“Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . Admiratio generat questionem, quæstio investigationem, investigatio inventionem.”—Hugo de S. Victore.

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

J. B. Pinelli ad Mazonium.
**CONTENTS OF VOL. XVI.**

**(FOURTH SERIES.)**

**NUMBER CIV.—JULY 1858.**

<table>
<thead>
<tr>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>The Astronomer Royal on the Mechanical Conditions of the Deposit of a Submarine Cable</td>
</tr>
<tr>
<td>Mr. W. R. Grove on the Striae seen in the Electrical Discharge in vacuo</td>
</tr>
<tr>
<td>Mr. W. Gravatt on the Atlantic Cable</td>
</tr>
<tr>
<td>Dr. W. Odling on the Doctrine of Equivalents</td>
</tr>
<tr>
<td>Prof. Clausius on the Nature of Ozone</td>
</tr>
<tr>
<td>Proceedings of the Royal Society:</td>
</tr>
<tr>
<td>Major-General Edward Sabine on Hourly Observations of the Magnetic Declination</td>
</tr>
<tr>
<td>Dr. J. P. Joule on the Expansion of Wood by Heat</td>
</tr>
<tr>
<td>The Rev. T. P. Kirkman on the Partitions of the r-Pyramid</td>
</tr>
<tr>
<td>Dr. W. B. Herapath on the Cinchona Alkaloids</td>
</tr>
<tr>
<td>Dr. J. H. Gladstone on the Chemical Action of Water on Soluble Salts</td>
</tr>
<tr>
<td>Proceedings of the Cambridge Philosophical Society:</td>
</tr>
<tr>
<td>The Rev. J. Power on the Theory of Polarized Fasciculi, commonly known as Haidinger’s Brushes</td>
</tr>
<tr>
<td>Prof. Stokes on the Dicontinuity of Arbitrary Constants</td>
</tr>
<tr>
<td>Proceedings of the Geological Society:</td>
</tr>
<tr>
<td>Dr. J. W. Dawson on the Lower Carboniferous Coal-measures of British America</td>
</tr>
<tr>
<td>Mr. E. W. Binney on the Structure of <em>Stigmaria ficoides</em></td>
</tr>
<tr>
<td>Mr. J. Morris on a New Fossil Fern from Worcestershire</td>
</tr>
<tr>
<td>Prof. A. C. Ramsay on some of the Glacial Phænomena of Canada</td>
</tr>
<tr>
<td>Mr. G. P. Scrope on Lamination and Cleavage</td>
</tr>
<tr>
<td>Microscopic Observations on the Electric Spark, by R. Fabibri</td>
</tr>
<tr>
<td>On the origin of Felspars, and on some points of Chemical Lithology, by T. Sterry Hunt, Esq</td>
</tr>
</tbody>
</table>

**NUMBER CV.—AUGUST.**

<table>
<thead>
<tr>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mr. J. A. Broun on certain Results of Magnetical Observations</td>
</tr>
<tr>
<td>Mr. D. Vaughan on the Sun’s Spots</td>
</tr>
</tbody>
</table>
M. W. G. Hankel on the Development of Electricity between Metals and Heated Salts ........................................ 101
Mr. A. S. Couper on a New Chemical Theory .................. 104
Prof. Sylvester on the Algebraical Theory of Derivative Points of Curves of the Third Degree .................................. 116
Prof. Plücker on the Action of the Magnet upon the Electrical Discharge in Rarefied Gases. (With a Plate.) ............... 119
Dr. Atkinson's Chemical Notices from Foreign Journals ....... 135
Proceedings of the Royal Society:—
  Prof. Kölliker on the Poison of the *Upas Antiar* ........... 140
  Major-General Sabine on the Magnetic Observations transmitted from York Fort in Hudson's Bay, in August 1857. 143
Proceedings of the Cambridge Philosophical Society:—
  Mr. Candy on a Physiological Alphabet ........................ 150
  Prof. Sedgwick on a series of dislocations which have moved the Cambrian and Silurian Rocks between Leven Sands and Duddon Sands .................................................. 155
  Prof. De Morgan on the Beats of Imperfect Consonances. 158
On the Fluorescence of a Solution of Fraxine which appears under certain circumstances of a yellow colour, by the Prince Salm-Horstmar .......................................................... 159
Note on Internal Conical Refraction, by Dr. P. Lech. 159
On the Influence of Pressure on Chemical Affinity, by Dr. Lothar Meyer ....................................................... 160

**NUMBER CVI.—SEPTEMBER.**

Dr. Hirst on Equally Attracting Bodies .......................... 161
Prof. Schönbein on the Allotropic Modifications of Oxygen, and on the Compound Nature of Chlorine, Bromine, &c. ........... 178
Prof. J. Le Conte's Preliminary Researches on the alleged Influence of Solar Light on the Process of Combustion .......... 182
Mr. T. H. Henry on the Separation of Nickel and Cobalt from Manganese ......................................................... 197
Mr. C. W. Merrifield on the Geometry of the Elliptic Equation. 198
Dr. Atkinson's Chemical Notices from Foreign Journals ....... 209
Proceedings of the Royal Society:—
  Dr. Matthiessen on the Electric-conducting Power of the Metals, and on the Thermo-electric Series .................... 219
  Mr. A. Cayley on the Theory of Matrices ..................... 223
  Prof. Haughton on the Physical Structure of the Old Red Sandstone of the County of Waterford .......................... 224
  Mr. L. Horner on the Geological History of the Alluvial Land of Egypt .......................................................... 225
Proceedings of the Cambridge Philosophical Society:—
  Prof. Thompson on the *Sophista* of Plato ..................... 229
  Prof. Miller on a simple proof of the principle of Amsler's Planimeter ......................................................... 230
<table>
<thead>
<tr>
<th>CONTENTS OF VOL. XVI.—FOURTH SERIES.</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>The Astronomer Royal on the treatment of some Geometrical Problems</td>
<td>231</td>
</tr>
<tr>
<td>Prof. De Morgan on a Proof of the existence of a Root in every Algebraic Equation</td>
<td>232</td>
</tr>
<tr>
<td>Proceedings of the Geological Society:</td>
<td></td>
</tr>
<tr>
<td>Mr. J. Prestwich on the Pleistocene Sea-bed of the Sussex Coast</td>
<td>233</td>
</tr>
<tr>
<td>Dr. J. J. Bigsby on the Palæozoic Fossils of the State of New York</td>
<td>234</td>
</tr>
<tr>
<td>Prof. Harkness on Jointings, and on the Dolomites near Cork</td>
<td>235</td>
</tr>
<tr>
<td>Mr. W. Hawkes on an Experiment in melting and cooling some of the Rowley Rag</td>
<td>236</td>
</tr>
<tr>
<td>Mr. W. Smyth on the Iron Ores of Exmoor</td>
<td>236</td>
</tr>
<tr>
<td>Captain W. Vivian on Native Copper in the Llandudno Mine, North Wales</td>
<td>237</td>
</tr>
<tr>
<td>Prof. J. Nicol on the Slate-rocks and Trap-veins of Easdale and Oban</td>
<td>238</td>
</tr>
<tr>
<td>On the Resistance of Ships, by Prof. W. J. Macquorn Rankine</td>
<td>238</td>
</tr>
<tr>
<td>Observations on the Native Phosphates of Copper, by Prof. Bergemann</td>
<td>239</td>
</tr>
<tr>
<td>On the Euphotides of Mt. Rose, by T. Sterry Hunt, Esq.</td>
<td>240</td>
</tr>
</tbody>
</table>

---

NUMBER CVII.—OCTOBER.

