot}134$

The gas consists, therefore, in 100 volumes, of—

Carbonic acid	52·67
Nitrogen	41·12
Carbonic oxide	3·88
Hydrogen	1·21
Sulphuretted hydrogen	0·60
Oxygen	0·52
Protoxide of nitrogen	0·00
	100·00

The most surprising thing in this analysis is, that free oxygen should be formed along with combustible gases at a high temperature. We do not believe that the 0·52 per cent. of oxygen found arises from an error of analysis, since the accuracy of the methods employed, and the care taken in the experiments, scarcely allow the possibility of such an error. The presence of this oxygen is rather to be explained by assuming that the residue, finely divided as smoke, which remains after the combustion of the charcoal and sulphur, and which still contains nitre, may give off small quantities of oxygen in cooling at a temperature no longer sufficient to explode the gases, diluted as they are with seventeen times the quantity of incombustible constituents.

If powder in exploding decomposed exactly into sulphide of potassium, nitrogen, and carbonic acid, as the theory hitherto adopted requires, the last two gases ought to stand in the relation 1 : 3. But experiment shows that not even the relation 1 : 1·5 is obtained. From this circumstance it may be inferred that the decomposition of gunpowder must depend on processes entirely different from those on which the old theory is based.

We may now proceed to answer the fourth question; that is, how much residue and smoke on the one hand, and how much gas on the other, are produced by the explosion of a determinate weight of powder?

To decide this, we collected together and analysed the residue and smoke formed in the combustion of the powder which yielded the gases just investigated.

For greater convenience in the analysis, an undetermined quantity of the substance was again dissolved in so much water that the volume of the whole liquid amounted to 500 cubic centims. To each determination 45·474 cubic centims. were taken, which were measured out in a graduated cylinder. The constants used in calculating the experiments are, where they are not specially stated, the same as in the analysis of the residue.

1. *Charcoal and Sulphur.*—500 cubic centims. gave 0·2141 grm. charcoal, sulphur, and incombustible residue. 0·1758 grm. of this mass, oxidized with nitric acid, gave 0·1749 grm. sulphate of baryta, which for 0·2141 grm., the total amount of the mass, corresponds to 0·0292 grm. sulphur. The mass which had been treated with nitric acid, and the residue not so treated, gave, when ignited together in an open crucible, 0·0248 grm. incombustible residue. For one measure, or 45·475 cubic centims. of the liquid used in the subsequent experiments, there correspond therefore,—

Charcoal . . .	0·014561 grm.
Sulphur	0·002656 grm.
Residue	0·002256 grm.

2. *Sulphide of Potassium.*—The whole liquid (500 cubic centims.), treated as before with oxide of copper, gave 0·9902 grm. sulphate of baryta, which is equivalent to 0·46787 sulphide of potassium, or 0·1358 sulphur; and for 45·475 cubic centims. corresponds to 0·04255 grm. sulphide of potassium.

3. *Hyposulphite of Potash.*—One measure gave 35·1 burette degrees, which corresponds to 0·06525 hyposulphite of potash.

4. *Sulphate of Potash.*—One measure gave 1·131 grm. sulphate of baryta, which corresponds to 0·84463 grm. sulphate of potash.

5. *Sulphocyanide of Potassium.*—One measure gave 12·5 burette degrees, which corresponds to 0·006105 sulphocyanide of potassium.

6. *Ammonia.*—One measure gave $\alpha = 0·06688$, $t_1 = 46·6$, $t = 17·3$. This corresponds to 0·01645 ammonia, or to 0·05709 $2NH^4O, 3CO^2$.

7. *Nitrate of Potash.*—One measure was taken. The ammonia was determined in a quantity of potash equal to that used in the experiment; it gave $\alpha = 0·05612$, $t_1 = 27$, and $t = 26·4$, which corresponds to a quantity of ammonia equivalent to

0·0034578 nitre, and which must therefore be subtracted from the quantity of nitrate of potash found in one measure of the liquid. One measure gave $\alpha=0{\cdot}05612$, $t_1=46{\cdot}6$, $t=23{\cdot}3$, which corresponds to 0·077808 grm., or after subtracting the above quantity, to 0·07435 grm. nitre.

8. *Carbonate of Potash and Hydrate of Potash.*—One measure gave 0·1124 grm. CO^2, and 0·2240 grm. Mn^3O^4. After subtracting the quantity of carbonate of potash equivalent to the carbonate of ammonia (6), this 0·1124 CO^2 corresponds to 0·3531 grm. carbonate of potash, which is equivalent to 0·19539 grm. Mn^3O^4. This 0·19539 grm., subtracted from the 0·2240 grm., leaves 0·02860 grm. Mn^3O^4, which is equivalent to 0·03515 KO. But since the quantity of potash equivalent to the sulphide of potassium amounts to 0·03635 grm., it may be assumed that no hydrate of potash is present.

Hence there are contained in one measure,—

Sulphate of potash . . .	0·84463	corresp. to	0·45676	potash.
Carbonate of potash . . .	0·25279	,,	0·17233	,,
Hyposulphite of potash . .	0·06525	,,	0·03232	,,
Sulphide of potassium . .	0·04255	,,	0·03637	,,
Hydrate of potash . . .	0·00000	,,	0·00000	,,
Sulphocyanide of potassium	0·00611	,,	0·00296	,,
Nitrate of potash	0·07435	,,	0·03464	,,
Charcoal	0·01456		0·73538	,,
Sulphur	0·00266			
Carbonate of ammonia $2(NH^4O), 3CO^2$.	0·05709			
	1·35999			

One measure gave 1·38 grm. sulphate of potash, corresponding to 0·7463 grm. potash, which agrees as closely with the potash found from the analysis as can be expected. Hence the gunpowder and the products into which it decomposes in its combustion, have the following composition :—

A.

Gunpowder.

	Nitre . .	78·99
	Sulphur .	9·84
Charcoal.	Carbon .	7·69
	Hydrogen.	0·41
	Oxygen .	3·07
		100·00

B.

Solid products of decomposition.

Sulphate of potash	62·10
Carbonate of potash . . .	18·58
Hyposulphite of potash . .	4·80
Sulphide of potassium . .	3·13
Sulphocyanide of potassium .	0·45
Nitrate of potash	5·47
Charcoal	1·07
Sulphur	0·20
Sesquicarbonate of ammonia.	4·20
	100·00

C.

Gaseous products of decomposition.

Carbonic acid	52·67
Nitrogen	41·12
Carbonic oxide	3·88
Hydrogen	1·21
Sulphuretted hydrogen	0·60
Oxygen	0·52
Protoxide of nitrogen	0·00
	100·00

Since the whole of the potassium of the powder remains in the solid products of combustion, the quantity of solid residue obtained by the combustion of 1 grm. powder may be calculated from the analyses A and B. According to analysis A, 1 grm. of powder contains 0·3055 potassium; the quantity of residue of the composition B which contains so much potassium, amounts to 0·6806 grm.

Calculating the nitrogen present in this 0·6806 grm. residue, and subtracting it from the nitrogen present in 1 grm. of powder, we obtain the quantity of nitrogen contained in the gases arising from a gramme of powder. The volume of gas, which, according to analysis C, corresponds to the nitrogen thus calculated, weighs 0·3138 grm. Hence a gramme of the powder examined decomposes, in burning, into 0·6806 grm. residue of the composition B, and into 0·3138 grm. gases of the composition C.

The transformation which gunpowder experiences in burning may be exhibited by the following scheme:—

D.

1 grm. powder		
Nitre		0·7899
Sulphur		0·0984
Charcoal	C	0·0769
	H	0·0041
	O	0·0307

gives, when exploded,

Residue 0·6806		gr.
	$KO\,SO^3$	0·4227
	$KO\,CO^2$	0·1264
	$KO\,S^2O^2$	0·0327
	KS	0·0213
	$K\,Cy\,S^2$	0·0030
	$KO\,NO^5$	0·0372
	C	0·0073
	S	0·0014
	$2NH^4O, 3CO^2$	0·0286

Gases	gr. 0·3138		gr.		cub. cents.
		N	0·0998	=	79·40
		CO^2	0·2012	=	101·71
		CO	0·0094	=	7·49
		H	0·0002	=	2·34
		HS	0·0018	=	1·16
		O	0·0014	=	1·00
	0·9944				193·10

In analytical investigations, agreement in the weight of the substances found, with the weight of the substance taken for analysis, is commonly used as a control. In our experiments this check is inapplicable, as the quantity of residue was not weighed. But, on the other hand, we have an equally certain verification in the fact that the quantities of K, N, S, C, O contained in 1 grm. powder must be found in almost unchanged quantity in the products of combustion. Such a comparison gives for our analysis,—

In the unexploded powder,—

K 0·3050; N=0·1096; S=0·0984; C=0·0769; O=0·4057.

In the exploded powder,—

K 0·3050; N=0·1096; S=0·0989; C=0·0780; O=0·3936.

The agreement in the quantities of potash and nitrogen, from which the equations for the calculation of the above scheme are taken, serve as proof of the accuracy of the calculation; the agreement of the three quantities of sulphur, charcoal, and oxygen, as so many proofs of the accuracy of the experiments.

It follows from this equation that 1 grm. of powder in exploding yields 193·1 cubic centims. of gas. The quantity which, according to the previous theory, ought to be found, amounts to 330·9 cubic centims., or above one-third more.

After having established the transformation which occurs in the combustion of gunpowder, we only need a determination of the temperature of its flame, to have all the elements from which may be deduced a theoretical measure of the work performed in the combustion of powder. We have therefore to occupy ourselves with the fifth question proposed at the commencement of our research, respecting the temperature of the combustion of powder. In order to obtain a certain basis for the solution of this question, it is necessary to form an accurate representation of the nature of the flame of powder. If we suppose 1 grm. of powder simultaneously burnt throughout its entire mass, w units of heat will be liberated; the products of decomposition of the exploded powder will be thereby heated $\frac{w}{s}$ degrees at the moment of their formation, in which quotient s is the specific heat of the products of combustion as compared with water. This quotient is therefore the temperature of the flame, measured by the thermometric unit of the heat of combustion. But the temperature of the flame experiences in fact a steady lowering, caused by radiation and conduction of heat. As the temperature $\frac{w}{s}$ only remains constant for an infinitely short space of time, it cannot be measured by the usual thermometric means. The phænomena which occur in the continuously burning scintillating flame of the pressed powder are entirely similar. This flame has indeed a constant temperature $\frac{w}{s}$ throughout the duration of the experiment, but this temperature only extends to the infinitely small layer which is in the act of exploding, and from radiation and conduction diminishes in a fixed ratio from this layer to the point of the flame.

If therefore we wish to know the temperature of the flame as produced by the process of combustion, and not affected by foreign influences, we must calculate it from the values w and s.

As w, the heat of combustion of powder, was unknown, we determined it in the following manner:—A, fig. 4, is a brass tube filled with a compressed mass of finely pulverized gunpowder of known weight p. In the wider part of this tube a glass piece b is cemented, in which the platinum wires $c\,c$ are fused. These wires are in connexion with a fine capillary platinum wire dipping in the powder. This small apparatus, A, is placed in the glass tube B, which is closed at one end, and this, together with the apparatus contained in it, is placed at the bottom of the tube C. This is closed at the bottom, but still open at the top, and is provided with two fine apertures at $d\,d$. The two wires $c\,c$ are passed through the apertures $d\,d$ and sealed at the blowpipe, and then the upper end of the tube c is also hermetically closed. The tube C has a small projection at its lower extremity, by which it is fitted in the cork K. By means of this cork the whole arrangement is fixed in the wide tube D, only half of which is seen in the figure. This consists of the finest sheet brass, and contains a sieve-shaped stirring arrangement, which can be moved up and down from without by the fine wires $g\,g$. The apparatus, the glass, platinum, and brass parts of which have all been severally weighed, is filled over the top with a weighed quantity of water, and, enclosed in a wooden box, is left in a place inaccessible to change of temperature until it has attained a uniform temperature.

To determine with this apparatus the heat of combustion of powder, the following time and temperature observations are needed, the latter of which may be directly made on the thermometer K, which is graduated to the $\frac{1}{100}$th of a degree Centigrade, and is placed in the lateral enlargement of the brass water-vessel.

The temperature is first observed for the times t and t_1; the powder is then exploded by passing a galvanic current through the wires $c\,c$, noting the time t_2-t_1 which transpired from the last observation to the commencement of the explosion. The time t_3-t_2 is then observed which elapses until the maximum of the heating from the explosion is reached. Finally, the times t_4 and t_5, counted from the maximum, and their corresponding regularly decreasing temperatures are to be observed. Throughout the entire duration of the experiment the liquid is kept in motion by the stirrer.

An experiment executed in this manner with great care, gave the following values for *w*:—

	Observed Time *t*.	Observed Temperature.
	0′	19·86
	5′	19·83
	6′	19·83
Explosion	67′	
Maximum temperature . .	16′	21·10
	26′	20·98
	56′	20·60

The weights of the separate parts of the apparatus were,—

Weight of the	Glass	79·14	grms.
,, ,,	Brass	132·11	,,
,, ,,	Platinum	3·50	,,
,, ,,	Gunpowder . . .	0·7152	,,
,, ,,	Water	376·40	,,

The value in water of all these weights is 404·7 grms. These 404·7 grms. water were raised 1°·14 C. by the heat produced in the combustion of 0·7125 grm. gunpowder.

Hence the heat of combustion of the powder used in our experiments, that is, the quantity by which one part by weight of water is heated by the combustion of one part by weight of powder, is 643°·9 C.

A small correction is necessary in this number; for the combustion of the 0·7125 grm. powder took place in the hermetically sealed tube which was full of air. Hence the combustible gaseous products of decomposition would burn, and thereby produce a small quantity of heat which does not really belong to the true decomposition. According to the scheme D, the 0·7125 grm. of powder taken for the experiment gave,—

0·00669	grm.	Carbonic oxide.
0·00014	,,	Hydrogen.
0·00128	,,	Sulphuretted hydrogen.

Assuming with Favre and Silbermann that the heat of combustion of these three constituents are respectively 2403, 34462, and 2741, the heat produced by their combustion, compared with a gramme of water, would amount to 24°·4 C. And the above number would accordingly have to be reduced by that quantity, which would place the real heat of combustion at

619°·5 C.

The heat, on the other hand, produced by the increased pressure of gas in the hermetically sealed combustion-tube may be safely neglected as being infinitely small. Calculating the heat of combustion of gunpowder on the assumption that its combustible constituents burn with free oxygen, and taking the heat of combustion of sulphur, of charcoal and of hydrogen as found by Favre and Silbermann, we obtain the number

1039°·1 C.

The oxidation of the constituents of powder by the oxygen of the nitre gives therefore far less heat than their combustion with free oxygen. This fact is by no means strange, when we consider that the nitrogen, amounting to two-thirds of the weight of the combustible constituents, must fix a considerable amount of heat in its change into the gaseous form.

The temperature of the flame of powder, or the temperature which would prevail in the burning mass if no heat were lost by radiation or conduction, is obtained directly by dividing the number 619°·5 by the specific heat of the products of combustion. But this specific heat may be calculated from the substances contained in 1 grm. of exploded powder, as shown in scheme D, by multiplying the weights of the substance in column *a* of the following Table by the specific heats corresponding to them in column *b*, and adding together the products. The small quantities of KO, SO^3, $K\,Cy\,S^2$, $2NH^4O$, $3CO^2$, and HS, whose specific heats are not known, may be safely disregarded, since their influence would only affect the desired numbers to a very inconsiderable extent.

	a.	*b.*	*a×b.*
Sulphate of potash . .	0·4554	0·1901	0·08656
Carbonate of potash .	0·1362	0·2162	0.02944
Sulphide of potassium .	0·0229	0·1081	0·00248
Nitre	0·0401	0·2388	0·00957
Charcoal	0·0079	0·2411	0·00190
Sulphur	0·0015	0·7026	0·00031
Nitrogen	0·1075	0·2440	0·02623
Carbonic acid	0·2167	0·2164	0·04692
Carbonic oxide	0·0101	0·2479	0·00251
Hydrogen	0·0002	3·4046	0·00073
Oxygen	0·0015	0·2182	0·00033
	1·0000		0·20698

Dividing the number 619°·5 by 0·207, we obtain for the temperature of the flame of powder burning freely in the air, the number

2993° C.

If the powder burns in a closed space in which the gases cannot expand, the temperature of the flame will be different. We obtain this temperature by dividing the heat of combustion of powder by the specific heat for a constant volume. The latter is obtained by a calculation similar to the above.

	a_1.	b_1.	$a_1\,b_1$.
Sulphate of potash . .	0·4554	0·1901	0·08656
Carbonate of potash .	0·1362	0·2162	0·02944
Sulphide of potassium .	0·0229	0·1081	0·00248
Nitre	0·0401	0·2388	0·00957
Charcoal	0·0079	0·2411	0·00191
Sulphur	0·0015	0·2026	0·00031
Nitrogen	0·1075	0·2440	0·01846
Carbonic acid . . .	0·2167	0·2164	0·03426
Carbonic oxide . . .	0·0101	0·2479	0·00177
Hydrogen	0·0002	3·4046	0·00048
Oxygen	0·0015	0·2182	0·00023
			0·18547

Temperature of the flame $\frac{619\cdot5}{0\cdot18547}=3340^\circ$ C.

Hence the temperature of the flame of powder whose gases are formed in a closed space and cannot freely expand is 3340° C.

If the flame consisted only of ignited gaseous substances, whose capacity for heat, as has been shown by Regnault's experiments and Clausius's theoretical considerations, does not change with the temperature, the value of $\frac{w}{s}$ might be determined with great accuracy. But since the specific heat of solid bodies increases with the temperature, the numbers 2993° and 3340° can only be accepted as approximate values, which cannot, however, from what we know of the magnitude of the increase with the temperature, be very far from the truth. Since s increases with the temperature, $\frac{w}{s}$ would be found too great; and since, in fact, the temperature of the flame is continually lowered by radiation and conduction, we may assume that the temperatures 3340° and 2993° represent limits to which the temperature of the flame more or less approximates, but which it can never fully reach or exceed. From this fact an important conclusion may be drawn as to the amount of pressure exerted by the powder in the space which it gravimetrically fills.

It has been hitherto generally assumed, that during the combustion of powder the residue assumes the vaporous condition, and by its tension materially helps the mechanical effects of the powder. Although a small volatilization cannot be denied, it may be shown from the calculation of the temperature of flame, that the tension caused by such a volatilization cannot amount to one atmosphere. The temperature of the flame of hydrogen burning in air is 3259° C.* A bead of powder residue melted on a thin platinum wire volatilized gradually in a jet of hydrogen burning in air, but it never reached ebullition, and hence the tension of its vapour could never have reached an atmosphere. And hence the pressure exerted by the vapour of the solid products of decomposition at the temperatures 2993° and 3340° C. can only be infinitely small, and may be safely neglected. It is possible, therefore, to calculate from our experiments the maximum pressure which is reached, but never exceeded, by the combustion of powder in a closed space.