| Prof. Hennessy on the Distribution of Heat over Islands, and especially over the British Isles | 241 |
| Dr. Simpson on a New Base obtained by the Action of Ammonia on the Terhromide of Allyle | 257 |
| Mr. A. B. Northcote on the Constitution of Thermophyllite | 263 |
| Dr. Hirst on Equally Attracting Bodies (concluded) | 266 |
| MM. Deville and Leblanc on the Gaseous Emanations which accompany the Boracic Acid in the Lagoons of Tuscany | 284 |
| Prof. Miller on some Crystallized Furnace Products | 292 |
| Dr. Atkinson’s Chemical Notices from Foreign Journals | 295 |
| Proceedings of the Royal Society: | |
| Mr. J. Jago on the Functions of the Tympanum | 298 |
| Prof. W. Thomson on the Interior Melting of Ice | 303 |
| Captain Moorsom on the Practical Use of the Aneroid Barometer as an Orometer | 304 |
| Mr. J. P. Gassiot on the Stratifications and Dark Bands in Electrical Discharges as observed in Torricellian Vacuums | 305 |
| Dr. Hofmann on the Poly-ammonias | 309 |
| Proceedings of the Cambridge Philosophical Society: | |
| Prof. De Morgan on the Syllogism, No. III., and on Logic in general | 313 |
Dr. Donaldson on the Statue of Solon mentioned by
Æschines and Demosthenes ................................ 317
Dr. Paget on some Instances of remarkable Defects in
the Voluntary Muscles .................................... 317
Mr. H. F. Baxter on Organic Polarity .................. 318
On the Double Tangents to Plane Curves, by the Rev. George
Salmon, Trinity College, Dublin .......................... 318
On the Electro-magnetic Rotation of Liquids, by A. Bertin .. 319

NUMBER CVIII.—NOVEMBER.

Mr. J. Bridge on the Diffraction of Light .................. 221
Mr. R. Cartmell on a Photochemical Method of recognizing
the Non-volatile Alkalies and Alkaline Earths ............ 328
Prof. Tyndall on some Physical Properties of Ice .......... 333
Mr. A. Cayley on a Theorem relating to Hypergeometric Series 356
Dr. Wallace on Chloro-arsenious Acid, and some of its Com-
pounds ...................................................... 358
Dr. Atkinson's Chemical Notices from Foreign Journals .... 361
Prof. Sylvester on the Equation in Numbers of the First Degree
between any number of Variables with Positive Coefficients. 369
Prof. Sylvester on the Problem of the Virgins, and the general
Theory of Compound Partition ................................ 371
Proceedings of the Royal Society:—
Mr. T. S. Hunt on the probable Origin of some Magnes-
ian Rocks .................................................... 376
Mr. C. G. Williams on the Constitution of the Essential
Oil of Rue .................................................... 380
Messrs. F. C. Calvert and R. Johnson on the Relative
Power of Metals and their Alloys to conduct Heat ....... 381
Mr. A. Cayley on the Surface which is the Envelope of
Planes through the Points of an Ellipsoid at right
angles to the Radii Vectores from the Centre ............... 383
Professors A. Kölliker and E. Pelikan on the Physiolo-
gical Action of the Tanghinia venenifera .................. 384
Dr. Alison on the Differential Stethophone ................ 385
Proceedings of the Geological Society:—
Dr. Abich on the History and Formation of Etna .......... 395
Lieut. Godwin-Austen on the Lacustrine or Karéwah De-
posits of Kashmir ............................................ 395
The Rev. S. Haughton on the Black Mica of the Granite
of Leinster and Donegal .................................... 396
Mr. T. F. Jamieson on an Outlier of Lias in Banffshire .. 397
Prof. Owen on a Collection of Australian Fossils ........ 397
Mr. J. Brown on the Occurrence of some Tertiary Fossils at
Chislet ......................................................... 397
Mr. S. Bate on a Fossil Crustacean found in the Magnesian Limestone of Durham ........................................... 397
Mr. J. W. Salter on *Eurypterus* ......................................................... 397
Mr. C. Gould on a New Fossil Crustacean from the Lower Greensand of Atherfield ........................................... 398
On the Relations between the Tensions and Temperatures of Vapours, by J. A. Groshans ....................................... 398
On Intermittent Fluorescence, by J. Müller ........................................... 399
On the Increase of the Electrical Resistance of the Metals with the Temperature, by R. Clausius ......................... 400

---

**NUMBER CIX.—DECEMBER.**

Archdeacon Pratt on the Figure of the Indian Meridian ........ 401
Prof. Plücker on the Electrical Discharge through Rarefied Gases (*continued*) ........................................... 408
Prof. Potter on the Properties of Compound Double-refracting Rhombs ......................................................... 419
Dr. Genth’s Contributions to Metallurgy ........................................... 420
Mr. W. R. Grove on the Influence of Light on the Polarized Electrode .......................................................... 426
Dr. Atkinson on Monoacetate of Glycol, and on the Preparation of Glycol ......................................................... 433
Dr. H. Debus on the Action of Caustic Potash on Glycerate of Potash .............................................................. 438
Mr. G. Gore on the Properties of Electro-deposited Antimony. 441
Proceedings of the Royal Society:—

  Captain Pullen on Deep-sea Soundings ................................. 458
  Mr. A. Claudet on the Stereomonoscope .................................. 462
  Prof. W. Thomson on the Stratification of Vesicular Ice by Pressure .............................................................. 463
  Dr. Hargreave on the Problem of Three Bodies ......................... 466

Proceedings of the Geological Society:—

  The Rev. O. Fisher on some Natural Pits on the Heaths of Dorsetshire ......................................................... 473
  Mr. G. W. Ormerod on the occurrence of an Earthquake along the Northern Edge of the Granite of the Dartmoor District, on September 28, 1858 ......................................................... 473
  Mr. G. W. Ormerod on certain Veins of Granite in the Carbonaceous Rocks on the North and East of Dartmoor. 474
  Mr. N. T. Wetherell on the Structure of some of the Siliceous Nodules of the Chalk ........................................... 474
  Mr. C. W. Stow on some Fossils from South Africa ................ 474
  Dr. Rubidge on some points in the Geology of South Africa. 475
  The Hon. C. A. Murray on some Mineral Springs at Teheran, Persia .............................................................. 477
Observations on Mr. Couper's New Chemical Theory, by M. A. Kekulé ........................................ 478
Meteoric Stone of Borkut ........................................ 479
On Crystallized Valerianate of Atropine, by H. Callmann ...... 480
Crystallized Furnace-products .................................. 480

NUMBER CX.—SUPPLEMENT TO VOL. XVI.

Mr. J. J. Waterston on the Theory of Sound .................. 481
Prof. Secchi on Solar Spots, and the Method of determining their Depth ........................................ 495
Mr. D. Vaughan on Luminous Meteors and Temporary Stars. 500
M. B. W. Feddersen on the Electric Spark .................. 503
Dr. Atkinson's Chemical Notices from Foreign Journals ....... 516
Proceedings of the Royal Society:—
   Drs. Williamson and Russell on the Measurement of Gases in Analysis ........................................ 524
   Mr. R. Moon on the Theory of Internal Resistance and Internal Friction in Fluids; and on the Theories of Sound and of Auscultation ........................................ 528
   Mr. T. Hopkins on the Influence of Heated Terrestrial Surfaces in disturbing the Atmosphere ............... 531
   Dr. Hofmann on the Poly-ammonias .......................... 532
   Mr. W. Fairbairn on the Resistance of Tubes to collapse. 534
   Baron Pollock on some Remarkable Relations which obtain among the Roots of the Four Squares into which a Number may be divided ........................................ 537
Proceedings of the Geological Society:—
   Sir R. I. Murchison on the Geological Structure of the North of Scotland and the Orkney and Shetland Islands. 543
On some Properties of Ice near its Melting-point, by Professor J. D. Forbes ........................................ 544
Index ..................................................................... 547

PLATE.

I. Illustrative of Prof. Plücker's Paper on the Action of the Magnet upon the Electrical Discharge in Rarefied Gases, and M. Feddersen's on the Electric Spark.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Royal Observatory, Greenwich, June 16, 1858.

In the present state of the enterprise for laying down the Atlantic Submarine Telegraph Cable, and after the failure of last year, it appears desirable to investigate, with the utmost practicable accuracy, the circumstances of such a cable in the act of deposit; especially with reference to the tension which its different parts sustain. The problem is by no means simple, as will appear from the following considerations:

First, if there were no frictional resistance of water (its influence in diminishing the sensible weight of the cable being always recognized), still the circumstances of what may be called a travelling curve are totally different from those of a fixed catenary, and require an investigation of totally different form. This, however, is not difficult; and the result, as will be seen in the first investigation below, is remarkably simple.

Secondly, the frictional resistance of the water to the movement of the cable is so great as entirely to modify the circumstances of its deposit. Some time since, I had prepared an investigation of approximate character, on the supposition that the friction is small, when I learnt from good authority that, in consequence of the slenderness of the Atlantic Cable and the lightness of its materials, its terminal velocity in falling through water would not much exceed three feet per second; and that the ship's velocity would probably be more than double the terminal velocity of the falling cable. It was impossible after


B
this to consider the resistance as a small quantity, for which an
approximation to the first order might suffice; and I then
undertook the investigation in its fullest generality, as regarded
the magnitude of the resisting force.

In regard to the law of the resisting force, another difficulty
presented itself. It is known that, with large velocities, the re-
sistance varies nearly as the square of the velocity. This law
offers almost insuperable difficulties to mathematical treatment,
if we do not restrict ourselves closely by limitations as to the
form and position of the curve. But with the term depending
on the square of the velocity there is combined a term depending
on the simple power of the velocity; and with movements so
slow as those of the cable in some parts, the term depending on
the simple power may perhaps be the predominant term. I
have therefore considered that no error of the least importance
would be introduced by assuming the resistance generally to
vary as the simple power of velocity, provided that the co-
efficient be so adjusted that the resistance at or near the
terminal falling velocity may be correct. As far as any error
is sensible it will be of this kind, that, for the lower velocities, I
have assumed a too great resistance; the general effect of this,
however, will be neutralized by assuming the ship’s velocity to be
a trifle greater than I have taken it in the numerical calculations.

There is another difficulty peculiar to the circumstances of a
moving cable. The cable moves longitudinally as well as
laterally, and there is a considerable longitudinal friction as
well as a lateral resistance. I have no data for determining the
coefficient of this friction. A small error, however, in the co-
efficient is not important, for the longitudinal motion of the
cable is always much less than its lateral motion. Under these
circumstances I have considered that the law and coefficient of
the longitudinal friction might, without practical error, be
assumed to be the same as those of the lateral friction.

The combination of these two assumptions introduces great
comparative simplicity into the formulæ; for it makes the
friction in the directions of any rectangular coordinates exactly
proportional to the resolved velocities in the directions of the
respective coordinates. The differential equations resulting
from the mechanical considerations are therefore linear, and to
the facilities for solution peculiar to linear equations is entirely
due the success of the investigation. This inquiry, general in
its geometrical suppositions, but not wholly supported in its
physical assumptions, occupies the second part.

Thirdly, it appears from the last investigation that one of the
possible forms of the curve is a straight line. It is easy to see
that it will be practicable to solve the equations for this form,
of Deposit of a Submarine Cable.

with no limitation as to the physical laws. Accordingly the investigation is then undertaken on the suppositions, that the resistance varies as the square of the velocity, that the coefficient for longitudinal motion may be different from that for lateral motion, and that the speed of delivery of the cable may be different from the ship's speed. Commencing with the equations in their most general form, it is shown that the straight line is one of the forms of solution; and the tension of the cable is investigated on different suppositions, as to the proportion of the coefficients, and as to the rate of the ship and that of cable-delivery. I trust that this investigation will be found to possess real practical value. This is the third part of the paper.

I shall now proceed with the investigations.

1. It is assumed that the bottom of the sea is level, that the ship moves with uniform velocity and delivers out the cable with uniform velocity, and that the form of the curve taken by the cable from the point of leaving the ship to the point of touching the ground is always the same. The movement of any individual point of the cable may therefore be conceived as compounded of two motions, namely, a sliding down of the point along the curve, while that curve travels horizontally after the ship.

2. The following is the notation which will be employed:—
   \( n \) the ship's velocity.
   \( m \) the velocity of delivery of the cable (when it differs from the ship's velocity).
   \( x \) the horizontal ordinate of any point of the cable; measured from the place where the cable touches the ground, in the direction of the ship's motion.
   \( x' \) the horizontal ordinate of the same point measured from a fixed origin. \( x' \) therefore \( = x + nt + K \).
   \( y \) the vertical ordinate of the same point, measured upwards from the bottom.
   \( s \) the length of the curve from the place where the cable touches the bottom to the same point.
   \( s' \) the length of the cable from a fixed origin to the same point. \( s' \) therefore \( = s + nt + L \) (if the cable be delivered with the same speed with which the ship goes), or \( = s + mt + L \) (if delivered with a different speed).
   \( T \) the tension of the cable at the same point, as measured by the length \( T \) of the cable weighed in the water.
   \( g \) the acceleration produced by gravity in one second.
   \( g' \) the acceleration produced by gravity when diminished in
the proportion of the cable's real weight to its apparent weight in water,

\[ a = \frac{n^2}{g} = \text{twice the height due to velocity } n, \text{ with diminished gravity.} \]

b the coefficient of the simple power of velocity in the formula, \( b \times \text{velocity} = \text{frictional resistance.} \)

c the proportion of the ship's speed to the terminal velocity of the cable when falling freely in water, \( \frac{bn}{g^p}. \)

Additional symbols will be introduced as they are required.

3. In the first place, the form of the curve will be investigated when the velocity of delivery of the cable is equal to the ship's velocity, and when the frictional resistance is neglected.

4. The equations of motion of the point, whose coordinates are \( x' \) and \( y, \) are

\[
\frac{d^2x'}{dt^2} = \text{accelerating force in the direction of } x';
\]

\[
\frac{d^2y}{dt^2} = \text{accelerating force in the direction of } y;
\]

or, considering the small part of the cable included between the points \( s \) and \( s + \delta s, \)

\[
\frac{d^2x'}{dt^2} = \frac{\delta (\text{real value of the horizontal part of tension})}{\text{real mass of } \delta s} \times g;
\]

\[
\frac{d^2y}{dt^2} = \frac{\delta (\text{real value of the vertical upward part of tension})}{\text{real mass of } \delta s} \times g - g'.
\]