If we call G_p the weight of the powder, S_p its gravimetric† density, G_r the weight of residue obtained from the G_p powder, S_r the density of the residue at 3340° C., and V the volume of gas at 0° C. and 760 millims. obtained from G_p. If, further, t be the temperature of the flame of powder burning in an enclosed space, we obtain, from the following equation, the pressure p^0 which powder exerts when it burns in the space $\frac{G_p}{S_r}$ which it occupies, and which is supposed to be impervious to heat,

$$p_0 = \frac{V(1 \times 0{\cdot}00366 \times t)}{\frac{G_p}{S_p} - \frac{G_r}{S_r}}.$$

In this equation there is only one magnitude whose determination presents any difficulty, that is S_r, or the specific gravity of the fixed powder residue at 3340° C., the temperature of the flame. We have determined this specific gravity by a method not yet published, which one of us has used to determine the volatilization and expansion of rocks melted at high temperatures, independently of the expansion of the vessels containing them. An experiment according to this method gave, with approximate but sufficient accuracy, for the specific gravity of the residue,

At 18° C.	2·350
At 2808° C.	1·520

By interpolation for the specific gravity of the residue at 3340° we obtain $S_r = 1{\cdot}50$. The values of the magnitudes occurring in the formula are therefore for the powder investigated,

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* *Gasometrische Methoden*, von R. Bunsen, p. 254. Roscoe's translation, p. 242.

† This expression is used in artillery to denote the weight of a cubic centimetre of granular powder.

$G_p = 1{\cdot}000$ grm.
$S_p = 0{\cdot}964$ „
$G_r = 0{\cdot}6806$ „
$S_r = 1{\cdot}50$ „
$V = 193{\cdot}1$ cubic centims.
$t = 3340°$ C.

By substituting them in the formula, we obtain for p_0 the value

4373·6.

Calculating the pressure with the assumption of the specific gravity of the residue at ordinary temperatures (2·35), we obtain for p_0 the value 3414·6. Hence about 1000 of the 4374 atmospheres are due to the expansion caused by the heating of the powder residue.

A powder of the composition of that investigated by us, which explodes in a gun behind the projectile, can never exert a pressure of 4500 atmospheres, in consequence of the unavoidable loss of heat in the combustion. Whether essential changes in the decomposition of powder occur if it explodes freely, or at high pressure in the gun, might be easily ascertained from the composition of the residue deposited in the gun, or from the composition of the gases. If it be shown that under these circumstances the decomposition is essentially the same, many previous assumptions as to the pressure exerted in guns must depend on very faulty premises, for the best artillery works give this pressure at from 50,000 to 100,000 atmospheres*.

These experiments give us at the same time the means of determining the maximum mechanical effect of powder, that is, the theoretical work which it performs when its gases expand under a pressure corresponding to their respective volume in a space supposed to be impervious to heat.

Let $a_1 a_3 a_3 a_1$ (fig. 5) be the space filled by the powder G_p, $a_2 a_3 a_3 a_2$ the volume occupied by the residue G_r, and $a_1 a_2 a_2 a_1$ the volume $v_0 = \frac{G_p}{S_p} - \frac{G_r}{S_r}$ occupied by the gases in the moment of the combustion, in which volume the pressure p prevails, which has just been determined; lastly, let $a\, a_2 a_2 a$ be the space occupied by the gases after they have expanded to the pressure p_1. Further, if $c\, a_2 a_2 c$ be the infinitely small expansion of the residue, the work effected during this expansion under the pressure p_0 will be $p_0 dv$, and the total work which the gas effects in its total expansion will be

$$\text{Work} = \int_{v_0}^{\infty} p\,dv.$$

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* Piobert, *Traité d'Artillerie*, 1847, p. 322.

But on the assumption that a gas expands in an envelope impervious to heat, we have, if p_0 denotes the pressure corresponding to v_0*,

$$p=p_0\left(\frac{v_0}{v}\right)^{\mathrm{K}},$$

in which K denotes the relation of the specific heat of the gas for a constant pressure and constant volume. It follows from the integral,

$$\text{Work}=\frac{p_0v_0}{\mathrm{K}-1}.$$

A gramme of the powder used by us gives for $v_0=\frac{\mathrm{G}_p}{\mathrm{S}_p}-\frac{\mathrm{G}_r}{\mathrm{S}_r}$, 0·5836 cubic centims.; and for p_0, $1029{\cdot}8\times4373{\cdot}6$ grms. K is found from the composition of the gases to be 1·39. A kilogramme of the powder, in undergoing the above decomposition, effects therefore a theoretical work of 67410 metre kilogrammes.

LXIII. *Crystallographic Notices.* By W. H. Miller, *M.A., F.R.S., Professor of Mineralogy in the University of Cambridge*[†].

An Improved Method of finding the position of any Face in Crystals belonging to the Anorthic System.

In the Numbers of the Philosophical Magazine for February and May 1857, it is proved that the anharmonic ratio of the axes of four zones having a face in common, is a simple and symmetric function of the indices of two of the zones, and of two faces one in each of the other two zones, of the same form as the expression for the anharmonic ratio of four faces in one zone. This expression enables us to find the position of any face of a crystal of the anorthic system much more directly than by the methods given in my 'Crystallography,' Chap. VII., and in the new edition of Phillips's 'Mineralogy,' arts. 217, 218. The expressions referred to above, beginning with the latter, may be enunciated as follows.

Let P, Q, R, S be the poles of any four faces, no two of which are parallel, in one zone; *hkl, uvw* the symbols of Q, S; efg, pqr the symbols of any two zones containing the faces P, R respectively, neither of which is the zone PR. Then

$$\frac{\mathrm{e}h+\mathrm{f}k+\mathrm{g}l}{\sin\mathrm{PQ}}\,\frac{\mathrm{p}u+\mathrm{q}v+\mathrm{r}w}{\sin\mathrm{RS}}=\frac{\mathrm{p}h+\mathrm{q}k+\mathrm{r}l}{\sin\mathrm{RQ}}\,\frac{\mathrm{e}u+\mathrm{f}v+\mathrm{g}w}{\sin\mathrm{PS}}.\quad.\quad.\quad(\alpha)$$

This expression may be easily remembered, if we observe that

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* Poisson, *Mech.* ii. p. 647. Clausius, Pogg. *Annalen*, vol. lxxix. p. 396.

[†] Communicated by the Author.

the indices which enter into any one of the numerators, refer to the extremities of the arc which appears in the corresponding denominator.

Supposing PR to be less than a semicircle, sin PQ : sin RQ is negative, except when Q lies between P and R, and sin PS : sin RS is negative, except when S lies between P and R.

When PQ, RQ, the symbols of the poles Q, S, and of the zones containing P, R, are known, the preceding equation gives the value of the ratio sin RS : sin PS. The ratio sin RS : sin PS having been found, PS is given by the equations

$$\tan\theta = \frac{\sin \mathrm{RS}}{\sin \mathrm{PS}},\ \tan(\mathrm{PS} - \tfrac{1}{2}\mathrm{PR}) = \tan\tfrac{1}{2}\mathrm{PR}\tan\left(\frac{\pi}{4} - \theta\right).$$

PS may be found directly by substituting for $\frac{\sin \mathrm{PQ}}{\sin \mathrm{RQ}}\frac{\sin \mathrm{RS}}{\sin \mathrm{PS}}$ its equivalent $\frac{\cot \mathrm{PS} - \cot \mathrm{PR}}{\cot \mathrm{PQ} - \cot \mathrm{PR}}$.

When PQ is less than a quadrant, cot PQ is positive or negative according as PQ, PR are measured from P in the same or in opposite directions. The sign of PS is determined by a similar rule.

Let KP, KQ, KR, KS be any four zone-circles passing through the pole K; efg, pqr the symbols of KP, KR; *hkl, uvw* the symbols of any poles Q, S, except K, in the zone-circles KQ, KS. Then

$$\frac{\mathrm{e}h+\mathrm{f}k+\mathrm{g}l}{\sin \mathrm{PKQ}}\,\frac{\mathrm{p}u+\mathrm{q}v+\mathrm{r}w}{\sin \mathrm{RKS}} = \frac{\mathrm{p}h+\mathrm{q}k+\mathrm{r}l}{\sin \mathrm{RKQ}}\,\frac{\mathrm{e}u+\mathrm{f}v+\mathrm{g}w}{\sin \mathrm{PKS}}. \quad . \quad (\beta)$$

When PKR is less than two right angles, sin PKQ : sin RKQ is negative, except when Q lies in the lune PKR, and sin PKS : sin RKS is negative, except when S lies in the lune PKR.

The angle PKS may be found by means of the equations

$$\tan\theta = \frac{\sin \mathrm{RKS}}{\sin \mathrm{PKS}},\ \tan(\mathrm{PKS} - \tfrac{1}{2}\mathrm{PKR}) = \tan\tfrac{1}{2}\mathrm{PKR}\tan\left(\frac{\pi}{4} - \theta\right).$$

PKS may also be found by writing $\frac{\cot \mathrm{PKS} - \cot \mathrm{PKR}}{\cot \mathrm{PKQ} - \cot \mathrm{PKR}}$ instead of $\frac{\sin \mathrm{PKQ}}{\sin \mathrm{RKQ}}\frac{\sin \mathrm{RKS}}{\sin \mathrm{PKS}}$ in (β).

The signs of PKS, cot PKS, cot PKQ are determined by rules similar to those already given for the signs of PS, cot PS, cot PQ.

Let D, E, G, H be four poles, no three of which are in one zone-circle, P any other pole. Having given the symbols of the poles, and five of the six arcs joining every two of the poles D, E, F, G, to find the position of P with respect to D, E.

From five of the six arcs joining every two of the poles D, E, F, G, compute DE, if not already given, and the angles GDE, HDE, GED, HED. From the symbols of D, E, G, H find the symbols of the zone-circle DE, of one of the zone-circles DG, DH, and of one of the zone-circles EG, EH. Then, the symbols of the zone-circles through D and two of the three poles E, G, H, the symbol of the third pole, and that of P, and the angles GDE, HDE being known, the angle PDE is given by (β). In the same manner the angle PED may be found. Hence, knowing the arc DE, and the angles PDE, PED, the arcs PD, PE may be found, and the position of P determined with respect to D and E.

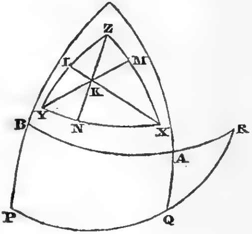
When any other pole T occurs in the zone-circle DP, the symbol of the pole in which DP meets either EG or HG, and its distance from D, may be found, and then DT is given by (α).

Nearly all the expressions for calculating the dihedral angles of crystals given in the new edition of Phillips's 'Mineralogy,' are merely particular cases of (α) and (β). These very simple and useful formulæ cannot be readily expressed in any notation which differs essentially from the notation used in the work above mentioned; and cannot be expressed at all in any notation which merely designates a simple form, without being able to distinguish from each other the different faces of which it is composed.

To find the direction of the axis of a zone.

The determination of the direction of the axis of a zone in terms of the indices of the zone is important, inasmuch as it materially aids us in forming a distinct conception of the nature of crystalline forms, and is also useful in constructing models of crystals and in drawing their figures. In the Philosophical Magazine for May 1857, I determined the direction of the axis of a zone by means of elementary geometry. The following investigation was undertaken in order to render more complete the treatment of Crystallography by spherical trigonometry.

Let the axes of the crystal OX, OY, OZ meet the surface of the sphere of projection in X, Y, Z. Let *a*, *b*, *c* be its parameters; A, B, C the poles of 1 0 0, 0 1 0, 0 0 1; K the pole of the zone-circle uvw. P, Q, R the points in which the zone-circle uvw intersects BC, CA, AB. XKL, YKM, ZKN great circles meeting YZ, ZX, XY in L, M, N.



Observing that the great circles RX, RY, QX, QZ, PY, PZ make with PC, QC, RB and PQR, six right-angled spherical triangles having QR, RP, PQ for their hypothenuses, and that P, Q, R are the poles of KX, KY, KZ, we obtain

$$\begin{aligned}
\cos RX &= \sin RP \sin P = \sin ZKX \sin KX,\\
-\cos RY &= \sin RQ \sin Q = \sin YKZ \sin KY,\\
\cos QX &= \sin PQ \sin P = \sin XKY \sin KX,\\
-\cos QZ &= \sin RQ \sin R = \sin YKZ \sin KZ,\\
\cos PY &= \sin PQ \sin Q = \sin XKY \sin KY,\\
-\cos PZ &= \sin RP \sin R = \sin ZKX \sin KZ.
\end{aligned}$$

P, Q, R are the intersections of the zone-circle uvw with the zone-circles BC, CA, AB. Hence the symbols of P, Q, R will be $0w\bar{v}$, $w0\bar{u}$, $v\bar{u}0$. Therefore

$$\frac{b}{w}\cos PY = -\frac{c}{v}\cos PZ,$$

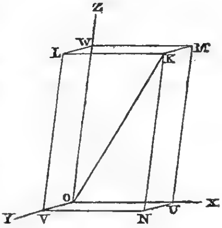
$$\frac{a}{w}\cos QX = -\frac{c}{u}\cos QZ,$$

$$\frac{a}{v}\cos RX = -\frac{b}{u}\cos RY.$$

Hence

$$au\frac{\sin KX}{\sin YKZ} = bv\frac{\sin KY}{\sin ZKX} = cw\frac{\sin KZ}{\sin XKY}.$$

Construct a parallelopiped UVW having OK, the axis of the zone, for a diagonal, and three of its edges OU, OV, OW coincident with OX, OY, OZ, the axes of the crystal. Let KL, KM, KN be the edges respectively parallel to OU, OV, OW.



$$\frac{OV}{OU} = \frac{\sin GOX}{\sin GOY} = \frac{\sin NX}{\sin NY} = \frac{\sin KX \sin ZKX}{\sin KY \sin YKZ} = \frac{bv}{au}.$$

In like manner $\frac{OW}{OU} = \frac{cw}{au}$. Therefore

$$\frac{OU}{au} = \frac{OV}{bv} = \frac{OW}{cw}.$$

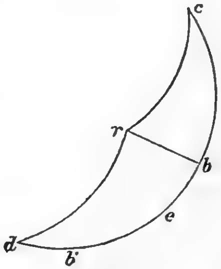
Hence, the axis of the zone uvw is the diagonal of a parallelopiped, the edges of which coincide with the axes of the crystal, and are respectively proportional to *a*u, *b*v, *c*w.

On the lines observed by M. Victor v. Lang *on the faces of crystals of quartz.*

It is an important advantage of Neumann's graphic method of indicating the positions of the faces of a crystal, that it supersedes the employment of troublesome constructions, in the solution of problems relating either to the dihedral angles of crystals, or to the plane angles which the edges of the faces make with each other. The determination of the crystallographic import of the lines observed by v. Lang on the faces of the six-sided pyramid, in certain crystals of quartz, affords a good example of the superiority of this method over other methods commonly used.

According to the observations of V. Lang (*Sitzungsberichte der Mathem.-naturw. Classe der kais. Akademie der Wissenschaften,* vol. xx. p. 392.), these lines occur in groups of threes, the angles which two of the lines of each group make with the edges in which the faces of the six-sided pyramid meet the faces of the six-sided prism, being 52° 5′ and 84° 40′, measured in opposite directions. It is not possible to measure these angles with much accuracy.

Let b, b', r denote the poles of $2\,\bar{1}\,\bar{1}$, $1\,1\,\bar{2}$, $1\,0\,0$ respectively; rc, rd zone-circles having their axes parallel to the lines on the face r. Then



$$rb=38^\circ\ 13',\ brc=52^\circ\ 5',$$
$$brd=84^\circ\ 40'.$$
$$\sin br=\tan bc \cot brc.$$

log sin	38° 13′	9·79144
log tan	52 5	0·11110
log tan	38 37·5	9·90254

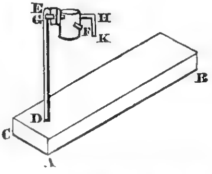
Hence c is the pole of $5\,\bar{6}\,1$. The correct value of cb is 38° 56′·8, and that of brc is 52° 34′·2. In like manner d is found to be the pole of $1\,5\,\bar{6}$. The correct value of db is 81° 3′·2, and that of brd is 84° 26′·2. A third line was too imperfect to admit of determining its direction by observation with even moderate accuracy. It probably makes an angle of 31° 53′·3 with the edge rb, and is parallel to the axis of the zone-circle re, where e is the pole of $6\,\bar{1}\,\bar{5}$. $eb=21^\circ\ 3'{\cdot}2$. We arrive at the same result supposing the lines to occur on z, a face of the rhombohedron $\bar{1}\,2\,2$, which forms a dirhombohedral combination with $1\,0\,0$.

The faces *c*, *d*, *e*, and the faces parallel to them, constitute the hemihedral form with parallel faces $\pi\ 1\ 5\ \bar{6}$.

The form $1\ 5\ \bar{6}$ was observed by Haidinger in a group of crystals of quartz from Chamonix in Mr. Allan's cabinet, now in the possession of Mr. Greg. The faces of this form are marked *d* in fig. 147, plate 27, of the second volume of Haidinger's translation of Mohs. In the figure of this crystal the faces *d* are represented as occurring on the alternate angles only of the six-sided prism; an arrangement which does not accord with the laws of symmetry that prevail in crystals of the rhombohedral system. By the kindness of Mr. Greg, I have been permitted to examine this group of crystals, and have ascertained that a pair of faces *d* exists on each of two adjacent edges, in one of the crystals of the group, but the faces are so narrow as to be scarcely perceptible. By using for the bright signal a large aperture in a screen through which sunlight was reflected from a plane mirror, the positions of these faces were determined with sufficient accuracy for the purpose of identification.

On a substitute for the Reflective Goniometer.