Now, considering the tension as expressed by a length of weight of the cable as weighed in water, the real value of the horizontal part of tension \( = T \frac{dx}{ds}, \) and therefore \( \delta \) (real value of the horizontal part of tension) \( = \delta \left( T \frac{dx}{ds} \right) = \frac{d}{ds} \left( T \frac{dx}{ds} \right) \times \delta s. \) But the real mass of \( \delta s \) in the denominator, on which the inertia depends, is not expressed by the length \( \delta s \) as weighed in water, but by the length \( \delta s \times \frac{g}{g'} \) as weighed in air, or by the length \( \delta s \times \frac{g}{g'} \) as weighed in water. Thus we obtain

\[
\frac{d^2x'}{dt^2} = g' \frac{d}{ds} \left( T \frac{dx}{ds} \right);
\]

and similarly,

\[
\frac{d^2y}{dt^2} = g' \frac{d}{ds} \left( T \frac{dy}{ds} \right) - g'.
\]
5. Now \( x' = x + nt + K \); therefore
\[
\frac{dx'}{dt} = \frac{dx}{dt} + n = \frac{dx}{ds} \cdot \frac{ds}{dt} + n.
\]
But \( s' = s + nt + L \); and, for a definite point of the cable, \( s' \) is
invariable with regard to time: therefore \( 0 = \frac{ds}{dt} + n \); and con-
sequently
\[
\frac{dx'}{dt} = -n \frac{dx}{ds} + n,
\]
from which
\[
\frac{d^2x'}{dt^2} = -n \frac{d}{dt} \left( \frac{dx}{ds} \right) = -n \frac{d}{ds} \left( \frac{dx}{ds} \right) \cdot \frac{ds}{dt} = +n^2 \frac{d^2x}{ds^2}.
\]
Also
\[
\frac{dy}{dt} = \frac{dy}{ds} \cdot \frac{ds}{dt} = -n \frac{dy}{ds} ; \quad \frac{d^2y}{dt^2} = -n \frac{d}{dt} \left( \frac{dy}{ds} \right) = +n^2 \frac{d^2y}{ds^2}.
\]
6. The equations of motion therefore become
\[
n^2 \frac{d^2x}{ds^2} = g' \frac{d}{ds} \left( \frac{T dx}{ds} \right);
\]
\[
n^2 \frac{d^2y}{ds^2} = g' \frac{d}{ds} \left( \frac{T dy}{ds} \right) - g'.
\]
Or transposing and dividing by \( g' \),
\[
0 = \frac{d}{ds} \left\{ \left( T - a \right) \frac{dx}{ds} \right\} ;
\]
\[
0 = \frac{d}{ds} \left\{ \left( T - a \right) \frac{dy}{ds} \right\} - 1 ;
\]
Integrating,
\[
\left( T - a \right) \frac{dx}{ds} = c,
\]
\[
\left( T - a \right) \frac{dy}{ds} = c' + s.
\]
At the bottom, on the supposition that there is even the
smallest conceivable tension, \( \frac{dy}{ds} = 0 \); and \( s = 0 \); therefore \( c' = 0 \).
But \( c \) is (nearly) the tension at the bottom, and must be retained.
7. Dividing the equation \( (T - a) \frac{dx}{ds} = c \)
by the equation \( (T - a) \frac{dy}{ds} = c' \)
we find
\[
\frac{dx}{dy} = \frac{c}{s'},
\]
the equation to the common catenary. Also \( T - a \) is given by
the same equation which gives the tension in the common catenary. Hence we obtain this singular result: that the form of the travelling curve is exactly the same as the common catenary; and that the tension at every point is exactly the same as at the corresponding point of a similar fixed catenary, increased only by an absolute constant $a$, which depends only on the velocity with which the curve travels. In the instance of the Atlantic cable, this constant $a$ is insignificantly small in comparison with the other tensions.

8. In the second place, I shall investigate the form of the curve when the velocity of delivery of the cable is equal to the ship's velocity, and when the frictional resistance is fully taken into account.

9. The accelerating force produced by the frictional resistance in any direction $= -b \times$ velocity in that direction. Attaching the proper terms therefore to our former equations, we now have

$$n^2 \frac{d^2x}{ds^2} = g' \frac{d}{ds} \left( \frac{dx}{ds} \right) - b \frac{dx}{dt} ;$$

$$n^2 \frac{d^2y}{ds^2} = g' \frac{d}{ds} \left( \frac{dy}{ds} \right) - g' - b \frac{dy}{dt} .$$

Remarking that $\frac{dx}{dt} = n - n \frac{dx}{ds}$, $\frac{dy}{dt} = -n \frac{dy}{ds}$, and that $\frac{bn}{g'} = e$, these equations become

$$0 = \frac{d}{ds} \left\{ (T - a) \frac{dx}{ds} \right\} + e \frac{dx}{ds} - e ;$$

$$0 = \frac{d}{ds} \left\{ (T - a) \frac{dy}{ds} \right\} - 1 + e \frac{dy}{ds} .$$

Integrating, with the same attention to the constants as before,

$$(T - a) \frac{dx}{ds} = c - ex + es ;$$

$$(T - a) \frac{dy}{ds} = s - ey ;$$

from which $\frac{dy}{dx} = \frac{s - ey}{c - ex + es}$, the differential equation to the curve.

10. The treatment of this repulsive equation is much simpler than might have been anticipated. Expressing $s$ in terms of the other quantities,

$$s = \frac{e \frac{dy}{dx} - ex \frac{dy}{dx} + ey}{1 - e \frac{dy}{dx}} ;$$
and differentiating,
\[
\sqrt{(1 + \left(\frac{dy}{dx}\right)^2)} = \frac{d^2y}{dx^2} \times \frac{c - ex + e^2y}{\left(1 - e \frac{dy}{dx}\right)^2}.
\]

Put \( p \) for \( \frac{dy}{dx} \) where convenient, and put \( v \) for \( c - ex + e^2y \); the equation then becomes
\[
\frac{1}{v} \cdot \frac{dv}{dx} = \frac{-e \frac{dp}{dx}}{(1 - ep) \sqrt{(1 + p^2)}},
\]
in which the variables are separated, and the forms are well known. If \( w = \frac{1}{1 - ep} \), the integral is
\[
\sqrt{\left(1 + \frac{1}{e^2}\right)} \cdot \log \frac{D}{v} = \log \left\{ w - \frac{1}{e^2 + 1} + \sqrt{\left(w^2 - \frac{2}{e^2 + 1} \cdot w + \frac{1}{e^2 + 1}\right)} \right\};
\]
from which, after removing the logarithms and restoring former symbols,
\[
e\frac{1}{e^2 + 1} \left(\frac{c - ex + e^2y}{D}\right)^{\sqrt{(1 + \frac{1}{e^2})}} = \frac{e}{e^2 + 1} \left(\frac{c - ex + e^2y}{D}\right)^{\sqrt{(1 + \frac{1}{e^2})}} = 2 \left(\frac{e + \frac{dy}{dx}}{1 - e \frac{dy}{dx}}\right).
\]
As \( x = 0, y = 0 \), and \( \frac{dy}{dx} = 0 \), at the bottom, \( D \) may be determined. On substituting,
\[
\left\{ \begin{array}{l}
\{ \sqrt{(e^2 + 1)} + e\} \left(1 - e \frac{x}{e} + e^2 \frac{y}{c}\right)^{\sqrt{(1 + \frac{1}{e^2})}} \\
- \{ \sqrt{(e^2 + 1)} - e\} \left(1 - e \frac{x}{e} + e^2 \frac{y}{c}\right)^{\sqrt{(1 + \frac{1}{e^2})}}
\end{array} \right\} \times \left(1 - e \frac{dy}{dx}\right)
\]