The methods of measuring the angles of crystals, described by M. Haidinger in the *Sitzungsberichte der Mathem.-naturw. Classe der kais. Akademie der Wissenschaften*, vol. xiv. p. 3, and vol. xvii. p. 187, and by M. Casamajor in Silliman's Journal for Sept. 1857, are highly useful in determining the forms of crystals when better instrumental means are not at command. The following description of a substitute for the reflective goniometer, which I have occasionally used and exhibited in my lectures, is offered in the hope that it also may be found serviceable under similar circumstances. It resembles the last-mentioned contrivance, but allows the crystal to be adjusted more readily, without being more difficult to construct. It consists of a rectangular parallelopiped of wood ABC, about 9 inches long, 2 inches wide, and nearly 1 inch thick, into which, near one end, is fixed a wire DEF about 0·15 inch in diameter, bent at a right angle at E, so that DE may be about 5·5 inches and EF 1·5 inch long. GHK is a wire of the same diameter bent at a right angle at H, GH being 1·5 inch, and HK 1 inch long. The wires EF, HG fit into two holes bored at right angles to each other in a small cork. On the end G of the wire GH is fixed a bit of wood 0·35 inch square, and 0·7 inch long, to the upper end of which the crystal is cemented.



A sheet of paper being fastened on a horizontal table, ABC is laid upon the table, and two signals being placed in a plane through the crystal parallel to the table, the crystal is adjusted, by means of the rotations round EF and HG, so that the edge in which the planes of two of its faces intersect may be made perpendicular to the plane ABC. ABC being turned in its own plane till the image of the bright signal in one face of the crystal coincides with the faint signal seen by direct vision, a line is drawn on the paper along the edge AB. The same process is gone through with the reflected image of the bright signal in the second face of the crystal, taking care that the intersection of the faces shall occupy as nearly as possible the same position during each observation, and a line is traced upon the paper along the edge AB. The angle between these two lines is the angle between normals to the two faces of the crystal. This angle may be measured by describing a circle round their intersection as a centre, and comparing the arc intercepted between the lines, with an arc of 60°, by the method given in Legendre's *Géométrie*, livre ii., problême xviii. Or, a circle may be described passing through the intersection of the straight lines, and the arc which they intercept compared with the whole circumference by the same method. The value of the angle in degrees is obtained by multiplying the ratio by 60, in the former case, and by 180 in the latter.

LXIV. *Analysis of Anorthite from a Diorite from the Oural Mountains.* *By* Robert H. Scott, *Esq.*[*]

PROFESSOR G. ROSE having requested me to undertake the analysis of the felspar of a Diorite which forms the Konschekowskoi Kamm near Bogoslowsk, in the northern Oural Mountains, I accordingly did so, and found it to be anorthite, the results of my analysis being,—

Anorthite.

	Per-centage.	Oxygen ratios.	
Silica	46·794	24·2969	
Alumina	33·166	15·5028	17·6338
Peroxide of iron	3·043	2·1310	
Lime	15·968	4·5408	4·9632
Magnesia	trace		
Potash	0·554	0·0939	
Soda	1·281	0·3285	
	100·806		

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[*] Communicated by the Author.

The specific gravity of the portion analysed was 2·72. The mineral is granular, not exhibiting distinct crystalline faces, and is soluble in muriatic acid.

The second constituent mineral of the Diorite, a greenish-black hornblende, has been analysed by Professor Rammelsberg (Poggendorff's *Annalen*, 1858, 4. p. 441). His analysis gives the following:—

Hornblende.

	Per-centage.	Oxygen ratios.
Silica	44·24	22·97
Alumina	8·85	4·13
Peroxide of iron . .	5·13	1·54
Protoxide of iron . .	11·80	2·62
Lime	10·82	3·09
Magnesia	13·46	5·38
Soda	2·08	0·53
Potash	0·24	0·04
Titanic acid . . .	1·01	
Fluorine	0·25	
Loss by ignition . .	0·39	
	98·27	

The mineral has a specific gravity of 3·214; is compact, with perfect cleavage.

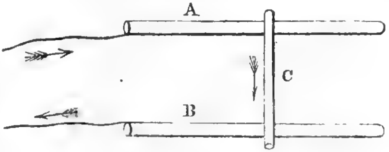
The Diorite itself is very coarse-grained, the minerals being distributed through it in irregular masses. It contains also a little quartz and some brownish-white mica.

Munich, May 3, 1858.

LXV. *Rotation of Metallic Tubes and Spheres by Electricity.*

By G. Gore, *Esq.*[*]

If two strips or pieces of metal, A and B, are placed parallel to each other and in the same horizontal plane, and a third piece of metal, C, consisting of a light tube or other form of metal capable of rolling upon A and B by a feeble impulse, is placed upon them, and a strong electric current of *quantity* is passed either from A to B, or *vice versâ* by means of C, and the smallest amount of motion be then imparted to the latter metal, it will continue to move in the direction of



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[*] Communicated by the Author.

the motion imparted to it, provided the resistances to its motion are sufficiently small and uniform; or if the resistances are not uniform, it will continue to vibrate or roll backwards and forwards as long as the electric current is passing.

This effect I have obtained in several ways by forming the cylinder of various metals, of different diameters, lengths and weights; and the rails of various metals, thin sheet, plate, and stretched wire of different thicknesses, with various degrees of success. Upon the small scale I have obtained the backward and forward motion thus:—Two thin strips of brass, $\frac{1}{2}$ an inch wide and 10 inches long, were affixed parallel to each other upon the opposite sides of a bar of wood about 3 inches wide; their edges rising one-eighth of an inch above the surface of the wood and forming two metallic rails. A thin zinc tube, $3\frac{1}{2}$ inches long and 2 inches diameter, weighing about 300 grains, was placed upon the rails, and the latter connected with two pairs of Smee's battery, strongly charged, and containing single plates of zinc and platinized silver, 8 inches by 10 inches.

To obtain a continuous rolling motion in one uniform direction, I have constructed the following apparatus (see figures):—A is a circular base of wood provided with two brass rails or hoops, B and C, about $\frac{1}{35}$th of an inch thick; level, uniform, and equidistant, and metallically connected with the screws D and E; the outer one is a quarter of an inch higher than the other. F is a thin copper ball, hollow, of uniform thickness, weighing about 500 grains, and as round as it can be made.

The base having been set perfectly level, the screws D and E connected with a suitable battery, *i. e.* one yielding a current of copious quantity and moderate intensity*, and the ball placed upon the rails, the ball immediately began to vibrate, and increased its motion until complete rotation and revolution was obtained; and it continued to revolve in either direction with equal facility as long as the current was passing. The ball also at the same time became much heated.†

As some difficulty may be experienced in obtaining a suitable ball, the following particulars are given:—A thick ring of brass was turned in a lathe to an internal diameter the same as that of the ball; it was then divided into two equal parts, and one of them used as a pattern from which to form in a lathe a hemispherical "chuck" of hard and dry wood; a disc of thin copper was then placed against the wood and formed into a cup by the

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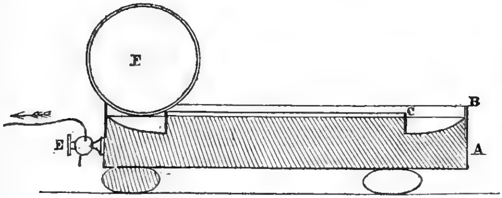
* Three zinc and carbon batteries, with zinc cylinders 6 inches high and $3\frac{1}{2}$ inches diameter, strongly charged with dilute sulphuric and strong nitric acids, and connected together as one pair, propelled the ball at a rate of sixteen revolutions per minute.

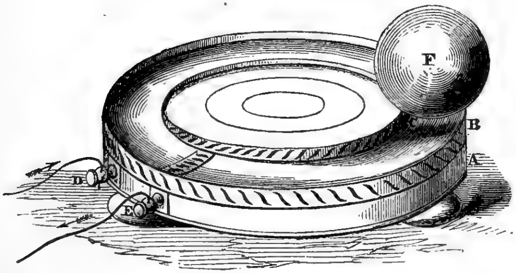
† Experiments exhibited before the Royal Society, June 3, 1858.

mechanical process termed "spinning." Two hemispherical cups were thus formed, and were reduced to the requisite degree of lightness and to equal weights by repeatedly filling them with weak nitric acid; they were then soldered together, and their remaining slight inequalities removed by mechanical means.

Sectional view. Scale $\frac{1}{3}$rd.



Perspective.



In all cases yet observed, the motion has been attended by a peculiar crackling sound at the surfaces of contact, and by heating of the rolling metal; and in experiments upon a large scale with thick tubes, strong vibrations accompanied by emission of musical sounds were observed, similar in a moderate degree to Trevelyan's experiment with heated metals. In a dark place electric sparks appeared occasionally at the points of contact, especially as the tube or sphere rolled *from* the spectator. The motion did not occur either when the rolling metal was too light to maintain sufficient contact with the rails, nor when it was too heavy; also it did not occur when the electric current was too feeble, nor when it was too strong and partly fused the metals at their points of contact. Tubes of small diameter acted less powerfully than larger ones.

The revolution of the ball does not appear to be an effect of ordinary electro-magnetic action, either of the fixed conductors upon the moveable one, or of the magnetism of the earth upon it; because in whichever direction the electric current may be passing, either from D to E, or *vice versâ*, the direction of the motion is not affected; the ball will also, with either direction of current, revolve equally well in opposite directions. The cause of the motion *appears* to be an intermittent thermic action taking place at the surfaces of contact, at a point a minute distance behind the line of the centre of gravity of the rolling metal.

These experiments had their origin in a phænomenon observed by Mr. Fearn of Birmingham, in his electro-gilding establishment;—that a tube of brass, $\frac{1}{2}$ an inch in diameter and 4 feet long, placed upon two horizontal and parallel brass tubes, 1 inch in diameter and 9 feet long, and at right angles to them, and the latter connected with a strong voltaic battery consisting of from 2 to 20 pairs of large zinc and carbon elements, the transverse tube immediately began to vibrate and finally to roll upon the others.

Birmingham.

LXVI. *On the Singular Solutions of Differential Equations. By the* Rev. Robert Carmichael, *M.A., Fellow of Trinity College, Dublin*[*].

THE objects proposed in the following memoir are briefly:—

(1) The transformation into a symmetrical form of Clairaut's well-known theorem for the integration of a certain class of ordinary differential equations in a single independent variable, and the simultaneous determination of their singular solutions; the generalization of this transformed type, and the application of the method thence suggested to the determination of the singular solutions of an extensive class of partial differential equations. Of this application copious illustrations are given.

(2) The examination of the theory attributed to Laplace, for the determination of the singular solutions of differential equations, where they admit of such, from the differential equations themselves, without the knowledge of the general integrals.

(3) The indication of certain desiderata for the completion of the general theory.

1. In the memoir by Clairaut, to which allusion has been already made, a theorem, now well known, was given for the integration of a certain class of ordinary differential equations in a single independent variable, and the determination at the same time of their singular solutions. The difficulty of apprehending

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[*] Communicated by the Author.

this theorem as a practical truth admitting of large and useful application, and the still higher difficulty of generalizing it with a view to the integration of corresponding classes of partial differential equations in any number of independent variables, and the simultaneous determination of their singular solutions, seem to have arisen in a great measure from the unsymmetrical form in which this theorem happens to have been presented.

Stated in a symmetrical manner, the differential equation with which Clairaut was concerned assumes the shape

$$x\frac{du}{dx}+y\frac{du}{dy}-\mathrm{F}_1\left(\frac{du}{dx},\frac{du}{dy}\right)=0,$$

where F_1 is any homogeneous function of the first order in the quantities included within the brackets.

At a glance we perceive that the general solution, $u=0$, of any differential equation of this type is

$$\alpha x+\beta y-\mathrm{F}_1(\alpha,\beta)=0,$$

where α and β are any arbitrary constants, and that the singular solution is due to the elimination of α, β between this general solution and the system of equations

$$\left.\begin{aligned} x-\frac{d\mathrm{F}_1}{d\alpha}&=0,\\ y-\frac{d\mathrm{F}_1}{d\beta}&=0.\end{aligned}\right\}$$

But, more generally, we see that if we were given any partial differential equation of the type

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}-\mathrm{F}_1\left(\frac{du}{dx},\frac{du}{dy},\frac{du}{dz}\right)=0,$$

its general solution, $u=0$, is

$$\alpha x+\beta y+\gamma z-\mathrm{F}_1(\alpha,\beta,\gamma)=0,$$

where α, β, γ are any arbitrary constants, and that the singular solution is due to the elimination of α, β, γ between this general solution and the system of equations

$$\left.\begin{aligned} x-\frac{d\mathrm{F}_1}{d\alpha}&=0,\\ y-\frac{d\mathrm{F}_1}{d\beta}&=0,\\ z-\frac{d\mathrm{F}_1}{d\gamma}&=0.\end{aligned}\right\}.$$

It will be remarked, that in the previous case, although there are in appearance two arbitrary constants, there is in reality but one (as there should be), since the equation may be divided off by α. In the present case we see that there are in reality but two independent arbitrary constants in the general solution; and as one of these may be represented by an arbitrary function of the other, the complete solution of the differential equation proposed may, with Monge, be represented by the system of equations, exhibiting one arbitrary function,

$$\left.\begin{aligned} 0&=x+by+\Phi(b)z-F_1\{1,\ b,\ \Phi(b)\},\\ 0&=\quad y+\Phi'(b)z-\left(\frac{dF_1}{db}\right), \end{aligned}\right\}$$

where b is any arbitrary constant.

In general, we see that for all partial differential equations, in any number of independent variables, of the type

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+w\frac{du}{dw}+\text{\&c.}-F_1\left(\frac{du}{dx},\frac{du}{dy},\frac{du}{dz},\frac{du}{dw},\text{\&c.}\right)=0,$$

the form of the general (or *primary*, in the nomenclature proposed by Professor De Morgan) solution is

$$\alpha x+\beta y+\gamma z+\delta w+\text{\&c.}-F_1(\alpha,\ \beta,\ \gamma,\ \delta,\ \text{\&c.})=0,$$

where α, β, γ, δ are n arbitrary constants, as many in number as the n variables x, y, z, &c.; that the singular solution is obtained by eliminating α, β, γ, &c. between this general solution and the system of equations

$$\left.\begin{aligned} x-\frac{dF_1}{d\alpha}&=0,\\ y-\frac{dF_1}{d\beta}&=0,\\ z-\frac{dF_1}{d\gamma}&=0,\\ \text{\&c.};& \end{aligned}\right\}$$

and that the complete solution is given by a system of equations analogous to the last, and apparently exhibiting $(n-2)$ arbitrary functions. This latter point will deserve attention, as it appears that the number of arbitrary functions which the complete solution of any partial differential equation of the first order can exhibit is not necessarily limited to one, and is in certain cases dependent on the number of independent variables.

A similar method of solution would appear to be applicable to partial differential equations of higher orders.

The utility of the observations now made will be best exhibited by their application to some examples; and it will be observed that it is only as stated in their primary form that the theory, which may be fairly said to be due to Lagrange, for the deduction of singular solutions from the integral expressions, applies, in the case of partial differential equations in general.

It will be remarked, too, that in the generalized form of Clairaut's equation, the result of the elimination indicated for the determination of the singular solution required is the same as the result of the elimination of $\frac{du}{dx}, \frac{du}{dy}, \frac{du}{dz}$, &c. between the original equations and its *dérivées* with respect to these quantities.

Examples.

I. Let it be proposed to investigate the singular solution of the partial differential equation

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\text{\&c.}=\left\{a^m\left(\frac{du}{dx}\right)^m+b^m\left(\frac{du}{dy}\right)^m+c^m\left(\frac{du}{dz}\right)^m+\text{\&c.}\right\}^{\frac{1}{m}}.$$

The primary or general solution of this equation is, as we have just seen,

$$\alpha x+\beta y+\gamma z+\text{\&c.}=\{a^m\alpha^m+b^m\beta^m+c^m\gamma^m+\text{\&c.}\}^{\frac{1}{m}},$$

where α, β, γ, &c. are arbitrary constants as many in number as the variables x, y, z, &c.

Differentiating this equation with respect to α, β, γ, &c. severally, and denoting the expression within the brackets in the right-hand member by U, we get

$$\left.\begin{aligned} x&=\frac{a^m\alpha^{m-1}}{U^{\frac{m-1}{m}}},\\ y&=\frac{b^m\beta^{m-1}}{U^{\frac{m-1}{m}}},\\ z&=\frac{c^m\gamma^{m-1}}{U^{\frac{m-1}{m}}},\\ &\text{\&c.}\end{aligned}\right\}$$

Hence, by an obvious process of elimination, we obtain as the desired singular solution,

$$\left(\frac{x}{a}\right)^{\frac{m}{m-1}}+\left(\frac{y}{b}\right)^{\frac{m}{m-1}}+\left(\frac{z}{c}\right)^{\frac{m}{m-1}}+\text{\&c.}=1.$$

We shall find that it is only necessary to substitute, in this result, particular values for m and limit the number of variables, in order to obtain not merely the singular solutions of the greater number of the differential equations, total or partial, which have been at various times proposed as examples, but a large variety of new and interesting results besides.

(α) Thus, let $m=3$, and we find that the singular solution of the equation

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\&c.=\left\{a^3\left(\frac{du}{dx}\right)^3+b^3\left(\frac{du}{dy}\right)^3+c^3\left(\frac{du}{dz}\right)^3+\&c.\right\}^{\frac{1}{3}}$$

is

$$\left(\frac{x}{a}\right)^{\frac{3}{2}}+\left(\frac{y}{b}\right)^{\frac{3}{2}}+\left(\frac{z}{c}\right)^{\frac{3}{2}}+\&c.=1\,;$$

and if the number of variables x, y, z, &c. be reduced to two, that the singular solution of the equation

$$x\frac{du}{dx}+y\frac{du}{dy}=\left\{a^3\left(\frac{du}{dx}\right)^3+b^3\left(\frac{du}{dy}\right)^3\right\}^{\frac{1}{3}}$$

is

$$\left(\frac{x}{a}\right)^{\frac{3}{2}}+\left(\frac{y}{b}\right)^{\frac{3}{2}}=1.$$

(β) Again, let $m=2$, and we find that the singular solution of the equation

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\&c.=\left\{a^2\left(\frac{du}{dx}\right)^2+b^2\left(\frac{du}{dy}\right)^2+c^2\left(\frac{du}{dz}\right)^2+\&c.\right\}^{\frac{1}{2}}$$

is

$$\left(\frac{x}{a}\right)^2+\left(\frac{y}{b}\right)^2+\left(\frac{z}{c}\right)^2+\&c.=1.$$

When the number of variables x, y, z is reduced to three, we have the known theorem that the envelope of the series of developable surfaces whose general partial differential equation is

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}=\left\{a^2\left(\frac{du}{dx}\right)^2+b^2\left(\frac{du}{dy}\right)^2+c^2\left(\frac{du}{dz}\right)^2\right\}^{\frac{1}{2}},$$

is the ellipsoid

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1.$$

It is worth remarking, that the differential equation which first suggested to Taylor the existence of a solution not involved in the general integral, "singularis quædam solutio problematis," as quoted by Lagrange from the *Methodus Incrementum* (p. 27), namely,

$$y^2-2xy\frac{dy}{dx}+(1+x^2)\left(\frac{dy}{dx}\right)^2=1,$$

comes under this example, as a particular case. In fact, if we reduce this to a symmetrical form, we obtain

$$\left(x\frac{du}{dx}+y\frac{du}{dy}\right)^2=\left(\frac{du}{dy}\right)^2-\left(\frac{du}{dx}\right)^2,$$

the singular solution of which is

$$1=y^2-x^2;$$

while the general integral is

$$(\alpha x+\beta y)^2=\beta^2-\alpha^2,$$

where α and β are arbitrary constants.

More generally, the singular solution of the equation

$$\left(x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\text{\&c.}\right)^2=c^2\left(\frac{du}{dz}\right)^2-a^2\left(\frac{du}{dx}\right)^2-b^2\left(\frac{du}{dy}\right)^2-\text{\&c.}$$

is

$$1=\frac{z^2}{c^2}-\frac{x^2}{a^2}-\frac{y^2}{b^2}-\text{\&c.}$$

(γ) When $m=1$, we must have recourse to the original equation, and we find that the singular solution in this case is the aggregate of the equations

$$\left.\begin{array}{r}x-a=0,\\ y-b=0,\\ z-c=0,\\ \text{\&c.}\end{array}\right\}$$

In the case in which the number of variables x, y, z, &c. is reduced to three, the geometrical interpretation of this result is, that the envelope of all the cones, whose common vertex is the point whose coordinates are a, b, c, is that point itself.

(δ) When $m=-1$, we find that the singular solution of the equation

$$\left(x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\text{\&c.}\right)\left(\frac{1}{a\frac{du}{dx}}+\frac{1}{b\frac{du}{dy}}+\frac{1}{c\frac{du}{dz}}+\text{\&c.}\right)=1,$$

is

$$\left(\frac{x}{a}\right)^{\frac{1}{2}}+\left(\frac{y}{b}\right)^{\frac{1}{2}}+\left(\frac{z}{c}\right)^{\frac{1}{2}}+\text{\&c.}=1.$$

If $a=b=c=n$, and the number of independent variables be reduced to three, it is obvious that if P be the perpendicular from the origin on the tangent plane to the surface indicated, and α, β, γ the angles made by it with the axes of coordinates, the equation

$$\left(x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}\right)\left(\frac{1}{\frac{du}{dx}}+\frac{1}{\frac{du}{dy}}+\frac{1}{\frac{du}{dz}}\right)=n$$

is equivalent to

$$\mathrm{P}\left(\frac{1}{\cos\alpha}+\frac{1}{\cos\beta}+\frac{1}{\cos\gamma}\right)=n.$$

Hence we infer the known theorem, that the surface

$$\sqrt{x}+\sqrt{y}+\sqrt{z}=\sqrt{n}$$

is such that the sum of the intercepts upon the axes made by any tangent plane is constant and equal to n.

(ϵ) When $m=-2$, we find that the singular solution of the equation

$$\left(x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\&c.\right)\left\{\frac{1}{a^2\left(\frac{du}{dx}\right)^2}+\frac{1}{b^2\left(\frac{du}{dy}\right)^2}+\frac{1}{c^2\left(\frac{du}{dz}\right)^2}+\&c.\right\}^{\frac{1}{2}}=1$$

is

$$\left(\frac{x}{a}\right)^{\frac{2}{3}}+\left(\frac{y}{b}\right)^{\frac{2}{3}}+\left(\frac{z}{c}\right)^{\frac{2}{3}}+\&c.=1.$$

If the number of variables x, y, &c. be reduced to two, and $a=b=n$, we find that the singular solution of the equation

$$\left(x\frac{du}{dx}+y\frac{du}{dy}\right)\left\{\frac{1}{\left(\frac{du}{dx}\right)^2}+\frac{1}{\left(\frac{du}{dy}\right)^2}\right\}^{\frac{1}{2}}=n$$

is

$$x^{\frac{2}{3}}+y^{\frac{2}{3}}=n^{\frac{2}{3}},$$

the equation to a hypocycloid. As this curve is known to be the envelope of a right line of constant length, whose extremities move on two rectangular axes, we have thus arrived at the geometrical interpretation of this differential equation, an interpretation readily confirmed *à posteriori*.

More generally we see that the singular solution of the equation

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}=\left\{\frac{1}{n^2\left(\frac{du}{dx}\right)^2}+\frac{1}{n^2\left(\frac{du}{dy}\right)^2}+\frac{1}{n^2\left(\frac{du}{dz}\right)^2}\right\}^{-\frac{1}{2}}$$

is

$$x^{\frac{2}{3}}+y^{\frac{2}{3}}+z^{\frac{2}{3}}=n^{\frac{2}{3}};$$

and that this again is, as is known, the equation to the surface such that the sum of the squares of the intercepts upon the axes made by any tangent plane is constant and equal to n^2.

(ζ) When $m=-3$, we find that the singular solution of the equation

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\&c.=\left\{\frac{1}{a^3\left(\frac{du}{dx}\right)^3}+\frac{1}{b^3\left(\frac{du}{dy}\right)^3}+\frac{1}{c^3\left(\frac{du}{dz}\right)^3}+\&c.\right\}^{-\frac{1}{3}}$$

is
$$\left(\frac{x}{a}\right)^{\frac{3}{4}}+\left(\frac{y}{b}\right)^{\frac{3}{4}}+\left(\frac{z}{c}\right)^{\frac{3}{4}}+\&c.=1,$$

and the geometrical corollary, analogous to those in the two previous cases, is obvious.

(η) Hitherto the values assigned to m have been integers; but if we suppose $m=\frac{3}{2}$, we find that the singular solution of the equation, taken for simplicity in three independent variables,

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}=\left\{\left(a\frac{du}{dx}\right)^{\frac{3}{2}}+\left(b\frac{du}{dy}\right)^{\frac{3}{2}}+\left(c\frac{du}{dz}\right)^{\frac{3}{2}}\right\}^{\frac{2}{3}}$$

is the symmetrical surface of the third order

$$\frac{x^3}{a^3}+\frac{y^3}{b^3}+\frac{z^3}{c^3}=1.$$

Indeed we might have easily proved independently, that the perpendicular upon the tangent plane at any point of the surface

$$\frac{x^3}{a^3}+\frac{y^3}{b^3}+\frac{z^3}{c^3}=1,$$

is given by the equation

$$p^{\frac{3}{2}}=a^{\frac{3}{2}}\cos^{\frac{3}{2}}\lambda+b^{\frac{3}{2}}\cos^{\frac{3}{2}}\mu+c^{\frac{3}{2}}\cos^{\frac{3}{2}}\nu,$$

where λ, μ, ν are the angles made by the perpendicular with the axes of coordinates.

(θ) Again, if we suppose $m=\frac{1}{2}$, we find that the singular solution of the equation, taken, as before, in three independent variables,

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}=\left\{\left(a\frac{du}{dx}\right)^{\frac{1}{2}}+\left(b\frac{du}{dy}\right)^{\frac{1}{2}}+\left(c\frac{du}{dz}\right)^{\frac{1}{2}}\right\}^{2}$$

is given by the equation

$$\frac{a}{x}+\frac{b}{y}+\frac{c}{z}=1.$$

II. Let it be proposed to investigate the singular solution of the equation

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\&c.=\mathrm{A}\left(\frac{du}{dx}\right)^{\frac{a}{m}}.\left(\frac{du}{dy}\right)^{\frac{b}{m}}.\left(\frac{du}{dz}\right)^{\frac{c}{m}}.\ldots,$$

where

$$a+b+c+\&c.=m.$$

The general solution of this equation is, as we have seen,

$$\alpha x+\beta y+\gamma z+\&c.=\mathrm{A}\alpha^{\frac{a}{m}}.\beta^{\frac{b}{m}}.\gamma^{\frac{c}{m}}\ldots,$$

where α, β, γ, &c. are arbitrary constants.

Differentiating this expression with respect to α, and for brevity putting the right-hand member equal to K, we obtain

$$x=\frac{a\mathrm{K}}{m\alpha},\quad y=\frac{b\mathrm{K}}{m\beta},\quad z=\frac{c\mathrm{K}}{m\gamma},\ \&c.,$$

whence

$$\alpha=\frac{a\mathrm{K}}{mx},\quad \beta=\frac{b\mathrm{K}}{my},\quad \gamma=\frac{c\mathrm{K}}{mz},\ \&c.,$$

and the singular solution desired is

$$m.x^{\frac{a}{m}}.y^{\frac{b}{m}}.z^{\frac{c}{m}}\ldots=\mathrm{A}a^{\frac{a}{m}}.b^{\frac{b}{m}}.c^{\frac{c}{m}}\ldots,$$

or

$$m^m.x^a y^b z^c\ldots=\mathrm{A}^m.a^a b^b c^c\ldots$$

If the number of variables x, y, z, &c. be reduced to three, and

$$a=b=c=1,$$

we see that the singular solution of the equation

$$x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}=\mathrm{A}\left(\frac{du}{dx}\right)^{\frac{1}{3}}.\left(\frac{du}{dy}\right)^{\frac{1}{3}}.\left(\frac{du}{dz}\right)^{\frac{1}{3}},$$

or

$$\left\{x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}\right\}^3=\mathrm{A}^3\frac{du}{dx}\cdot\frac{du}{dy}\cdot\frac{du}{dz},$$

is

$$27\,xyz=\mathrm{A}^3.$$

It is known independently that this is the equation of the surface such that the product of the intercepts made upon the rectangular axes of coordinates by any tangent plane is constant, the volume of the constant pyramid being to the volume of the parallelopiped formed by the coordinates of the point of contact as 27 : 6, or as $4\frac{1}{2}$: 1. It is easy to identify the two results by dividing both sides of the last equation by

$$\left\{\left(\frac{du}{dx}\right)^2+\left(\frac{du}{dy}\right)^2+\left(\frac{du}{dz}\right)^2\right\}^{\frac{3}{2}}.$$

III. The singular solution of the partial differential equation

$$a\frac{du}{dx}\left(x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}+\&c.\right)+b^2\left(\frac{du}{dy}\right)^2+c^2\left(\frac{du}{dz}\right)^2+\&c.=0$$

is easily determined in a similar manner.

The general solution is

$$a\alpha(\alpha x+\beta y+\gamma z+\&c.)+b^2\beta^2+c^2\gamma^2+\&c.=0,$$

where, as before, α, β, γ, &c. are arbitrary constants, and the singular solution desired is readily determined to be

$$\frac{y^2}{b^2}+\frac{z^2}{c^2}+\frac{w^2}{d^2}+\&c.=\frac{4x}{a}.$$

When the number of variables x, y, z, &c. is reduced to three, it appears then that the singular solution of the equation

$$a\frac{du}{dx}\left(x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}\right)+b^2\left(\frac{du}{dy}\right)^2+c^2\left(\frac{du}{dz}\right)^2=0$$

is

$$\frac{y^2}{b^2}+\frac{z^2}{c^2}=\frac{4x}{a},$$

a result which is at once verified by the known properties of the paraboloid.

2. Thus far we have been employed in the derivation of the singular solutions of partial differential equations *from their general or primary integrals*, the method being suggested by an easy modification and subsequent generalization of Clairaut's theorem, and the application of the method being illustrated by examples sufficiently numerous. I now proceed to consider a general method which has been proposed for determining the singular solutions of differential equations, both total and partial, *from the equations themselves, without the knowledge of the general or primary solution.*

The following theory for differential equations of the first order has been attributed to Laplace:—"Let U$=0$ be a differential equation of the first order between x and y, cleared of radicals and fractions; then if we represent $\frac{dy}{dx}$ by p, the relations between x and y found by eliminating p between

$$\text{U}=0,\quad \frac{d\text{U}}{dp}=0,$$

are singular solutions of U$=0$, *provided they satisfy that equation and do not at the same time make*

$$\frac{d\text{U}}{dy}=0;$$

we might also deduce the singular solutions by eliminating $\frac{dx}{dy}$ between

$$\text{U}=0,\quad \frac{d\text{U}}{dp_1}=0,$$

where $p_1=\frac{dy}{dx}$, provided that they satisfy the given equation and do not at the same time make

$$\frac{d\mathrm{U}}{dx}=0."$$

Now upon this it may be remarked, that in the last and best collection of examples on the higher branches of the integral calculus which has been published, it is somewhat singular that the instances which are cited as illustrative and confirmatory of this theory, are all merely cases of Clairaut's theorem, in which the singular solution is readily determined from the general solution, and where, as the applicability of the theory appears to be accidental, so the truth of the results obtained by its application furnishes no verification: in fact, that as this method coincides in all its steps with that which determines the singular solutions by elimination of the arbitrary constants from the primary solution, and which must be true, so the verity of the results obtained by the theory above stated cannot be looked upon as confirmative of either its adequacy or its generality. But, more than this, in the cases cited in the justly-valued collection of examples above referred to (Gregory's 'Examples on the Differential and Integral Calculus,' edited by Walton, Cambridge, 1846), which do not fall under the Clairaut type, the theory appears to fail, and the characteristics of such cases of apparent failure deserve attention.

In proof of the statement now made, the examples proposed in illustration of the theory shall be cited *seriatim*.

(1) $xp^2-yp+m=0.$

(2) $y+(y-x)p+(a-x)p^2=0.$

(3) $y^2-2xyp+(1+x^2)p^2=1.$

(4) $x^2+2xyp+(a^2-x^2)p^2=0.$

(6) $(1+p^2)(y-xp)^2-a^2p^2=0.$

Now the equations numbered (1), (2), (3), (6), are severally reducible to the forms

$$(xp-y)+\frac{m}{p}=0,$$

$$y-xp+\frac{ap^2}{1+p}=0,$$

$$(y-xp)^2+(p^2-1)=0,$$

$$(y-xp)^2-\frac{a^2p^2}{1+p^2}=0,$$

which are all of the Clairaut type; while the example (4) is due to the combination of the equation

$$y-px=a\sqrt{1+p^2},$$

also of the Clairaut type, with its singular solution

$$x^2+y^2=a^2.$$

The above examples, when reduced to a symmetrical form, would appear thus:—

(1)′ $$\left(x\frac{du}{dx}+y\frac{du}{dy}\right)\frac{du}{dx}+m^2\left(\frac{du}{dy}\right)^2=0,$$

(2)′ $$\left(x\frac{du}{dx}+y\frac{du}{dy}\right)\left(\frac{du}{dx}-\frac{du}{dy}\right)=a\left(\frac{du}{dy}\right)^2,$$

(3)′ $$\left(x\frac{du}{dx}+y\frac{du}{dy}\right)^2=\left(\frac{du}{dy}\right)^2-\left(\frac{du}{dx}\right)^2,$$ (Taylor's equation)

(6)′ $$\left(x\frac{du}{dx}+y\frac{du}{dy}\right)^2\left\{\left(\frac{du}{dx}\right)^2+\left(\frac{du}{dy}\right)^2\right\}=a^2,$$

the general solutions of which are, as we have seen, severally

$$(\alpha x+\beta y)\alpha+m^2\beta^2=0,$$
$$(\alpha x+\beta y)(\alpha-\beta)=a\beta^2,$$
$$(\alpha x+\beta y)=\beta^2-\alpha^2,$$
$$(\alpha x+\beta y)^2(\alpha^2+\beta^2)=a^2,$$

and the singular solutions

$$y^2-4mx=0,$$
$$(x+y)^2-4ay=0,$$
$$y^2-x^2=1,$$ (Taylor's solution)
$$x^{\frac{2}{3}}+y^{\frac{2}{3}}=a^{\frac{2}{3}}.$$

It is worthy of remark, too, that in the case of the examples numbered (5) and (7) in the same collection, namely,

$$p^2+yp+x=0,$$
$$y^2p^2-2xyp+ax+by=0,$$

the method when applied seems to fail, inasmuch as the results obtained, respectively,

$$y^2-4x=0,$$
$$ax+by-x^2=0,$$

cannot be regarded as any solutions at all, inasmuch as they do not satisfy the corresponding equations at all.

Now it appears pretty evident that this result might have been anticipated beforehand, and that the theory is deficient in not indicating any of the characteristics by which such equations as do not admit of its application may be *à priori* recognized.

Light seems to be thrown upon the subject by the circumstance that, as is readily seen, the equations last written are, respectively, the conditions that the previous equations to which they belong should have equal roots, the former being regarded as a quadratic in p, the latter in yp, the result in neither case being reducible to the Clairaut type. Again, in the equation numbered (8) in the same collection,

$$(xp-y)(xp-2y)+x^3=0,$$

or

$$(xp-y)^2-(xp-y)y+x^3=0,$$

the condition for equal roots in $(xp-y)$ is

$$y^2-4x^3=0\,;$$

and the result obtained by the theory is a true singular solution, inasmuch as it satisfies the given equation. It appears, too, that if P be the perpendicular upon the tangent at any point of this parabolic curve, and α the angle which it makes with the axis of x, the following relation always holds,

$$\mathrm{P}^2+\mathrm{P}y\sin\alpha+x^3\sin^2\alpha=0.$$

The remark previously made will perhaps be more readily apprehended by observing that the symmetrical form of the above equation is

$$\left(x\frac{du}{dx}+y\frac{du}{dy}\right)^2+\left(x\frac{du}{dx}+y\frac{du}{dy}\right)y\frac{du}{dy}+x^3\left(\frac{du}{dy}\right)^2=0,$$

which becomes in the case stated,

$$\left(x\frac{du}{dx}+y\frac{du}{dy}\right)+\frac{y}{2}\frac{du}{dy}=x\frac{du}{dx}+y^{\frac{2}{3}}\frac{du}{dy^{\frac{2}{3}}}=0,$$

an equation of the linear or Clairaut type, and the solution of which is

$$u=u_0(x,\, y^{\frac{2}{3}})=0,$$

where u_0 is any homogeneous function of the order 0.