Integrating, determining the constant by making \( x \) and \( y \) vanish together, and putting
\[
z = 1 - e \frac{x}{e} + e^2 \frac{y}{c}, \quad \zeta = 2 e \frac{x}{e} + 2 \frac{y}{e} + 2 + 4 e^2,
\]
the integrated equation becomes
\[
\frac{\sqrt{(e^2 + 1)} - e}{\sqrt{(e^2 + 1)} + e} z^{\sqrt{(1 + \frac{1}{e^2})} + 1} + \frac{\sqrt{(e^2 + 1)} + e}{\sqrt{(e^2 + 1)} - e} z^{-\sqrt{(1 + \frac{1}{e^2})} + 1} = \zeta.
\]
11. This equation defines the curve algebraically, and also gives us the means of tracing it graphically. If we assume arbitrary values for \( z \), and compute from the equation the corresponding values of \( \xi \), we may then determine the numerical values of \( x \) and \( y \) from these equations,

\[
\frac{x}{c} = \frac{e}{2(e^2+1)} \xi - \frac{z}{c(e^2+1)} - \frac{2e^2-1}{e},
\]

\[
\frac{y}{c} = \frac{\xi}{2(e^2+1)} + \frac{z}{e^2+1} - 2.
\]

But, before making numerical computations, the following treatment of the symbols will give a clearer idea of the nature of the curve. If \( \lambda \) be the angle whose cotangent is \( e \) (which angle may be called "the limiting angle"), then

\[
\begin{align*}
z &= \frac{e \sqrt{(e^2+1)}}{c} \left\{ \frac{e}{c \sqrt{(e^2+1)}} - x \sin \lambda + y \cos \lambda \right\}, \\
\xi &= \frac{2 \sqrt{(e^2+1)}}{c} \left\{ \frac{c(2e^2+1)}{\sqrt{(e^2+1)}} + x \cos \lambda + y \sin \lambda \right\}.
\end{align*}
\]

From these expressions it appears that \( z \) and \( \xi \) are simple multiples of rectangular coordinates, on a system in which the ordinate corresponding to \( \xi \) is inclined upwards from \( x \) by the angle \( \lambda \), and the ordinate corresponding to \( z \) is inclined from \( y \) in the direction from the ship by the angle \( \lambda \); the values of the new coordinates which apply to the first origin of coordinates (the place where the cable touches the ground) being, for the ordinate of which \( z \) is a multiple, \( \frac{e \sqrt{(e^2+1)}}{c} \); and for the ordinate of which \( \xi \) is a multiple, \( \frac{e(2e^2+1)}{\sqrt{(e^2+1)}} \).

12. Remarking that the index of \( z \) in the second term of the formula for \( \xi \) is essentially negative, it will be seen that upon making \( z \) smaller and smaller, but always positive, \( \xi \) becomes indefinitely great, but positive. Consequently the curve has an asymptote in the direction of the ordinate corresponding to \( \xi \) positive, that is, making the angle \( \lambda \) with the horizon, inclined upwards as from points near the place of touching the ground to points near the ship. The asymptote does not pass through the place of touching the ground, but below it, its smallest distance being \( \frac{e}{c \sqrt{(e^2+1)}} \); and it does not pass through the ship, but below it, its nearest distance being \( \frac{e}{c \sqrt{(e^2+1)}} \times \) the value of \( z \) at the ship. It is unnecessary for our purpose to examine the form of the branch of the curve corresponding to \( z > 1 \).
13. Combining the consideration of the asymptote with the previously established characteristics of the curve (that when \(x=0, y=0, \) and \( \frac{dy}{dx} = 0 \)), its general form will be seen to be this.

In its lower part it resembles a circular arc, or the lower part of a common catenary. But in its upper part, the curve does not tend to become vertical, but tends to approach to an asymptote, making the angle \( \lambda \) with the horizon: which angle, when the ship's speed greatly exceeds the terminal falling velocity of the cable, will be very small; implying that the asymptote and the upper part of the curve will be little inclined to the horizon.

14. Other formulae, necessary for a complete knowledge of the properties of the curve and the tension at different points, will be obtained thus:

Differentiating the equations at the beginning of (11),
\[
\frac{d}{dz} \left( \frac{w}{c} \right) = \frac{e}{2(e^2 + 1)} \cdot \frac{d\xi}{dz} - \frac{1}{e(e^2 + 1)}
\]
\[
\frac{d}{dz} \left( \frac{y}{c} \right) = \frac{1}{2(e^2 + 1)} \cdot \frac{d\xi}{dz} + \frac{1}{e^2 + 1}.
\]

Squaring these, adding, and extracting the square root,
\[
\frac{d}{dz} \left( \frac{s}{c} \right) = \frac{-1}{2e \sqrt{(e^2 + 1)}} \sqrt{\left\{ e^2 \left( \frac{d\xi}{dz} \right)^2 + 4 \right\}}
\]
(the negative sign being taken because, as \( z \) diminishes, \( s \) increases),

\[
= \frac{-1}{2e \sqrt{(e^2 + 1)}} \left\{ \sqrt{(e^2 + 1) + e} z^{-\sqrt{(1 + \frac{1}{e^2})}} + \sqrt{(e^2 + 1) - e} z^{\sqrt{(1 + \frac{1}{e^2})}} \right\}
\]

Integrating, and making \( s \) vanish when \( x \) and \( y = 0 \),
\[
2 \sqrt{(e^2 + 1)} \left( \frac{s}{c} + 2e \right) = \frac{\sqrt{(e^2 + 1) + e} z^{-\sqrt{(1 + \frac{1}{e^2})}} + 1}{\sqrt{(e^2 + 1) - e} z^{\sqrt{(1 + \frac{1}{e^2})}} + 1}.
\]

15. Again, to find \( \frac{ds}{dx} \); putting for abbreviation
\[
\left\{ \sqrt{(e^2 + 1) + e} z^{-\sqrt{(1 + \frac{1}{e^2})}} \right\} = q,
\]
we have
\[
\frac{ds}{dx} = \frac{ds}{dz} \cdot \frac{dz}{dx} = \frac{-e}{2e \sqrt{(e^2 + 1)}} \left\{ q + \frac{1}{q} \right\} \cdot \frac{dz}{dx}.
\]
But
\[
\frac{dz}{dx} = \frac{1}{c} \cdot \frac{2e(e^2 + 1)}{e^2 \frac{d\xi}{dz} - 2} = \frac{-1}{c} \cdot \frac{2e(e^2 + 1)}{e \left( q - \frac{1}{q} \right) - 2};
\]
The Astronomer Royal on the Conditions

therefore

\[ \frac{ds}{dx} = \sqrt{(e^2 + 1)} \frac{\frac{1}{q}}{\frac{1}{q} - 2}. \]

Then, to find \( \frac{T-a}{c} \), we have merely to form \( \frac{ds}{dx} (1 + \frac{es}{c} - \frac{ex}{c}) \), as given by one of the last equations of Article 9.

16. By means of these formulae, the numbers in the following Tables have been computed, on the supposition that the ship's velocity is double the terminal velocity of the cable in falling through water, or that \( e = 2 \).

The first Table exhibits the ordinates and tensions in a definite cable-curve. The unit of measure is the constant \( c \), and the unit of tension is the weight in water of a piece of the cable whose length \( = c \).