The necessity of clearing any proposed equation of radicals and fractions is easily intelligible, from the consideration that any equation linear with respect to p, but which involves radicals or fractions, can only be regarded as a factor of some equation of higher degree. In equations reducible to the Clairaut type, it is evident from the examples previously given that such a preliminary operation is unnecessary. As regards the alleged necessity of verification by substitution of any supposed singular solution in the given equation, the following example will suffice. If it had been proposed to investigate the singular solution of the equation

$$\Phi(x, y, z, \&c.)\frac{du}{dx}+\Psi(x, y, z, \&c.)\frac{du}{dy}+\mathrm{X}(x, y, z, \&c.)\frac{du}{dz}+\&c.$$

$$=$$

$$\left\{a^m\left(\frac{du}{dx}\right)^m+b^m\left(\frac{du}{dy}\right)^m+c^m\left(\frac{du}{dz}\right)^m+\&c.\right\}^{\frac{1}{m}},$$

it might be supposed that, generalizing the above theory, and eliminating $\frac{du}{dx}, \frac{du}{dy}, \frac{du}{dz}$, &c. between this equation and its *dérivées* with respect to these same quantities, we should obtain as its singular solution,

$$\left(\frac{\Phi}{a}\right)^{\frac{m}{m-1}}+\left(\frac{\Psi}{b}\right)^{\frac{m}{m-1}}+\left(\frac{\mathrm{X}}{c}\right)^{\frac{m}{m-1}}+\&c.=1.$$

But, upon deriving from this equation the values of the partial differential coefficients $\frac{du}{dx}, \frac{du}{dy}, \frac{du}{dz}$, &c., and substituting them in the equation proposed, we find that it is not satisfied; consequently the expression which we have obtained, though the result of the required elimination, and perhaps in a certain unrecognized sort an integral, is not the singular solution properly so called. In fact we may regard the failure of the method as in itself significant, indicating that the question admits of no singular solution; or, in other words, that the geometrical property symbolized by the differential equation and the curve or surface, as the case may be, symbolized by its general integral, are incompatible with the condition of the admissibility of an envelope.

3. The theory above stated as attributed to Laplace, is rather his by implication than expressly. The form in which the differential equation of the first order was regarded and discussed by him was that of

$$p=f(x, y);$$

and Laplace argued, that if we derive from this equation the value of

$$\frac{dp}{dy}=\frac{\mathrm{M}}{\mathrm{N}},$$

then all the factors of $\mathrm{N}=0$, which satisfy the differential equation, are singular solutions; but the existence and significance of those factors which do not satisfy the differential equation are not explained. The theory previously quoted is an obvious consequence of that which has been now stated, but it has omitted to account for or interpret the condition, unproductive, indeed, but stated as indispensable,

$$\frac{d\mathrm{U}}{dy} > < 0,$$

in the case in which

$$\mathrm{U}=f\left(x,\ y,\ \frac{dy}{dx}\right)=0,$$

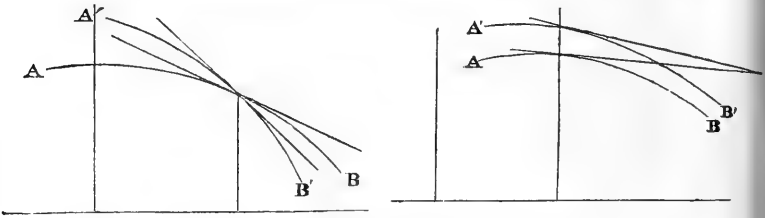
and

$$\frac{d\mathrm{U}}{dx} > < 0,$$

in the case in which

$$\mathrm{U}=f\left(x,\ y,\ \frac{dx}{dy}\right)=0.$$

If we confine our attention for a moment to the former case, the interpretation of the condition $\frac{d\mathrm{U}}{dy}$ *not* $=0$ appears to be,—if a change of form be attributed to the function y, as well as an increment to $\frac{dy}{dx}$,—no envelope; and the geometrical confirmation of this is obvious enough from the accompanying figure.



To the theory this condition is indispensable; but then an analogous condition or conditions must be regarded as indispensable in the investigation of the singular solutions of differential equations of the second and higher orders; and none such are given. Moreover, in the investigation of the singular solutions of partial differential equations of the first order, represented in general by the type

$$\mathrm{U}=f(x,\ y,\ z,\ p,\ q)=0,$$

we should expect to find stated, as a condition indispensable to the theory, that the result of the elimination of p and q between the equations

$$\mathrm{U}=0,\quad \frac{d\mathrm{U}}{dp}=0,\quad \frac{d\mathrm{U}}{dq}=0,$$

should at the same time render

$$\frac{dU}{dz} > \text{or} < 0,$$

this condition being similarly interpretable as above in the case of a single independent variable. We should look, too, for analogous conditions in the case of partial differential equations of the higher orders, but none such are given.

4. As regards partial differential equations of the second order, it may be said that, either in the way of theory or illustration, little or nothing has as yet been done; the field may be regarded, not merely as unexplored, but as almost wholly unknown. The contributions to the subject which have been made by Poisson are scanty, and too much of a tentative character, whether they be regarded as illustrative of this interesting department of science, or suggestive of the means for its development and improvement.

Proceeding indirectly from the integral expression

$$y = \frac{ax^2}{2} + bx + f(a, b),$$

where a and b are arbitrary constants, Lagrange has discussed the differential equation thence derived, and which is typical of a class, namely

$$y = xp - \frac{x^2}{2}p' + f(p', p - xp'),$$

where $p' = \frac{d^2y}{dx^2}$, and has pointed out that this equation admits of a singular solution derived from the elimination of p between this equation and

$$\frac{x^2}{2} - \frac{df}{dp'} = 0.$$

By adopting the same indirect method, we should obtain a corresponding theorem for partial differential equations.

It is suggested, as deserving of examination, whether the Laplacian theory for the determination of singular solutions of total differential equations of the first order should not be reconstructed. Both upon analytical and geometrical grounds, the condition with which it is embarrassed, and which is indispensable to its existence as a theory, appears to be unnecessary and troublesome. But if this examination result in its confirmation and retention, then must an analogous condition be laid down for partial differential equations of the first order, and corresponding conditions for all differential equations, both total and partial, of the higher orders.

It may save some useless labour to know that there are no theorems in partial differentials of the second order corresponding to that stated for the ellipsoid in partial differentials of the first order under the case of the first example denoted by (β).

Thus, in general, if R_1, R_2 be the principal radii of curvature at any point of a surface, resuming for a moment the conventional notation,

$$\frac{1}{R_1}+\frac{1}{R_2}=\frac{(1+q^2)r-2pqs+(1+p^2)t}{(1+p^2+q^2)^{\frac{3}{2}}}.$$

But for the ellipsoid, if P be the perpendicular on the tangent plane,

$$\frac{1}{R_1}+\frac{1}{R_2}=\frac{P^3}{a^2b^2c^2}\left\{a^2+b^2+c^2-(x^2+y^2+z^2)\right\}.$$

Hence, if any corresponding theorem exist, it should be determined by equating these two expressions, or by the form

$$(1+q^2)r-2pqs+(1+p^2)t=\frac{a^2+b^2+c^2-(x^2+y^2+z^2)}{a^2b^2c^2}$$
$$(a^2+b^2p^2+c^2q^2)^{\frac{3}{2}},$$

an equation which has no singular solution, properly speaking, and could have none, being linear in r, s, and t.

Again, in general we know that

$$\frac{1}{R_1R_2}=\frac{rt-s^2}{(1+p^2+q^2)^2};$$

and for the ellipsoid we know that

$$\frac{1}{R_1R_2}=\frac{P^4}{a^2b^2c^2}.$$

By identifying these expressions we obtain the equation

$$rt-s^2=\frac{1}{a^2b^2c^2}\left\{a^2+b^2p^2+c^2q^2\right\}^2,$$

but of this, again, there is no singular solution.

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LXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 482.]

June 18, 1857.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the Thermal Effects of Longitudinal Compression of Solids." By J. P. Joule, Esq., F.R.S.; and "On the Alterations of Temperature accompanying Changes of Pressure in Fluids." By Prof. W. Thomson, F.R.S.

In the further prosecution of the experiments of which an outline was given in the Phil. Mag. for September, 1857, the author has verified the theory of Professor Thomson, as applied to the thermal effects of laying weights on and taking them off metallic pillars and cylinders of vulcanized india-rubber. Heat is evolved by compression, and absorbed on removing the compressing force in every substance yet experimented on. In the case of metals, the results agree very closely with the formula in which e, the longitudinal expansion by heat under pressure, is considered the same as the expansion without pressure. It was observed, however, that all the experimental results were a little in excess of the theoretical, and it became therefore important to inquire whether the force of elasticity in metals is impaired by heat. In the first arrangements for this purpose, the actual expansion of the bars employed in the experiments was ascertained by a micrometric apparatus,—1st, when there was no tensile force, and 2nd, when a weight of 700 lbs. was hung to the extremity of the quarter-inch rods. The results, reliable to less than one-hundredth of their whole value, did not exhibit any notable effect of tensile force on the coefficient of expansion by heat. An experiment susceptible of greater delicacy was now tried. Steel wire of $\frac{1}{90}$th of an inch in diameter was wound upon a rod of iron $\frac{1}{4}$ of an inch in diameter. This was heated to redness. Then, after plunging in cold water, the spiral was slipped off. The number of convolutions of the spiral was 420, and its weight 58 grains. Its length, when suspended from one end, was 6·35 inches, but on adding to the extremity a weight of 129 grains, it stretched without sensible set to 14·55 inches. The temperature of the spiral thus stretched was raised or lowered at pleasure by putting it in, or removing it out of an oven. After several experiments it was found that between the limits of temperature 84° and 280° Fahr., each degree Centigrade of rising temperature caused the spiral to lengthen as much as ·00337 of an inch, and that a contraction of equal amount took place with each degree Centigrade of descending temperature. Hence, as Mr. James Thomson has shown that the pulling out of a spiral is equivalent to twisting a wire, it follows that the force of torsion in steel wire is decreased ·00041 by each degree of temperature.

An equally decisive result was obtained with copper wire, of which an elastic spiral was formed by stretching out a piece of soft wire, and then rolling it on a rod $\frac{1}{4}$ of an inch in diameter. The spiral thus formed consisted of 235 turns of wire, $\frac{1}{40}$ of an inch in diameter weighing altogether 230 grains. Unstretched it measured 6·7 inches, but with a weight of 1251 grains attached to it, it stretched, without set, to 10·05 inches. Experiments made with it showed an elongation of ·00157 of an inch for each degree Centigrade of elevation of temperature, and an equal shortening on lowering the temperature. The diminution of the force of torsion was in this case ·00047 per degree Centigrade*.

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* Since writing the above, I have become acquainted with M. Kupffer's researches on the influence of temperature on the elasticity of metals (*Compte*

Professor Thomson has obligingly furnished me with the following investigation:—

On the Alterations of Temperature accompanying Changes of Pressure in Fluids.

Let a mass of fluid, given at a temperature t and under a pressure p, be subjected to the following cycle of four operations in order.

(1) The fluid being protected against gain or loss of heat, let the pressure on it be increased from p to $p+\varpi$.

(2) Let heat be added, and the pressure of the fluid maintained constant at $p+\varpi$, till its temperature rises by dt.

(3) The fluid being again protected against gain or loss of heat, let its pressure be reduced from $p+\varpi$ to p.

(4) Let heat be abstracted, and the pressure maintained at p, till the temperature sinks to t again.

At the end of this cycle of operations, the fluid is again in the same physical condition as it was at the beginning, but, as is shown by the following considerations, a certain transformation of heat into work or the reverse has been effected by means of it.

In two of these four operations the fluid increases in bulk, and in the other two it contracts to an equal extent. If the pressure were uniform during them all, there would be neither gain nor loss of work; but inasmuch as the pressure is greater by ϖ during operation (2) than during operation (4), and rises during (1) by the same amount as it falls during (3), there will, on the whole, be an amount of work equal to $\varpi\, dv$, done by the fluid in expanding, over and above that which is spent on it by pressure from without while it is contracting, if dv denote a certain augmentation of volume which, when ϖ and dt are infinitely small, is infinitely nearly equal to the expansion of the fluid during operation (2), or its contraction during operation (4). Hence, considering the bulk of the fluid primitively operated on as unity, if we take

$$\frac{dv}{dt}=e,$$

to denote an average coefficient of expansion of the fluid under constant pressure of from p to $p+\varpi$, or simply its coefficient of expansion at temperature t and pressure p, when we regard ϖ as infinitely small, we have an amount of work equal to

$$\varpi\, e\, dt$$

gained from the cycle. The case of a fluid such as water below 39°·1 Fahr., which contracts under constant pressure, with an elevation of temperature, is of course included by admitting negative values for e, and making the corresponding changes in statement.

Rendu Annuel, St. Petersburg, 1856). He finds by his method of twisting and transverse oscillations that the decrease of elasticity for steel and copper is ·000471 and ·000478. Very careful experiments recently made by Prof. Thomson, indicate a slight increase of expansibility by heat in wires placed under tension.—August 1. J. P. J.

Since the fluid is restored to its primitive physical condition at the end of the cycle, the source from which the work thus gained is drawn, must be heat, and since the operations are each perfectly reversible, Carnot's principle must hold; that is to say, if θ denote the excess of temperature of the body while taking in heat above its temperature while giving out heat, and if μ denote "Carnot's function," the work gained, per unit of heat taken in at the higher temperature, must be equal to

$$\mu\,\theta.$$

But while the fluid is giving out heat, that is to say, during operation (4), its temperature is sinking from $t+dt$ to t, and may be regarded as being on the average $t+\frac{1}{2}dt$; and while it is taking in heat, that is, during operation (2), its temperature is rising from what it was at the end of operation (1) to a temperature higher by dt, or on the average exceeds by $\frac{1}{2}dt$, the temperature at the end of operation (1). The average temperature while heat is taken in consequently exceeds the average temperature while heat is given out, by just as much as the body rises in temperature during operation (1). If, therefore, this be denoted by θ, and if $\mathrm{K}\,dt$ denote the quantity of heat taken in during operation (2), the gain of work from heat in the whole cycle of operations must be equal to $\mu\,\theta\,\mathrm{K}\,dt$, and hence we have

$$\mu\,\theta\,.\,\mathrm{K}\,dt=\varpi\,e\,dt.$$

From this we find

$$\theta=\frac{e}{\mu\mathrm{K}}\varpi,$$

where, according to the notation that has been introduced, θ is the elevation of temperature consequent on a sudden augmentation of pressure from p to $p+\varpi$; e is the coefficient of expansion of the fluid, and K its capacity for heat, under constant pressure; and μ is Carnot's function, being, according to the absolute thermodynamic scale of temperature, simply the reciprocal of the temperature, multiplied by the mechanical equivalent of the thermal unit. If then t denote the absolute temperature, which we have shown by experiment* agrees sensibly with temperature by the air-thermometer Cent. with 274° added, and if J denote the mechanical equivalent of the thermal unit Centigrade, we have

$$\theta=\frac{t\,e}{\mathrm{JK}}\varpi.$$

This expression agrees in reality, but is somewhat more convenient in form, than that first given, Dynamical Theory of Heat, § 49, Phil. Mag. for September 1852.

Thus for water, the value of K, the thermal capacity of a cubic foot under constant pressure, is 63·447, and e varies from 0 to about $\frac{1}{2200}$, for temperatures rising from that of maximum density to 50° Cent., and the elevation of temperature produced by an augmentation of pressure amounting to n times 2117 lbs. per square foot (that is

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* See Part II. of our Paper "On the Thermal Effects of Fluids in Motion," Philosophical Transactions, 1854.

to say, to n atmospheres), is

$$\frac{t\,e\times2117}{1390\times63{\cdot}447}\,n.$$

For mercury, we have $\dfrac{t\,e\times2117}{1390\times28{\cdot}68}\,n.$

If, as a rough estimate, we take

$$e=\frac{t-278}{46}\times\frac{1}{2200},$$

this becomes $\dfrac{t(t-278)}{420000}n.$

If, for instance, the temperature be 300° on the absolute scale (tha is, 26° of the Centig. thermometer), we have

$$\frac{n}{636}$$

as the heating effect produced by the sudden compression of water at that temperature: so that ten atmospheres of pressure would give $\frac{1}{64}$ of a degree Cent., or about five divisions on the scale of the most sensitive of the ether thermometers we have as yet had constructed.

Thus if we take $\frac{1}{5500}$ as the value of e, this becomes

$$\frac{t}{103600}n;$$

and at temperature 26° Cent., the heating effect of ten atmospheres is found to be $\frac{1}{34}$ of a degree Cent.

Table giving the thermal effects of a pressure of ten atmospheres on water and mercury*.

Temperature.	Increase or decrease of temperature in water.	Increase of temperature in mercury.
0°	·005 decrease	·026
3°·95	·0	·0264
10°	·006 increase	·027
20°	·015 do.	·028
30°	·022 do.	·029
40°	·029 do.	·030
50°	·035 do.	·031
60°	·041 do.	·032
70°	·047 do.	·033
80°	·055 do.	·034
90°	·065 do.	·035
100°	·078 do.	·036

"Supplementary Researches on the Partition of Numbers." By Arthur Cayley, Esq., F.R.S.

The paper is supplementary to the author's memoir, "Researches on the Partition of Numbers," which comprises the two papers, abstracts of which appear in the 'Proceedings' of the Meeting of the 3rd of May, 1855. It contains some additional developments in

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* Added August 1.

relation to the theorem referred to at the conclusion of the former memoir, and an application to the determination of the expression for P (1, 2, 3, 4, 5, 6) *q*.

"On the Anatomy and Physiology of the *Spongiadæ*." By J. S. Bowerbank, F.R.S., F.L.S. &c.

"On the Fructification of certain Sphæriaceous Fungi." By Frederick Currey, Esq.

"On the Anatomy of *Tridacna*." By John Denis Macdonald, Esq., Assistant-Surgeon, R.N.

"Experimental Researches on the Influence of Efforts of Inspiration on the Movements of the Heart." By E. Brown-Séquard, M.D.

A very interesting fact, of which many circumstances have been carefully investigated by Professor Donders and Dr. S. W. Mitchell, has received a wrong explanation from those physiologists. This fact consists in a diminution of either the strength or the frequency of the beatings of the heart, when an energetic effort at breathing is made and maintained for half a minute or a little more. Professor Donders thinks that this influence of inspiration on the heart is due to a mechanical agency of the dilated lungs on this organ.

They admit that the state of the lungs has a great influence on the heart, but the principal cause of the diminution in the movements of this organ is very different from what has been supposed by Professor Donders, by Professors J. Müller, and others. It is known that when the medulla oblongata, or the par vagum are excited (either by galvanism, as the Brothers Weber have discovered, or by other means, such as a mere compression, or a sudden wound, as I have found), the heart's beatings diminish or cease entirely. Whether this stoppage be due to the cause I have attributed it to or not, is indifferent to my present object. What is important is, that in these cases an irritation on the origin of the par vagum acts through it on the heart to diminish or to destroy its action. I thought that it would be interesting to decide, if, at the time that there is an effort at inspiration, there is not also an influence of the medulla oblongata on the par vagum, more or less similar to that which exists when we galvanize or otherwise irritate the medulla oblongata. To ascertain if it is so, I have made experiments on newly-born animals, and on birds. As I have already published some of the results of my researches on newly-born animals, and as these results are not so completely decisive as those of my experiments made on birds, I will merely give here a summary of what I have seen in these last animals. I have found the same facts in ducks, geese and pigeons; but as I have repeated the experiments more frequently on the last-mentioned animals, I will speak of them only. When their abdomen has been widely opened and their heart exposed to sight, pigeons may live, as it is well known, for a long while. I wait until they are almost dying, having only one, two, or three inspirations in a minute, and then, if the weather is cold, and if the animal has lost many degrees of its temperature, I find that, at each effort it makes to inspire, the heart either almost suddenly stops, or beats much less quickly.

I have frequently seen the heart completely arrested for five or ten seconds, and twice for twenty or twenty-five seconds, in cases where there was only one respiration in two minutes. This stoppage of the heart's movements was the more remarkable, as they were at the rate of more than two hundred in a minute when the effort at inspiration took place. To decide that it was in consequence of an influence of the par vagum that this occurred, I divided this nerve in the neck, and then found that there was no more influence of the inspiration on the heart, or if there was, it consisted in an augmentation of the frequency of the movements of this organ—an augmentation due to the shaking of the heart when the chest dilated.

Sometimes, when the heart was very irritable, and when the efforts at inspiration were still frequent and not energetic (the par vagum being undivided), these efforts were accompanied, or rather immediately followed by an increase in the strength of the heart's movements, probably caused by the shaking. But always when the inspiratory efforts were energetic and rare, they coexisted with a diminution or a momentary cessation of the heart's contractions; and always in these cases the section of the par vagum has destroyed the diminishing influence of the respiratory efforts on the heart. It would be easy to show that the influence of the inspiratory effort on the central organ of circulation is comparable to the change taking place in the pupil when the globe of the eye is drawn inwards: it is an associated action.

From the facts I have found in the case of newly-born animals and birds, and from the facts observed in man by Professors J. Müller, Donders, and others, it results that, during efforts at inspiration, a nervous influence passes along the par vagum from the medulla oblongata to the heart, diminishing the movements of this organ. And as by an action of our will we may inspire with energy, it follows that we can by an influence of our will diminish the action of our heart, just as we can contract our pupil by drawing our eyes inwards.

"On the Influence of Oxygen on the vital properties of the Spinal Cord, Nerves, and Muscles." By E. Brown-Séquard, M.D.

The influence of oxygen and carbonic acid on the living tissues, has been very little investigated, either by physiologists or practitioners of medicine. I have made a great many experiments on this subject, but will relate here merely a few of them, which are sufficient to show that oxidation of the spinal cord and nerves, as well as that of muscles, increases their vital properties, sometimes in a high degree.

After the opening of the spinal canal, the dura mater being laid bare, we find that an evident hyperæsthesia appears after a short time in the parts of the body which are behind the opening, and also on the same level with it, and a little above it. I think this increase of sensibility depends on the absorption of oxygen. To ascertain that it is so, with the help of a special apparatus, immediately after laying bare the cord, I pump out the air in contact with the dura mater, and substitute for it hydrogen. Then I find that no increase of sensibility takes place, at least during several hours. I then pump out the hydrogen, and inject atmospheric air, and in a few minutes the posterior parts of the body become hyperæsthetic.

When the posterior columns of the spinal cord have been divided transversely, there is, as I found long ago, an excessive hyperæsthesia in all the parts of the body situated behind the section, and also in some of the parts immediately above it. This hyperæsthesia begins almost at once after the operation, and increases for many hours, and sometimes for one or two days. It diminishes a little afterwards, and if there is no myelitis, it continues to exist, though less than at first, for years after the operation. A part of this hyperæsthesia (its great excess during the first hours and days), I have recently found to depend chiefly upon the influence of the oxygen of the air. If we perform the section of the posterior columns of the spinal cord, and cover the wound immediately afterwards with the apparatus above-mentioned, and pump out the air and inject hydrogen, we find that there is a delay in the development of hyperæsthesia, and that it is never as considerable as when air is in contact with the injured cord. If we replace the hydrogen by air, there is a rapid increase of the hyperæsthesia, and there is still more so if we inject pure oxygen. When the hyperæsthesia has once become excessive, hydrogen does not diminish it.

If carbonic acid is injected, slight convulsive movements are produced, and sensibility soon diminishes.

Analogous experiments on sensitive and motor nerves show that in them also an increase of the vital properties is produced by oxygen, and that a diminution after an excitation is produced by carbonic acid. Experiments on the abdominal sympathetic give similar results.

Rolando and others have found that the gray matter which is in the rhomboidal ventricle of the lumbar enlargement of the spinal cord in birds, is not excitable either to give pain or produce movement. I have ascertained that normally it is but little excitable, but that when it has been exposed to the contact of air for some 10 or 15 minutes, it is extremely excitable, particularly for the production of movements. This, perhaps, explains the curious disturbance in the voluntary movements which I found some years ago in birds, on which this gray matter had been simply laid bare.

From these facts, and a great many others, it results that in the spinal cord, the sensitive and motor nerves, and the sympathetic, as well as in muscles, there is an increase of the vital properties produced by oxygen.

"Ocular Spectres, Structures and Functions, Mutual Exponents." By James Jago, A.B. Cantab., M.B. Oxon.

SECTION I.—*Introduction.*

Our visual organs are not only capable, by an adjusting lenticular system, of painting, under varying conditions, images of luminous objects, upon a membrane in special relation with the brain, but involve many adjuvant structures; and thus it happens that they reveal to us a number of adventitious phenomena—spectres as we may call them, whether caused by light at the parts that cover the eyeballs, or within them, or by any stimulus whatever affecting the special nervous tract. These must be eliminated, if we would avoid the risk of ascribing effects begotten by subordinate parts to more integral portions of the apparatus. Finally, they may be rendered serviceable for the solution of certain important points of ocular structure and function. Under the impression that so diversified a subject has not yet received all the elucidation of which it is susceptible, another *methodical* attempt to investigate it will be made in this memoir.

When light is an agent in the production of the spectral phenomena, they arise from certain rays being blocked from their course, at some obstacle they encounter, or turned aside by refraction, reflexion or inflection. And to make *precise* observations upon them, we must use fine pencils of rays which do not return to foci upon the retina. In order to estimate the relative and actual sizes, localities, and characters, of corpuscles whose shadows or images are projected upon the retina, pencils of light which are first convergent, and therefrom, by passing through foci, divergent (such as may be conveniently obtained from a small disc of light at a sufficient distance from the eye, when viewed through a lens of an inch focal length), are mainly employed, the foci being carried from before the eye to various depths in its interior*. If we neglect ocular refractions, whether a body fall in the convergent or divergent portions, the length of its shadow will be to its own, as their respective distances from the focus. With a couple of such pencils, whether a body fall in the convergent or divergent portions, the distance between its pair of shadows is to that between the foci, as that of the object from the shadow screen (retina) to that of the object from the line (parallel to the screen) which joins the foci. But the deflections of the shadow in the two sorts of rays are in *adverse* directions, so that if the axis of a single pencil were moved across the eye, whilst always kept parallel to itself, the shadows of all the objects lying in advance of the focus would travel in one direction, whilst those of objects

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* In the Allgem. Encyklop. der Physik, s. 166 (1856), in an able article on "Entoptics," Helmholtz states, that "the more decided entoptical 'methods' were established first by Listing and Brewster (1845), who were followed still later by Donders (1846–51)." The present writer refers to a paper of his own, published early in 1845, which substantially gives the methods alluded to; which are all modifications of one idea, that of obtaining a greater parallactic deviation of shadows for objects further from the retina, by means of two pencils of rays diverging from points in front of the eye, or by one moved across the optic axis. Now we can by this device only get marked differences in parallax for small differences in ocular depth for objects very near to the points of divergence, that is, near the surface of the eye; whereas the plan now proposed not only generally secures this end in a notable degree, but by placing foci between any two objects, causes their shadows to be deflected in *opposite* directions, and the more considerably as they are nearer together; besides supplying, it is believed, a variety of aids in entoptical researches.

lying behind the focus would travel in the opposite, the rate of movement being in both instances greater for objects nearer the focus. Also, whether with a couple of pencils or a single one in movement, for a given difference in ocular depth between two objects, the difference in parallactic deviation is greater as the two objects are nearer the foci or focus. Besides, generally, the picture of the contents of the eye, as shown by the convergent portion, is inverted in the divergent.

The above proportions are turned into equations which indicate, as the terms alter their values, every observed variation in the sizes of the shadows, parallactic deviations, inversions of figure, place, and movements whether of the ocular bodies with respect to the pencils, or of the foci and axes of the pencils with respect to them. Thus, in many modes, which are explained, we may at pleasure by mere inspection observe the structural and relative positions of the bodies in question, and can so manage to evade the effects of ocular refractions, as to render the proportions above stated available for calculating the sizes of the bodies, and their distances from the cornea, iris, faces of the crystalline lens, retina, or from any one of themselves —or can even measure their depths in the eye, almost without calculation, and free from any that involve the consideration of the optical qualities of the organ.

Inflective phenomena are alike in the convergent and divergent rays, but refractive differ, and afford us a useful means of detecting the nature of an object. Inflective coloration is too subordinate to the ocular chromatic dispersion to deserve particular notice. Dr. T. Young explains how narrow straight objects, viewed through a puncture, are by the influence of ocular refractions made to appear curved, unless they are seen as diameters of the projected image of the pupillary opening. It is appended, that if they are made to encroach, in a like way, laterally upon a divergent pencil, they appear not, as in the case mentioned, concave, but convex towards the centre of the pupil's image.

Diagrams and other drawings accompany these and other parts of the essay. The foregoing principles are henceforth applied to the actual exploration contemplated, in order, as follows:—

SECTION II.—*Apparitions from Eyelashes, Eyelids and Conjunctival Fluids.*

These phenomena are treated with an effort at greater precision than in previously existing accounts of them. Little bars of fluid along the margins of the lids are shown to occasion the long beams of light, which issue from flames regarded "with winking eyes," by their annulling the refractions of the cornea. These beams have been ascribed to reflexion at the edges of the lids, but reflexion only yields a very pale beam which can be distinguished easily from the other.

SECTION III.—*Apparitions from Iris and Crystalline Lens: with Corollaries.*

The margin of the iris, opaque and transparent bodies, and the structural stellate figure, in the crystalline lens are placed *methodically* in the order in which they lie in the depths of the eye, and the especial manifestations which they severally yield, explained. The combined effects of ocular chromatic aberration, inflection at the edge of the iris, and the limbs of the stellate figure when we look at thin objects, or black and white lines, especially if curved, render some singular illusions, which are dissected.

The method by the two sorts of pencils may be applied to test the recent doctrine advanced by Stellweg, that the iris so lies on the face of the crystalline lens that there is no posterior chamber in the aqueous humour, and will probably be found to disprove it.

A calculation is entered into to show that unless Dr. T. Young—in estimating that the accommodation of the eye to focal distance by means of an alteration in the length of the optic diameter, would require a faculty of doing this to the extent of $\frac{1}{7}$th of the whole, taken when vision is suited to parallel rays,—exceeds the truth by *many times*; it must be easy to detect, by the parallax of the lenticular corpuscles in a couple of pencils whose foci rest near them, how and where the change is effected. And then an argument is drawn, that the accommodation is by change in the form of the lens, producing a minute movement of its anterior face, which it is thought may be detected by the said method.

The want of symmetry in ocular refractions is glanced at, and a nebulous scattering of light in the eye,—hereafter found to be the cause of a singular supplementary version of Purkinje's vascular phantom.

SECTION IV.—*Apparitions from the Vitreous Humour, applied to explain its Structure.*

It can be observed that, *in the posterior chamber of the eye there exists a lax, irregular, fibrous network, springing from the hyaloid membrane, but spanning the crystalline lens, without attachment to its capsule, occupying principally the peripheral portion of the cavity, but spreading as one structure into its interior, towards an ever-lessening number of leading fibres. The whole system is of less specific gravity than the vitreous fluid, either of itself, or by being the framework of membrane, in more or less of its extent. The fibres are constituted entirely of rows of beads, which are round, or nearly so, transparent, and of greater refractive power than the fluid, and joined by passing into one another by small portions of their surface.* The dynamical and optical considerations upon which these conclusions depend, are very carefully entered into, and the nicer points illustrated with appropriate drawings *.

SECTION V.—*Apparitions from, or from behind the Retina; with Corollaries.*

The next object for study behind those in the vitreous, are the vasa centralia retinæ, which are imbedded in the substance of the

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* The 36th vol. pp. 97–104, of the Lond. Med. Gazette, is quoted to show that the writer maintained in 1845 that the usual muscæ volitantes are but

membrane in such a manner as to penetrate from the side of the hyaloid membrane outwards from the eye's centre, deeper as they approach the punctum aureum. They may be called into view by any pencil of rays that is in the act of sweeping over the retina. In the well-known experiment of Purkinje, of waving a candle-flame before the eye, the radiating point is the image of the flame at the back of the eye. In this experiment the vessels display a remarkable parallactic gliding over the visual field, first observed by Gudden, in 1849; but it was left for H. Müller, a few years after, to point out the cause of the phenomenon, and to calculate from entoptical observations the distance of the vessels from the "perceiving membrane" lying without them, required to account for the parallax. This essay adopts his hypothesis, but supports it by independent observations, and substitutes another mode of calculating the said distance.

The flame is made to pause successively, on opposite sides of the vessel to be observed, in an ocular meridional plane, or that of some great circle, so that the shadow may be seen to deviate equally, twice in one plane, from the retinal radius that passes through it, and this whole angle, β, is noted, as well as that, α, between the two positions of the flame, as viewed from the eye's centre. Then, if d be the perpendicular distance of the vessel from a sentient surface whose radius is r, it is found that

$$d=r\left\{1-\frac{\cos\frac{1}{4}\,(\alpha+\beta}{\cos\frac{1}{4}(\alpha-\beta)}\right\}.$$

If we imagine a dark spot without the sentient surface, as the pigment of the choroid, visible through the retina at the foramen centrale, to simulate a shadow by being seen, through a second reflexion of the rays already radiating by reflexion at the flame's image on the eye's coats, by a sentient surface lying *within* it; then if d be the distance of the dark spot from such a surface, we shall have

$$d=r\left\{\frac{\cos\left(\frac{1}{4}\alpha-\frac{\beta}{2}\right)}{\cos\frac{1}{4}\alpha}-1\right\}.$$

If a vessel imbedded in the sentient surface were to have its shadow thrown back upon that surface, through reflexion from some tunic at the distance d without it, we should have

$$d=r\left\{1-\frac{\cos\frac{1}{4}\left(\alpha+\frac{\beta}{2}\right)}{\cos\frac{1}{4}\left(\alpha-\frac{\beta}{2}\right)}\right\}.$$

apparitions of portions of the essential structure of the vitreous body, and that he then fundamentally and clearly enunciated the view now more particularly developed. Other writers have regarded these as remnants of the fœtal eye, or as pathological fragments, floating freely, or in loculi of the vitreous humour; for the most part differing very widely from the reasonings and conclusions in this essay. The writings of Brewster, Donders, Doncan, &c., are referred to.

And some slight modification of this equation would meet the case of a parallactic deviation by a second reflexion, should a vessel lie either a little without or within the sentient surface.

The parallaxes here suggested would all take place in the direction of those actually witnessed in Purkinje's experiment, and the calculated values of d would so far approach each other from the same values of α and β (since β is small), that we ought not to rest satisfied with the fact of these values agreeing well with the simple conception of the sentient surface lying a little without the vascular plexus; especially as there are two supplementary versions of the vascular phantom rendered manifest by the experiment, one very notable one to which attention is directed—and that were the shadows of the vessels displayed by a second reflexion, as imagined in the third equation, there would be more than one version of them, as, moreover, a dark figure of the foramen centrale is rendered visible at the same time. However, these other versions of the figure are eliminated by being traced to other sources, and, with H. Müller, the central dark spot is treated as the shadow of the wall of the foramen centrale; so that the sentient surface must be without the brim of the foramen.

The essay, with as much of method as is available, now passes on in quest of the causes of other spectral phenomena, in the production of which light is not an agent. It cites Young's observation of the images of objects that press from the outside of the eye upon the sclerotic coat, being seen by *flexure* of the retina along their outline; notices, as of this type, the circles of light seen at the bases of the optic nerves on turning the eyes sharply in their sockets; touches upon the colours displayed in such experiments; and points out how that, wherever the retina is so compressed as to evince quasi-lights, it comparatively or entirely fails to render us acquainted with the existence of luminous objects. It then explains how the retina is flexed and compressed by the action of the orbital muscles, always to some extent when we fix the eyes' axes in a given direction, and severely whenever we wilfully strain our vision—thus astonishing us by the flitting away of objects from our sight, burying some in quasi-lucid clouds, as if they had overspread one another, and, as the origin of the phenomenon was undetected, occasioning many surmises upon the inherent qualities of the special nervous structure in order to account for them. An observation upon the inverted image of a candle formed at the posterior face of the crystalline lens is mentioned, which indicates other muscular action besides that which rotates the eyeball when the eye is vehemently strained, as if the lens became flattened. The phenomena which inform us of a differential structure in the retinal surface, with respect to the punctum cæcum, foramen centrale, and the elementary rods and cones which H. Müller believes to constitute the sentient layer, are adduced; as well as the conclusions to which we are led, after eliminating the various phenomena studied, as regards the ultimate structure of the sentient surface.

GEOLOGICAL SOCIETY.

[Continued from p. 486.]

April 14, 1858.—Prof. Phillips, President, in the Chair.

The following communication was read:—

"On a Fossil Fruit found in the Upper Wealden deposits in the Isle of Purbeck." By Prof. J. Phillips, M.A., Pres. G.S. &c.

In one of the Upper Wealden shales in the northern cliffs of Swanage Bay, the author found a small imperfect spherical pyritous seed-vessel, about half an inch in length, which presented evidence of having had eight meridional ridges, and an apparently fibrous surface. After a careful comparison of numerous forms of seeds of monocotyledonous, gymnospermous, and dicotyledonous plants, the author considered that the specimen presented some analogies with dicotyledons of several groups, especially *Euphorbiaceæ* and *Juglandaceæ*.

April 28.—Prof. Phillips, President, in the Chair.

The following communications were read:—

1. "On some Fossil Plants from Madeira." By C. J. F. Bunbury, Esq., F.R.S., F.G.S.