<table>
<thead>
<tr>
<th>( x )</th>
<th>( y )</th>
<th>( \frac{x}{c} )</th>
<th>( \frac{y}{c} )</th>
<th>( \frac{\xi}{c} )</th>
<th>( \frac{T-a}{c} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.90</td>
<td>18.213</td>
<td>0.0527</td>
<td>0.0013</td>
<td>0.0527</td>
<td>1.0073</td>
</tr>
<tr>
<td>0.80</td>
<td>18.458</td>
<td>0.0316</td>
<td>0.0058</td>
<td>0.1118</td>
<td>1.0074</td>
</tr>
<tr>
<td>0.70</td>
<td>18.742</td>
<td>0.0178</td>
<td>0.0142</td>
<td>0.1791</td>
<td>1.0228</td>
</tr>
<tr>
<td>0.60</td>
<td>19.078</td>
<td>0.0255</td>
<td>0.0278</td>
<td>0.2576</td>
<td>1.0240</td>
</tr>
<tr>
<td>0.50</td>
<td>19.487</td>
<td>0.0347</td>
<td>0.0487</td>
<td>0.3517</td>
<td>1.0401</td>
</tr>
<tr>
<td>0.40</td>
<td>20.002</td>
<td>0.0404</td>
<td>0.0802</td>
<td>0.4490</td>
<td>1.0630</td>
</tr>
<tr>
<td>0.30</td>
<td>20.689</td>
<td>0.0578</td>
<td>0.1289</td>
<td>0.6242</td>
<td>1.0960</td>
</tr>
<tr>
<td>0.20</td>
<td>21.700</td>
<td>0.0820</td>
<td>0.2100</td>
<td>0.8515</td>
<td>1.1471</td>
</tr>
<tr>
<td>0.10</td>
<td>23.549</td>
<td>1.1997</td>
<td>0.3749</td>
<td>1.2655</td>
<td>1.2434</td>
</tr>
<tr>
<td>0.05</td>
<td>25.556</td>
<td>1.6062</td>
<td>0.5656</td>
<td>1.7145</td>
<td>1.3491</td>
</tr>
</tbody>
</table>

On laying down a curve with these ordinates, it will be seen to possess the qualifications that we have mentioned; its lower part resembling a catenary, and its upper part approaching to a straight line.

17. The second Table exhibits the elements of different cable-curves corresponding to different amounts of stray length of the cable, in water of a definite depth. The unit of measure here is the depth of the sea or \( y \). The numbers in the last line are taken from the investigations in the next three articles. They will be materially altered by the more practical considerations in the third section.
of Deposit of a Submarine Cable.

<table>
<thead>
<tr>
<th>Angle made by the cable with the horizon at leaving the ship, (\text{cosine } \frac{dx}{ds})</th>
<th>Tension at the point of leaving the ship, (T = \frac{x}{y})</th>
<th>Length of suspended cable, (\frac{s}{y})</th>
<th>Corresponding horizontal extent, (\frac{x}{y})</th>
<th>Difference, or stray length necessary for the slope of the suspended cable, (\frac{s-x}{y})</th>
</tr>
</thead>
<tbody>
<tr>
<td>3 52</td>
<td>747:23</td>
<td>393:26</td>
<td>39311</td>
<td>0:015</td>
</tr>
<tr>
<td>5 43</td>
<td>173:62</td>
<td>193:03</td>
<td>19267</td>
<td>0:036</td>
</tr>
<tr>
<td>8 34</td>
<td>71:33</td>
<td>126:16</td>
<td>12564</td>
<td>0:052</td>
</tr>
<tr>
<td>11 22</td>
<td>36:78</td>
<td>9:254</td>
<td>9:184</td>
<td>0:070</td>
</tr>
<tr>
<td>14 9</td>
<td>21:36</td>
<td>7:222</td>
<td>7:134</td>
<td>0:088</td>
</tr>
<tr>
<td>16 53</td>
<td>13:26</td>
<td>5:849</td>
<td>5:742</td>
<td>0:107</td>
</tr>
<tr>
<td>19 32</td>
<td>8:50</td>
<td>4:843</td>
<td>4:716</td>
<td>0:127</td>
</tr>
<tr>
<td>22 6</td>
<td>5:46</td>
<td>4:055</td>
<td>3:905</td>
<td>0:150</td>
</tr>
<tr>
<td>24 30</td>
<td>3:32</td>
<td>3:376</td>
<td>3:200</td>
<td>0:176</td>
</tr>
<tr>
<td>25 37</td>
<td>2:39</td>
<td>3:031</td>
<td>2:839</td>
<td>0:192</td>
</tr>
<tr>
<td>26 34</td>
<td>0:53</td>
<td>2:236</td>
<td>2:000</td>
<td>0:236</td>
</tr>
</tbody>
</table>

The limiting angle.

It will be seen that a small change in the stray length makes a great change in the tension.

18. It is important to examine into the form and tension of the curve in one case (of which the results are included, for convenience, in the last line of the last Table), namely, the case when \(c\), the only constant, receives the special value 0. It will be remembered that \(c\) is sensibly the tension of the cable at the point where it touches the ground. This tension cannot, in the nature of things, be negative; for every case where it is positive, the formulæ already detailed are sufficiently complete; but when the tension at the bottom \(= 0\), these formulæ are without meaning, and a new investigation must be made.

19. Taking up the investigation where \(c\) is first introduced in Article 9, and making \(c = 0\), we have \(\frac{dy}{dx} = \frac{s - ey}{es - ex}\); then, as in Article 10,

\[
\sqrt{1 + \left(\frac{dy}{dx}\right)^2} = -e^{\frac{dy}{dx}^2} \cdot \frac{x - ey}{1 - e^{\frac{dy}{dx}^2}}
\]

Put \(v\) for \(x - ey\), and proceed as in Article 10; we ultimately find

\[
\left\{\begin{aligned}
\frac{e^2 + 1}{e} \cdot \frac{e}{\sqrt{(e^2 + 1) - e}} \cdot \left(\frac{x - ey}{D'}\right) & - \sqrt{(1 + \frac{1}{e^2}) + 1} \\
- \frac{e}{e^2 + 1} \cdot \frac{e}{\sqrt{(e^2 + 1) + e}} \cdot \left(\frac{x - ey}{D'}\right) & \sqrt{(1 + \frac{1}{e^2}) + 1}
\end{aligned}\right\} = \frac{2}{D'}(ex + y) + E'.
\]

Let us examine into the value of the terms of this equation when \(x\) and \(y\) are 0, on the supposition that \(D'\) and \(E'\) have finite values. The index of the first exponential term is negative, and therefore, when \(x - ey = 0\), if \(D'\) have a value, that term will be infinitely great; which is inconsistent with the
value of the other side of the equation. Therefore $D'$ must $= 0$. Again, the index of the second exponential term is positive; and therefore if at any other point of the curve $x - ey$ have a value, while $D' = 0$, that second term will be infinitely great. Consequently, $x - ey$ must always $= 0$. This equation, it will be seen, with its derivatives,

$$e \frac{dy}{dx} = 1, \quad \frac{d^2y}{dx^2} = 0, \quad \frac{ds}{dx} = \sqrt{\left(1 + \frac{1}{e^2}\right)}, \quad s = x \sqrt{\left(1 + \frac{1}{e^2}\right)},$$

satisfies the original equations from which it was deduced.

20. In this case then (when the cable is delivered exactly as fast as the ship sails, but when the tension at the bottom $= 0$), the form of the cable is a straight line, inclined to the horizon at the angle whose tangent $= \frac{1}{e}$, that is, at the angle $\lambda$. Its tension will be found from the original formula

$$T - a = e(s - x) \frac{ds}{dx} = e \sqrt{\left(1 + \frac{1}{e^2}\right)} x \left\{ \sqrt{\left(1 + \frac{1}{e^2}\right)} - 1 \right\},$$

or

$$= y \sqrt{\left(1 + \frac{1}{e^2}\right)} \left\{ \sqrt{\left(1 + \frac{1}{e^2}\right)} - 1 \right\}.$$

21. The result for the last case of our investigation, showing that the cable may assume the form of a straight line (a form which simplifies the mathematical conditions of the problem so much as to enable us to dispense with some of the limitations of supposition formerly made), suggests the following as an investigation to be made in the third place. Assuming (as a hypothesis to be confirmed by the investigation) that the cable, from the point of quitting the ship to the point of touching the ground, has the form of a straight line; assuming that the resistance in moving through the water is proportional to the square of velocity; assuming that the coefficient of resistance for longitudinal motion is different from that for lateral motion; and assuming that the rate of delivery of the cable may be greater than the ship's speed; to find the position and tension of the cable.