The vegetable remains described in this memoir were procured by Sir C. Lyell and M. Hartung from a leaf-bed discovered by them, in January 1854, in the ravine of S. Jorge in the Island of Madeira (Journ. Geol. Soc. vol. x. p. 326). They consist of detached leaves and fragments of leaves, mostly in a very imperfect state,—a large proportion of them, indeed, too imperfect to be even described. Dicotyledonous leaves predominate, but remains of Ferns are also numerous; always, however, in a very fragmentary condition. Monocotyledons are rare. No distinct remains of fruits have been found.

Mr. Bunbury described 23 different forms (8 Ferns, 1 Monocotyledon, and 14 Dicotyledons), which he has been able to discriminate among these remains; and added a list of those described by Professor Heer, in his memoir on the same subject. Each list contains several that are wanting in the other: M. Heer has 13 which are not found in the collection examined by Mr. Bunbury; and the latter collection contains 12 that do not seem to be noticed by the Swiss Professor. The total number of distinguishable forms in the two collections amounts to 37, the greater part of which must be considered as very imperfectly known. They gave occasion to the following remarks:—

1. The most abundant of the Dicotyledonous leaves and the most abundant of the Ferns are with great probability identified with species now existing in the island; namely, *Laurus Canariensis*, *Oreodaphne fœtens*, and *Pteris aquilina*. The *Pteris* is a plant which has a wide geographical range, and accommodates itself to a variety of conditions; but the two Laurels appear to be confined to the three Atlantic groups of islands, the Madeiras, Canaries, and Azores. Four other of the fossils appear to be very probably referable to

species now inhabiting Madeira; namely, *Vaccinium Maderense, Erica arborea, Woodwardia radicans*, and *Davallia Canariensis*. Of these, the *Vaccinium* is at present restricted to the island of Madeira; the others have a wider range. These facts may perhaps justify us in inferring (though certainly not with confidence) that the conditions of climate of the island in the time of the S. Jorge deposit were not very widely different from those now existing.

2. Certain other forms in the list (about four or five) are distinctly different from any now known, at least in Madeira or the neighbouring islands, and appear to belong to extinct species.

3. None of the fossils can be said to belong distinctly and positively to tropical families.

4. Those forms, among the fossils, which are different from the present vegetation of Madeira, do not show any marked analogy to the American or any other existing Flora.

5. The fossil leaves from S. Jorge show, on the whole, a decided analogy to the recent vegetation of the Madeira forests, inasmuch as there is among them a predominance of undivided and entire-edged leaves, with smooth (not wrinkled) and glossy surface.

6. The intermixture of abundant remains of Ferns with those of Dicotyledons is a characteristic of this leaf-bed, in which it shows a complete agreement with the existing state of things in the Madeira forests. Such an intermixture is not usual in the plant-bearing deposits of the palæozoic, secondary, or tertiary periods.

7. The very small number of Monocotyledons hitherto found is remarkable, but may be owing to accidental causes.

8. On the whole, the author is disposed to conclude that the vegetation of Madeira at the time when the S. Jorge leaf-bed was formed was, though not absolutely identical with that now existing, yet not very different from it. But all such conclusions must be received with great caution.

Mr. Bunbury moreover entered at some length into an inquiry as to the value of the evidence afforded by detached leaves of Dicotyledonous plants, and the degree of confidence due to conclusions founded upon such evidence, as to the affinities of extinct plants.

2. "On a Section of a part of the Fifeshire Coast." By the Rev. T. Brown. Communicated by Sir R. Murchison, V.P.G.S.

A section of the Lower Coal-measures, as exposed on the northern shore of the Firth of Forth, from Burntisland to Anstruther, constructed by the Rev. T. Brown, of Edinburgh, was exhibited to the Meeting. In his notes on the section, the author remarked that the limestone with which the section commences at Burntisland, the fossils of which have long been known, is the equivalent of the Burdiehouse beds. Passing eastward through the Mountain-limestone (500 feet thickness of sandstone, shale, and five or six beds of limestone), with trap-rocks, the section reaches the Upper Coal-measures (1700 feet thick). Crossing this coal-basin, the same Mountain-limestone beds reappear in reversed order, fold over an axis beyond Elie, and after minor bendings plunge under the basin of St. Monance, and rise again to the east. Between Elie and St. Monance the strata are singularly contorted; and an important fault runs nearly parallel with the coast, causing much confusion. Beyond St. Monance the section becomes clear, and, passing through the Mountain-limestone down to the level of the Burdiehouse beds, as at Burntisland, exhibits a magnificent series of the Lower Carboniferous rocks, consisting of freshwater or estuarine shales and sandstones, with a few bands of limestone, one of which latter is of marine origin. *Sphenopteris affinis* characterizes the upper portion of this inferior group; and *Cyclopteris*, its lower portion. The Burdiehouse limestone belongs to the upper portion; and, like the other calcareous bands, thins out eastwardly. The Grantham beds, on the contrary, appear to belong to the lower portion.

The fossils of the four limestone-bands in the upper part of the Mountain-limestone are *Zaphrentis, Orthis filaria, Productus punctatus, P. semireticulatus, Aviculopecten, Modiola, Loxonema rugifera, Orthoceras, Nautilus subsulcatus*. The fifth band and its accompanying bone-bed contains *Productus semireticulatus, Aviculopecten, Spirifer duplicostatus, Cochliodus*, and *Ctenacanthus*. The lowest band (at Ardross) has *Nucula tumida, N. attenuata, Schizodus sulcatus, Goniatites, Natica, Serpulites, Holoptychius Hibberti, Eurypterus?, Gampsonyx, Dithyrocaris*. The upper half of the Lower Carboniferous series contains *Sphenopteris affinis, Cypris?*, and *Palæoniscus*. The marine band, in the middle of the series, has *Encrinites, Fenestella, Nucula tumida, N. attenuata*, and *Productus semireticulatus*. The lower half of the series has *Cyclopteris, Stigmaria, Carpolithes, Cypris?, Pterichthys?*, and *Eurypterus*.

LXVIII. *Intelligence and Miscellaneous Articles.*

ON EUPHOTIDE AND SAUSSURITE. BY T. STERRY HUNT, OF THE GEOLOGICAL COMMISSION OF CANADA.

THROUGH the kindness of Prof. Arnold Guyot I have had an opportunity of examining a collection of the euphotides of Mt. Rose, and of satisfying myself as to the nature of the true *Saussurite* (the *jade* of De Saussure), which is a white mineral forming with grass-green smaragdite, the *Euphotide jadien* of Brongniart. It appears to be a compact epidote or zoisite, having, as De Saussure long since determined, the hardness of quartz, and a density of 3·3—3·4. My own analysis of a fragment from Mt. Rose, with a density of 3·36, gives the composition of a lime-alumina epidote, with a little soda. The analyses of Boulanger of the Saussurites of Mt. Genèvre and Orezza, lead to the same conclusion.

Saussurite is then nearly related to the massive white garnet from the Green Mountains in Canada, which, mixed with serpentine and with amphibole, gives rise to varieties of rocks which I have described, in my report of the Geological Survey of Canada for 1856, as resembling certain euphotides. The smaragdite of Mt. Rose is a vanadiferous diallage, approaching in composition the variety from near Genoa, analysed by Schafhaütl. I may here remark that I have lately detected vanadium, with chromic iron, and oxide of nickel in a serpentine from Gaspé.

The true euphotide is then distinct from those diallagic dolerites with which it has been confounded by Delesse, Coquand, and most modern lithologists. I propose soon to send you my results in a detailed form with a sketch of the history of euphotide.—*From* Silliman's *Journal of Science for* May 1858.

ANALYSIS OF THE PEROWSKITE OF SCHELINGEN. BY F. SENECA.

This mineral was first discovered by Butzengeiger in the granular limestone of Vogtsburg on the Kaiserstuhl, and described by Walchner in 1825. The latter regarded it as titanic acid, with a very small amount of iron, and considered it to be a peculiar species. The same crystals were subsequently described by Quenstedt and Volger as Perowskite. These crystals have been investigated by Seneca in Weltzien's laboratory.

The crystals are cubes. Their cleavage is very distinctly parallel to the faces of the cube. The individual crystals are always aggregates of smaller cubes, between which delicate films of calcareous spar are sometimes inserted, as may be easily ascertained by treatment with acids. Spec. grav.=4·02. Hardness between apatite and felspar. The mineral possesses an iron-black colour and a brilliant, metallic, glassy lustre; iridescence is only exhibited during weathering, as in other minerals containing protoxide of iron, the latter becoming converted into hydrated peroxide. Analysis gave—

	I.			Oxygen.
TiO^2	58·95	per cent.		23·5
CaO	35·69	,,	10·19	
FeO	6·23	,,	1·38	

	II.			Oxygen.
TiO^2	59·30	per cent.		23·61
CaO	35·94	,,	10·28	
FeO	5·99	,,	1·33	

The amount of oxygen of the base is to that of the acid as 1 : 2, from which results the formula CaO, TiO^2, in which the lime is partly replaced by protoxide of iron. According to the formula, Perowskite contains 41·09 per cent. of lime and 58·91 per cent. of titanic acid.—Liebig's *Annalen*, civ. p. 371.

INDEX TO VOL. XV.

ACETOSALICYLE, on the production and constitution of, 300.
Alcohols, on a new class of, 226.
Allyle, on new derivatives from, 226.
Aluminium, on the preparation of the iodide of, 117.
Ammonia, on the evolution of, from volcanos, 233.
Andrews (Prof.) on the density of ozone, 146.
Anisic acid, action of sulphuric acid upon, 143.
Anorthite, analysis of, 518.
Antimony-bases, on the, 147.
Arsenic-bases, on the, 147.
Atkinson (Dr.), chemical notices from foreign journals by, 109, 299, 453.
Aurora, on fluorescence produced by the, 326.
Baeyr (M.) on the coefficient of absorption of chloride of methyle, 453.
Barometer, on the daily fall of the, at Toronto, 327.
Benzole, on new substitution-products of, 455.
Berthelot (M.) on the synthesis of wood-spirit, 453; on the preparation of certain alcohols, 455.
Bigsby (Dr. J. J.) on the palæozoic strata of the State of New York, 76, 234.
Blowpipe, on the assaying of coals by the, 433.
Bode, on the law of, 206.
Bodies, on the percussion of, 161, 263, 349.
Bois-Reymond (M. du) on the electrical Silure, 45.
Books, new:—Williams' Chemical Manipulation, 66; Galloway's Qualitative Analysis, 140; Childe's Theory of Reflected Ray-surfaces, 390.
Boron, on some properties of, 110.
Bromine, on the separation of, 144; on the action of the electric current on aqueous solutions of, 328.
Brown-Séquard (Dr. E.) on the influence of efforts of inspiration on the movements of the heart, 543; on the influence of oxygen on the vital properties of the spinal cord, nerves and muscles, 544.
Brown (Rev. T.) on a section of a part of the Fifeshire coast, 552.
Brücke (Prof. E.) on gravitation and the conservation of force, 81.
Brüning (M.) on the action of potash on iodoform, 302.
Brunner (M.) on the preparation of metallic manganese, 112.
Buff (Prof.) on new silicon compounds, 456.
Bunbury (C. J. F.) on a remarkable specimen of *Neuropteris*, 318; on some fossil plants, 551.
Bunsen (Prof.) on the optical and chemical extinction of chemical rays, 230; on the chemical theory of gunpowder, 489.
Cahours (A.) on a new class of alcohols, 226; on acetosalicyle, 300; on the phosphorus-bases, 393.
Callan (Rev. N. J.) on a contact-breaker superior to any hitherto made, and on certain effects of a condenser on the action of various kinds of contact-breakers, 255.
Capillarity and its relation to latent heat, observations on, 1.
Carbonic acid gas, on the elasticity of, 303.
Carmichael (Rev. R.) on the singular solutions of differential equations, 522.
Caron (M.) on alloys of silicon, 114.
Carrington (R. C.) on the evidence which the observed motions of the solar spots offer for the existence of an atmosphere surrounding the sun, 444.
Cayley (A.) on a class of dynamical problems, 306; on the cubic transformation of an elliptic function, 363.

Cephalaspis, observations on the genus, 319.
Chapman (Prof. E. J.) on the assaying of coals by the blowpipe, 433.
Chemical notices from foreign journals, 109, 299, 453.
Chlorine, on the separation of, 144; on the action of the electric current on aqueous solutions of, 328.
Chromium, on the preparation of, 112.
Claudet (A.) on some stereoscopic phænomena, 397.
Clausius (Prof. R.) on the conduction of electricity in electrolytes, 94.
Clouds, on the forms of, 241.
Coal-gas, on the influence of musical sounds on the flame of a jet of, 235.
Coals, on the occurrence of, in Southern Chili, 321; on the assaying of, by the blowpipe, 433.
Cobalt, on the atomic weight of, 115.
Cockle (J.) on certain researches of Euler, 389.
Contact-breaker, description of a, 255.
Cooper (E. J.) on the perihelia and nodes of the planets, 316.
Copper, on the electric conductivity of commercial, 472.
Coumaric acid, observations on, 299.
Couper (M.) on bromobenzole and dibromobenzole, 455.
Crystalline forms, on the relations existing between certain groups of, belonging to different systems, 157.
Crystallographic notices, 512.
Crystals, on the microscopical structure of, 152; on an improved method of finding the position of any face in, belonging to the anorthic system, 512.
Damascus, on the lakes and rivers of, 260.
Daubeny (Dr.) on the evolution of ammonia from volcanos, 233.
Debus (Dr. H.) on the action of nitric acid on glycerine, 196.
Dellmann (F.) on atmospheric electricity, 460.
Despretz (M. C.) on the decomposition of lead-salts by the action of the voltaic current, 78.
Deville (M.) on the affinity of titanium for nitrogen, 109; on some properties of boron, 110; on the preparation of the metals, 113; on alloys of silicon, 114; on a new method of taking vapour-densities, 458.
Diorite, on the constitution of, 518.
Draper (Prof. J. W.) on the nature of flame, and on the conditions of the sun's surface, 90.
Dynamical problems, on a class of, 306.
Earth's surface, on the diurnal variations of the magnetic force at the, 192.
Eclipse, solar, on the intensity of light during the recent, 305.
Electric current, on the action of the, on aqueous solutions of chlorine, bromine, and iodine, 328.
Electrical excitation, on a new source of, 239.
—— theory of the thunder-cloud, on the, 252.
Electricity, on the conduction of, in electrolytes, 94; on the difference in the amount of, developed by equal surfaces of cylinder and plate electrical machines, 290; on atmospheric, 460; on the rotation of metallic tubes and spheres by, 519.
Electrolytes, on the conduction of electricity in, 94.
Electro-magnet, on the rotation of the electrical light round the pole of an, 463.
Epidotes, on the composition of, 159.
Equations, on the singular solutions of differential, 522.
Erdmann (Prof. O. L.) on the formation of isatine by ozone, 80.
Espy (J. P.) on the law of storms, 345.
Euphotide, on the constitution of, 553.
Euler, on certain researches of, 389.
Fairbairn (W.) on the strength of alloys of nickel and iron, 486.
Faröelite, on the crystalline form of, 28.
Favre (P. A.) on the mechanical equivalent of heat, 406.
Field (F.) on the separation of iodine, bromine, and chlorine, 144.
Flame, on the nature of, and on the condition of the sun's surface, 90.
Flames, sonorous, experiments on, 235, 261, 404.
Fluids in motion, on the thermal effects of, 477.
Fluorescence produced by the aurora, on, 326.

Foote (Mrs. E.) on a new source of electrical excitation, 239.
Force, on gravitation and the conservation of, 81; on the measure and transfer of, 329.
Frankland (Prof.) on a new series of compounds derived from ammonia and its analogues, 149.
Fremy (M.) on the preparation of metallic chromium and manganese, 112.
Fresenius (Dr. R.) on the occurrence of formic and propionic acids in mineral waters, 114.
Function, elliptic, on the cubic transformation of an, 363.
Gages (A.) on pseudomorphic tremolite, 180.
Galvanometer, on an improved, 432.
Garnets, on the composition of, 160.
Gassiot (J. P.), on a Ruhmkorff's induction apparatus, 466.
Gaudin (M.) on the production of artificial sapphires, 109.
Gemmellaro (G. G.) on the gradual elevation of a part of the coast of Sicily, 325.
Geological Society, proceedings of the, 72, 152, 233, 318, 400, 483, 551.
Glaciers, on the structure and motion of, 365.
Glyceric acid and salts, 198.
Glycerine, on the action of nitric acid on, 196.
Glycol, on the action of pentachloride of phosphorus upon, 454.
Godwin-Austen (R.) on extraneous rock-fragments found in the chalk, 155.
Gold-fields of Victoria, on the geology of the, 400, 483.
Goniometer, on a substitute for the reflective, 517.
Gore (G.) on the rotation of metallic tubes and spheres by electricity, 519.
Granites of Ireland, observations on the, 233.
Graphularia Wetherelli, on the occurrence of, 484.
Gravitation and the conservation of force, on, 81, 329.
Gunpowder, chemical theory of, 489.
Guthrie (F.) on a new form of spirator, 64.
Hardie (W.) on the telestereoscope, 156.
Haughton (Rev. Prof. S.) on the granites of Ireland, 234.
Hearder (J. N.) on the difference in the amount of electricity developed by equal surfaces of cylinder and plate electrical machines, 290.
Heart, on the cause of the rhythmic motion of the, 70; influence of inspiration on the, 543.
Heat, on capillarity and its relation to latent, 1; on the mechanical equivalent of, 406.
Heddle (Dr.) on the crystalline form of Faröelite, 28; on some new forms of British sphenes, 134.
Helmholtz (Prof. H.) on the telestereoscope, 19.
Hermann (R.) on the composition of epidotes, vesuvians and garnets, 159.
Hofmann (Prof.) on triethylamine, 70; on thialdine, 140; on the action of sulphuric acid on anisic and salicylic acids, 143; on the phosphorus-, arsenic- and antimony-bases, 147, 393; on a new class of alcohols, 226.
Hopkins (T.) on the daily fall of the barometer at Toronto, 327.
Hopkins (W.) on the conductive powers of various substances, 310.
Hull (E.) on the triassic and permian rocks of the Odenwald, 72.
Hunt (T. S.) on the part which the alkaline silicates play in the metamorphism of rocks, 68; on euphotide and Saussurite, 553.
Huxley (Prof.) on *Cephalaspis* and *Pteraspis*, 319; on a new species of *Plesiosaurus*, 320; on the structure and motion of glaciers, 365.
Indican, on the preparation and properties of, 29; action of acids on, 35; action of alkalies on, 183.
Indifulvine, on the preparation and properties of, 39, 117.
Indigo-blue, on the formation of, 29, 117, 183.
Indirubine, on the preparation and properties of, 41.
Induction apparatus, description of an, 466.
Iodine, on the separation of, 144; on the action of the electric current on aqueous solutions of, 328.