22. The notation of Article 2 may be retained, with the following addition only:

A the coefficient of the square of cable's lateral velocity in the formula, $A \times (\text{lateral velocity})^2 =$ lateral resistance.

B the coefficient of the square of cable's longitudinal velocity in the formula, $B \times (\text{longitudinal velocity})^2 =$ longitudinal resistance.

If we limit the meaning of $e$ to the proportion of the ship's speed to the terminal velocity of the cable when falling laterally
of Deposit of a Submarine Cable.

in water; since $A \times (\text{terminal velocity})^2 = g'$, we have, terminal velocity $= \sqrt{\frac{g'}{A}}$, and therefore $e = n \sqrt{\frac{A}{g'}}$.

23. Taking the first equations of Article 9, we must make

$$\frac{dx'}{dt} = n-m \frac{dx}{ds}, \quad \frac{dy}{dt} = -m \frac{dy}{ds};$$

whence

$$\frac{d^2x'}{dt^2} = + m^2 \frac{d^2x}{ds^2}, \quad \frac{d^2y}{dt^2} = + m^2 \frac{d^2y}{ds^2};$$

and

$$m^2 \frac{d^2x}{ds^2} = g' \frac{d}{ds} \left( T \frac{dx}{ds} \right) - \text{(resistance of water to motion in direction of } x' \text{ increasing)},$$

or $+$ (resistance of water to motion in direction of $x'$ diminishing);

$$m^2 \frac{d^2y}{ds^2} = g' \frac{d}{ds} \left( T \frac{dy}{ds} \right) - g' + \text{(resistance of water to motion in direction of } y \text{ diminishing}).$$

These equations apply to any form of the cable-curve.

24. To express the last terms of these equations by means of $\frac{dx}{ds}, \frac{ds}{dt}$, &c., we must express the lateral motion and the longitudinal motion by those quantities; then express the resistances to lateral and longitudinal motion, and then, from these, find the resistances in the direction of $x$ and $y$. And we must be careful to remark that both the lateral motion and the longitudinal motion are always downwards. Now the lateral motion downwards

$$= \frac{dx'}{dt} \cdot \frac{dy}{ds} - \frac{dy'}{dt} \cdot \frac{dx}{ds} = \left( n - m \frac{dx}{ds} \right) \frac{dy}{ds} + m \frac{dy}{ds} \cdot \frac{dx}{ds} = n \frac{dy}{ds};$$

and the lateral resistance, normal to the curve and acting upwards,

$$= \Lambda n^2 \left( \frac{dy}{ds} \right)^2. \quad \text{The longitudinal motion downwards}$$

$$= - \frac{dx}{dt} \cdot \frac{dx}{ds} - \frac{dy}{dt} \cdot \frac{dy}{ds} = \left( -n + m \frac{dx}{ds} \right) \frac{dx}{ds} + m \frac{dx}{ds} \cdot \frac{dy}{ds} = m - n \frac{dx}{ds};$$

and the longitudinal resistance, in the direction of the tangent and acting upwards, $= B \left( m - n \frac{dx}{ds} \right)^2$. From these we find,

Last term in the equation for $x =$

$$B \left( m - n \frac{dx}{ds} \right)^2 \cdot \frac{dx}{ds} - \Lambda n^2 \left( \frac{dy}{ds} \right)^2 \cdot \frac{dy}{ds}.$$
The Astronomer Royal on the Conditions

Last term in the equation for \( y = \)
\[
B\left( m - n \frac{dx}{ds} \right)^2 \cdot \frac{dy}{ds} + \Lambda n^2 \left( \frac{dy}{ds} \right)^2 \cdot \frac{dx}{ds}.
\]

And the equations of Article 23 become:
\[
m^2 \frac{d^2 x}{ds^2} = g' \frac{d}{ds} \left( T \frac{dx}{ds} \right) + B \left( m - n \frac{dx}{ds} \right)^2 \cdot \frac{dx}{ds} - \Lambda n^2 \left( \frac{dy}{ds} \right)^2; \\
m^2 \frac{d^2 y}{ds^2} = g' \frac{d}{ds} \left( T \frac{dy}{ds} \right) - g' + B \left( m - n \frac{dx}{ds} \right)^2 \cdot \frac{dy}{ds} + \Lambda n^2 \left( \frac{dy}{ds} \right)^2 \cdot \frac{dx}{ds}.
\]

Dividing by \( g' \); remarking that \( \frac{m}{g'} = \frac{m^2}{n^2} \), \( \frac{n}{g'} = \frac{n^2}{a} \), and that \( \frac{n^2 \Lambda}{g'} = e^2 \); the equations become
\[
0 = \frac{d}{ds} \left\{ \left( T - m^2 \frac{y}{n^2} a \right) \frac{dx}{ds} \right\} + \frac{B}{A} e^2 \left( m - n \frac{dx}{ds} \right)^2 \cdot \frac{dx}{ds} - \left( \frac{dy}{ds} \right)^3; \\
0 = \frac{d}{ds} \left\{ \left( T - m^2 \frac{n^2}{a} \right) \frac{dy}{ds} \right\} - 1 + \frac{B}{A} e^2 \left( m - n \frac{dx}{ds} \right)^2 \cdot \frac{dy}{ds} + \left( \frac{dy}{ds} \right)^2 \cdot \frac{dx}{ds}.
\]

These equations are accurate, on the suppositions that the resistances are proportional to the square of the velocity, and that the coefficients for lateral and for longitudinal motion are different; and they are perfectly general, as regards the form assumed by the cable.

25. We shall now limit our supposition of the form of the cable-curve to that of the straight line, and shall ascertain the conditions under which it will satisfy these equations. Let \( y = f \cdot x \), \( f \) being a constant multiplier; \( s = \sqrt{(1 + f^2)} \cdot x; \)
\[
\frac{dx}{ds} = \frac{1}{\sqrt{(1 + f^2)}}; \quad \frac{dy}{ds} = \frac{f}{\sqrt{(1 + f^2)}}; \quad \left( T - m^2 \frac{y}{n^2} a \right) \frac{dx}{ds} = \frac{1}{\sqrt{(1 + f^2)}} \left( T - m^2 \frac{a}{n^2} \right); \\
\frac{d}{ds} \left\{ \left( T - m^2 \frac{a}{n^2} \right) \frac{dx}{ds} \right\} = \frac{1}{\sqrt{(1 + f^2)}} \cdot \frac{dT}{ds} = \frac{1}{\sqrt{(1 + f^2)}} \cdot \frac{dT}{ds} = \frac{1}{\sqrt{(1 + f^2)}} \cdot \frac{dT}{ds} = \frac{1}{1 + f^2} \cdot \frac{dT}{ds};
\]
similarly,
\[
\frac{d}{ds} \left\{ \left( T - m^2 \frac{a}{n^2} \right) \frac{dy}{ds} \right\} = \frac{f}{1 + f^2} \cdot \frac{dT}{dx}.
\]
Substituting these, the equations become
\[
0 = \frac{1}{1 + f^2} \cdot \frac{dT}{dx} + \frac{B}{A} e^2 \left( m - \frac{1}{\sqrt{(1 + f^2)}} \right)^2 \cdot \frac{1}{\sqrt{(1 + f^2)}} - \frac{f^3}{(1 + f^2)^{\frac{3}{2}}}; \\
0 = \frac{f}{1 + f^2} \cdot \frac{dT}{dx} - 1 + \frac{B}{A} e^2 \left( m - \frac{1}{\sqrt{(1 + f^2)}} \right)^2 \cdot \frac{f}{\sqrt{(1 + f^2)}} + \frac{f^2}{(1 + f^2)^{\frac{3}{2}}}.
\]

26. To eliminate \( \frac{dT}{dx} \), multiply the first equation by \( f \), and
subtract the second. The remainder gives

\[ 0 = 1 - e^2 \frac{f^4 + f^2}{(1 + f^2)^{3/2}}, \quad \text{or} \quad 0 = 1 - \frac{e^2 f^2}{\sqrt{1 + f^2}}; \]

hence

\[ \sqrt{1 + f^2} = e^2 f^2, \quad \text{or} \quad \frac{1}{f^2} = \sqrt{\left( e^4 + \frac{1}{4} \right) - \frac{1}{2}}; \]

and with this value of \( f \) the equations are satisfied. It will be seen that \( f \) is the trigonometrical tangent of the angle made by the cable with the horizon, or \( \frac{1}{f} \) that of the angle made by the cable with the vertical. It is to be remarked, that this angle is independent of the fractions \( \frac{B}{A} \) and \( \frac{m}{n} \).