Iodoform, on the action of potash on, 302.

Iron, on the action of, on waters, 48; on the coercive power of pure, 80; on the effects of magnetization on the electric conductivity of, 469; on the strength of alloys of, with nickel, 486.

Jago (J.) on ocular spectres, 545.

Jennings (F. M.) on the lakes and rivers of Damascus, 260.

Jevons (W. S.) on the forms of clouds, 241.

Joule (J. P.) on the intensity of light during the recent solar eclipse, 305; on an improved galvanometer, 432; on the thermal effects of fluids in motion, 477; on the thermal effects of longitudinal compression of solids, 538.

Jupiter, on the satellite system of, 206.

Kraut (M.) on the occurrence of butyric and caproic acids in mineral waters, 114.

Lead-salts, on the decomposition of, by the voltaic current, 78.

Leckenby (J.) on the Kelloway's rock of the Yorkshire coast, 484.

Leconte (Dr. J.) on the influence of musical sounds on the flame of a jet of coal-gas, 235.

Leucine, on a new source of, 125.

Light, on the intensity of, during the recent solar eclipse, 305.

——, electrical, rotation of the, round the pole of an electro-magnet, 463.

Lloyd (Rev. H.) on the direct magnetic influence of a distant luminary upon the diurnal variations of the magnetic force at the earth's surface, 192.

Magnetic forces, on the diurnal variations of the, 192.

Magnetism, on the optical properties developed in transparent bodies by the action of, 409.

Manchester Society, proceedings of the, 486.

Manganese, on the preparation of metallic, 112.

Marignac (C.) on the relations existing between certain groups of crystalline forms belonging to different systems, 157.

Marmot, on the torpidity of the, 488.

Matthiessen (Dr. A.) on the coercive power of pure iron, 80.

Medlock (H.) on the reciprocal action of metals and the constituents of well- and river-waters, 48.

Metals, on the reciprocal action of, and the constituents of well- and river-waters, 48; on the preparation of the, 113; on the electrodynamic qualities of, 469.

Miascite, on the nature of, 180.

Miller (Prof. W. H.), crystallographic notices by, 512.

Molybdenum, on the nitride of, 88.

Moore (J. C.) on a protrusion of Silurian rock in the north of Ayrshire, 483.

Murchison (Sir R. I.) on the succession of rocks in the Northern Highlands, 322.

Neuropteris, on a remarkable specimen of, 318.

Nickel, on the atomic weight of, 115; on the effects of magnetization on the electric conductivity of, 469; on the strength of alloys of, with iron, 486.

Nitroform, on the preparation and properties of, 302.

Nitrogen, on the affinity of titanium for, 109; on the phosphide of, 113.

Ocular spectres, observations on, 545.

Ormerod (G. W.) on the rock-basins in the granite of Dartmoor, 484.

Owen (Prof.) on *Zygomaturus trilobus*, 403.

Oxindicanine, on the preparation and composition of, 188.

Oxygen, on the various conditions of, 24; on the influence of, on the vital properties of the spinal cord, nerves and muscles, 544.

Ozone, on the formation of isatine by, 80; on the density of, 146.

Paget (J.) on the rhythmic motion of the heart, 70.

Pauli (M.) on phosphuret of nitrogen, 113.

Perowskite, analysis of, 554.

Phillips (J.) on the gold-fields of Victoria, 401.

Phillips (Prof. J.) on estuary strata in Shotover Hill, 75; on a fossil fruit found in the Isle of Purbeck, 551.

Phosphates, on the mode in which the, pass into plants, 405.
Phosphorus-bases, on the, 147, 393.
Photometrical researches, 230.
Planets, on the perihelia and nodes of the, 316.
Plants, on the mode in which the phosphates pass into, 405.
Plesiosaurus, on a new species of, 320.
Poinsot (M.) on the percussion of bodies, 161, 263, 349.
Prestwich (J.) on the boring through the chalk at Harwich, 154.
Propylic alcohol, on the preparation of, 455.
Pteraspis, observations on the genus, 319.
Quartz, on the lines observed on the faces of crystals of, 516.
Rankine (W. J. M.) on the elasticity of carbonic acid gas, 303.
Rays, chemical, on the optical and chemical extinction of, 230.
Redaway (W.) on the gold-diggings at Ballaarat, 402.
Riche (M. A.) on the action of the electric current on aqueous solutions of chlorine, bromine, and iodine, 328.
Ritchie's (Mr.) modification of Ruhmkorff's induction apparatus, observations on, 466.
Rive (M. De la) on the rotation of the electrical light round the pole of an electro-magnet, 463.
Robinson (T. R.) on fluorescence produced by the aurora, 326.
Rocks, on the part which the alkaline silicates play in the metamorphism of, 68; on the origin of, 152.
Rogers (Prof. W. B.) on some sonorous flames, 261, 404.
Rosales (H.) on the gold-diggings at Ballaarat, 483.
Roscoe (Dr. H. E.) on the optical and chemical extinction of chemical rays, 230.
Royal Society, proceedings of the, 68, 140, 226, 306, 393, 469, 538.
Salicylic acid, action of sulphuric acid upon, 143.
Sapphires, on the production of artificial, 109.
Saussurite, analysis of, 553.
Schischkoff (L.) on nitroform, 302; on the chemical theory of gunpowder, 489.
Schneider (R.) on the atomic weights of cobalt and nickel, 115.
Schönbein (Dr.) on the various conditions of oxygen, 24.
Schüler (M.) on acetosalicyle, 301.
Schunck (Dr. E.) on the formation of indigo-blue, 29, 117, 183.
Scott (R. H.) on the composition of anorthite from the Oural Mountains, 518.
Selwyn (A. R. C.) on the geology of the gold-fields of Victoria, 400.
Seneca (F.) on Perowskite, 554.
Silicon, on a new mode of preparing, 114; on some new compounds of, 456.
Silure, on the electrical, 45.
Smyth (R. B.) on the extinct volcanos of Victoria, 74.
Solar spots, on the motions of the, 444.
Solids, on the thermal effects of longitudinal compression of, 538.
Sorby (H. C.) on the microscopical structure of crystals, 152.
Sphenes, British, on some new forms of, 134.
Spirator, on a new form of, 64.
Stars, variable, on the, 359.
Stereoscopic phænomena, on some, 397.
Stoney (G. J.) on the adjustments of the needle of a tangent-galvanometer, 135, 327.
Storms, on the law of, 345.
Sun, on the existence of an atmosphere surrounding the, 444; on the conditions of the surface of the, 90.
Tait (P. G.) on the density of ozone, 146.
Tangent-galvanometer, on the adjustments of the needle of a, 135, 327.
Tebay (S.) on the law of Bode, and on the rotation of a heavenly body, 206.
Telestereoscope, on the, 19, 156.
Temperature, terrestrial, observations on, 310.
Thenard (M. P.) on the mode in which the phosphates pass into plants, 405.
Thermometers, on a difference in the march of air- and mercury-, 212.
Thialdine, researches on, 140.
Thomson (Prof. W.) on the effects of

magnetization on the electric conductivity of nickel and of iron, 469; on the electric conductivity of commercial copper of various kinds, 472; on the thermal effects of fluids in motion, 477; on the alterations of temperature accompanying changes of pressure in fluids, 541.
Titanium, on the affinity of, for nitrogen, 109.
Tremolite, pseudomorphic, observations on, 180.
Triethylamine, on the formation of, 70.
Trinitroacetonitrile, on the action of water on, 302.
Troost (M.) on a new method of taking vapour-densities, 458.
Tungsten, on the nitruret of, 488.
Tyndall (Prof. J.) on the structure and motion of glaciers, 365.
Valentin (G.) on the torpidity of the marmot, 488.
Vapour, on the tension of aqueous, 212.
Vapour-densities, on a new method of taking, 458.
Vaughan (D.) on the solar spots and the variable stars, 359.
Verdet (M.) on the optical properties developed in transparent bodies by the action of magnetism, 409.
Vesuvians, on the composition of, 160.
Volcanos, on the evolution of ammonia from, 233.
Voltaic current, on the decomposition of lead-salts by the, 78.
Water, action of metals on, 48.
Waters, mineral, on the occurrence of organic acids in, 114.
Waterston (J. J.) on capillarity, and its relation to latent heat, 1; on the evidence of a graduated difference between the thermometers of air and mercury below 100° C., 212; on the integral of gravitation and its consequents with reference to the measure and transfer, or communication of force, 329.
Weber (M.) on the preparation of the iodide of aluminium, 117.
Wetherell (N. T.) on the occurrence of *Graphularia Wetherelli*, 484.
Wöhler (Prof.) on the affinity of titanium for nitrogen, 109; on some properties of boron, 110; on new silicon compounds, 456; on the presence of oxide of silicon in cast iron, 458; on the nitrurets of tungsten and molybdenum, 488.
Wood (S. V.) on the extraneous fossils of the red crag, 485.
Wood-spirit, on the synthesis of, 453.
Wurtz (M.) on chloride of æthylene, 454.
Zervas (L.) on the action of sulphuric acid upon anisic and salicylic acids, 143.
Zinc, on new compounds derived from ammonia and, 149.
Zygomaturus trilobus, observations on, 403.

END OF THE FIFTEENTH VOLUME.

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Fig. 1.

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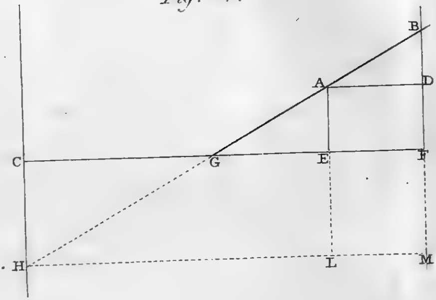

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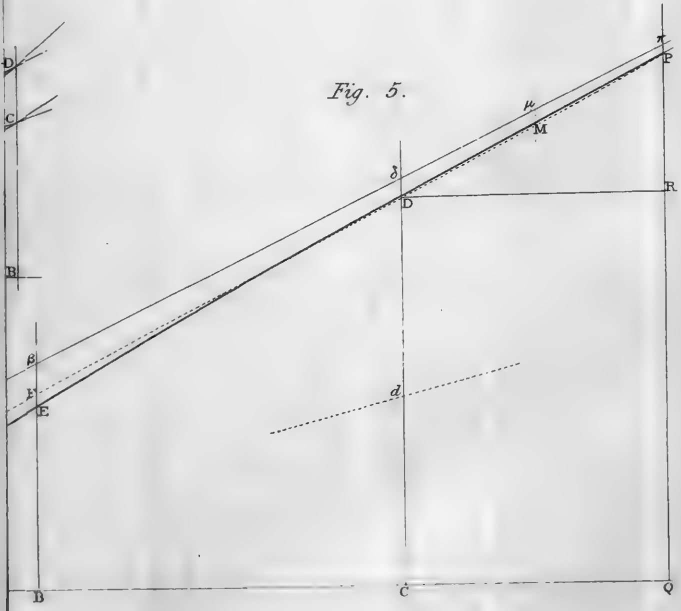

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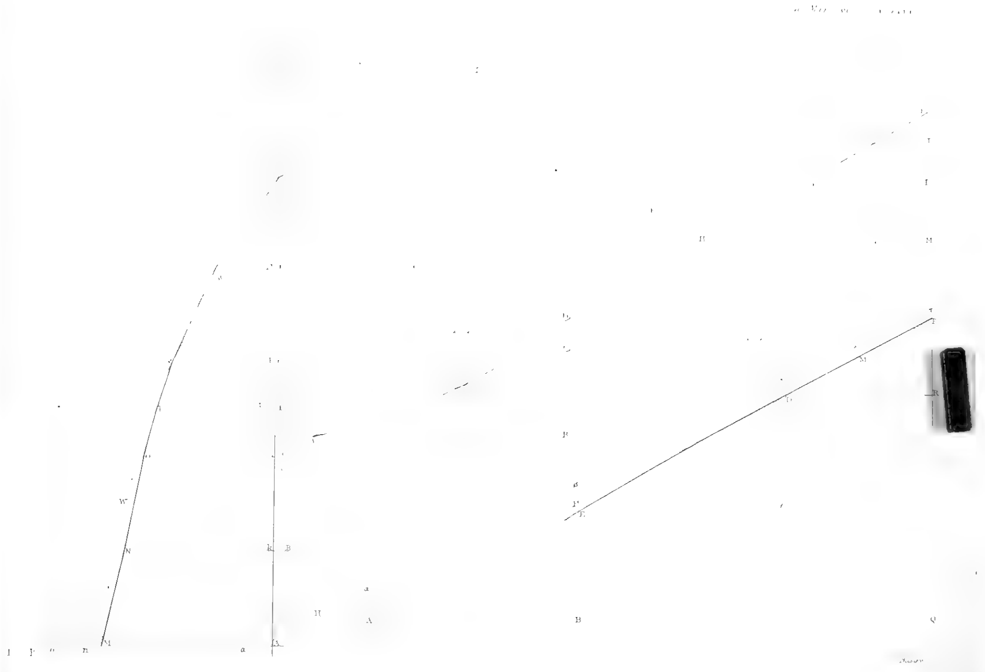W
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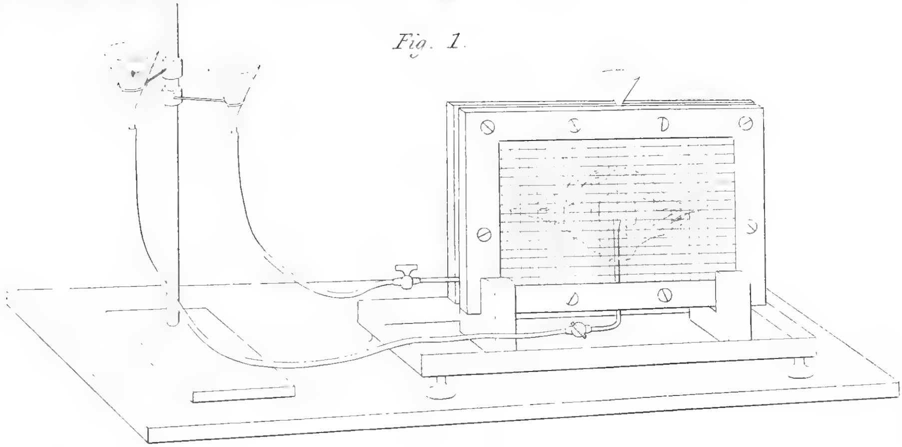

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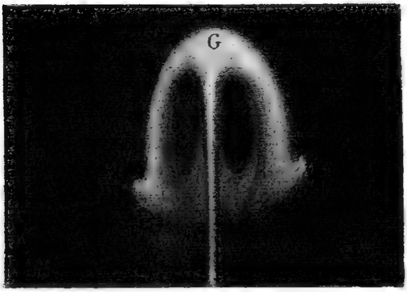

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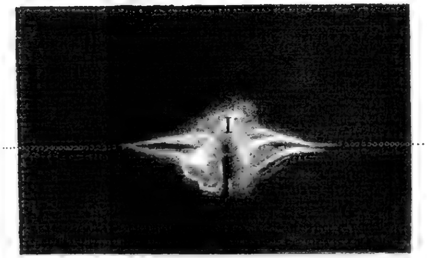

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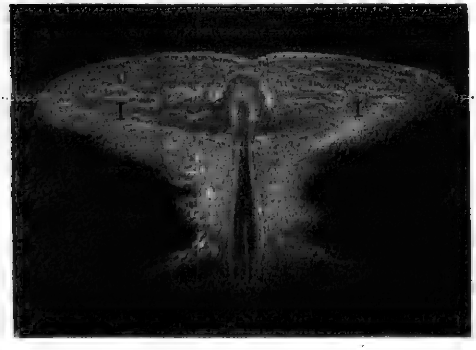

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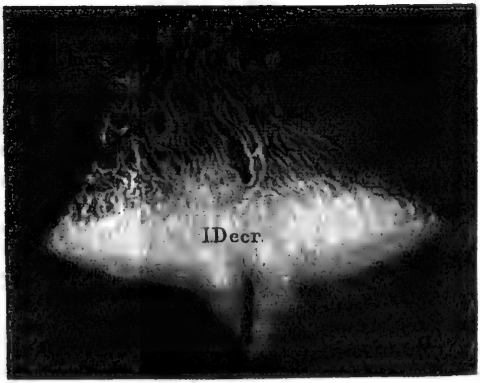


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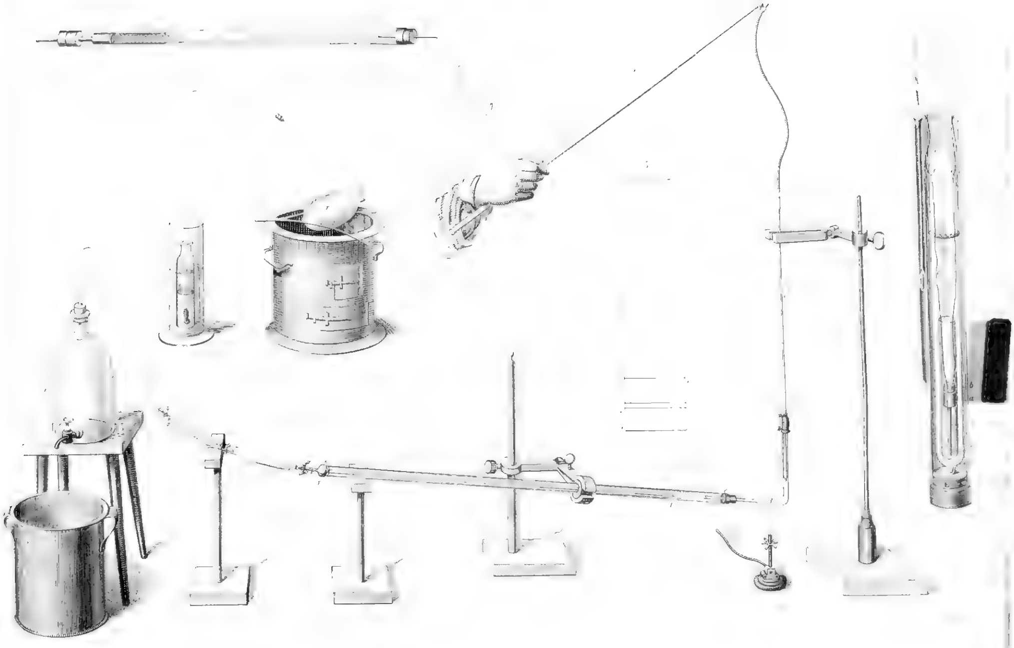
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