27. If \( e = 2 \), \( \frac{1}{f^2} = \sqrt{\left( \frac{65}{4} \right) - \frac{1}{2}} \), and the angle made by the cable with the horizon is \( 28^\circ \, 1' \, 10'' \). If \( e = 3 \), \( \frac{1}{f^2} = \sqrt{\left( \frac{325}{4} \right) - \frac{1}{2}} \), and the angle made by the cable with the horizon is \( 18^\circ \, 55' \, 0'' \).

28. To find the tension, we may take either of the final equations of Art. 25 and integrate it: the first equation will be the more convenient. The arbitrary constant will be determined by the consideration that the tension at the place of contact with the bottom = 0; and therefore when \( x = 0 \), \( T = 0 \). Thus we find

\[ T = - \frac{B}{A} \, e^2 \left( \frac{m}{n} - \frac{1}{\sqrt{1 + f^2}} \right)^2 \cdot \sqrt{1 + f^2} \cdot x + e^2 \cdot \frac{f^3}{\sqrt{1 + f^2}} \cdot x; \]

or, if we prefer to express it in terms of the vertical ordinate \( y \),

\[ T = - \frac{B}{A} \, e^2 \left( \frac{m}{n} - \frac{1}{\sqrt{1 + f^2}} \right)^2 \cdot \frac{\sqrt{1 + f^2}}{f} \cdot y + e^2 \cdot \frac{f^2}{\sqrt{1 + f^2}} \cdot y, \]

\[ = y - \frac{B}{A} \, e^2 \left( \frac{m}{n} - \frac{1}{\sqrt{1 + f^2}} \right)^2 \cdot \frac{\sqrt{1 + f^2}}{f} \cdot y. \]

If \( e = 2 \),

\[ \frac{e^2 \sqrt{1 + f^2}}{f} = 8.514, \quad \frac{1}{\sqrt{1 + f^2}} = 0.883; \]

and

\[ T = y - \frac{B}{A} \times 8.514 \times \left( \frac{m}{n} - 0.883 \right)^2 \times y. \]

If \( e = 3 \),

\[ \frac{e^2 \sqrt{1 + f^2}}{f} = 27.760, \quad \frac{1}{\sqrt{1 + f^2}} = 0.946; \]

and

\[ T = y - \frac{B}{A} \times 27.760 \times \left( \frac{m}{n} - 0.946 \right)^2 \times y. \]
If \( e = 0 \) (that is, if the ship is stationary), and if \( f \) have the corresponding value \( \infty \) (that is, if the cable hang vertically down, which makes the tension the least possible), \( T = y \).

29. The tension is the greatest possible when the cable is delivered with the least speed possible, that is, when \( \frac{m}{n} = 1 \).

This gives, for \( e = 2 \),

\[
T = y \times \left\{ 1 - \frac{B}{A} \times 0.117 \right\} ;
\]

and, for \( e = 3 \),

\[
T = y \times \left\{ 1 - \frac{B}{A} \times 0.081 \right\}.
\]

30. The tension will be diminished by increasing the speed of delivery of the cable; but without fixing upon a numerical value for \( \frac{B}{A} \) (the proportion of the coefficient of longitudinal resistance to that of lateral resistance), no statement of its value can be given more intelligible than those in the formula of Article 28. The tension will be made \( = 0 \) if we can make the second term of the expression \( = y \). This gives, for \( e = 2 \),

\[
\left( \frac{m}{n} - 0.883 \right)^2 = \frac{A}{B \times 8.514},
\]

or

\[
\frac{m}{n} = 0.883 + 0.3427\sqrt{\frac{A}{B}}
\]

(the other root of the quadratic being inadmissible, as it makes \( \frac{m}{n} < 1 \)). Thus, suppose \( \frac{B}{A} = \frac{1}{4} \), the tension is 0 if \( \frac{m}{n} = 1.68 \); or if the cable is delivered with a waste of 0.568 on the geographical length. If \( \frac{B}{A} = \frac{1}{9} \), the tension is 0 if \( \frac{m}{n} = 1.911 \).

For \( e = 3 \), the tension will be \( = 0 \) if

\[
\left( \frac{m}{n} - 0.946 \right)^2 = \frac{A}{B \times 27.760},
\]

or

\[
\frac{m}{n} = 0.946 + 0.1898\sqrt{\frac{A}{B}}.
\]

If \( \frac{B}{A} = \frac{1}{4} \), the tension is 0 if \( \frac{m}{n} = 1.326 \), or if the cable is delivered with a waste of 0.326 on the geographical length. If \( \frac{B}{A} = \frac{1}{9} \), the tension is 0 if \( \frac{m}{n} = 1.516 \).
Thus the tension may be diminished with smaller loss of cable when $e$ is large (that is, when the ship is running quickly) than when $e$ is small (or when the ship is running slowly).

31. The results of the preceding investigations will be applicable, I conceive, with great accuracy in the circumstances supposed; namely, uniform depth of water, and uniform motion of the ship; requiring only for their complete numerical interpretation an accurate evaluation of $e$ (which, it is believed, is pretty well known), and an accurate measure of $\frac{B}{A}$ (on which, apparently, little is known). It is almost unnecessary to remark that the investigation takes no account of the unevenness of the bottom or the disturbance of waves.

32. Before quitting the subject, I will revert to the second investigation, founded upon assumptions which make the equations linear and resolvable in the general case. On referring to the results in Article 11 and following articles, it will be seen that they do not give facilities for ascertaining the form and tension of the curve when $e$ is very small, that is, when the resistance of the water to the cable's motion is very small. I will here indicate the form and the results of a process applicable to this case. Assume $y = Y + \delta Y$, $s = S + \delta S$, where $Y$ and $S$ are the values corresponding to $e = 0$. The equation $\frac{dy}{dx} = \frac{s - ey}{e - ex + es}$ of Art. 9 gives, when $e = 0$, $\frac{dY}{dx} = \frac{S}{c}$, from which, as is wellknown, $Y$ is found $= \frac{c}{2} (e^x + e^{-x} - 2)$, $S = \frac{c}{2} (e^x - e^{-x})$. Now let $Y$ and $S$ be increased by the quantities $\delta Y$ and $\delta S$ of the same order as $e$, and let the squares, &c. of $\delta Y$ and $\delta S$ be neglected. Since

$$\left(\frac{dS}{dx}\right)^2 - \left(\frac{dY}{dx}\right)^2 = 1,$$

and

$$\left(\frac{d(S + \delta S)}{dx}\right)^2 - \left(\frac{d(Y + \delta Y)}{dx}\right)^2 = 1,$$

it follows that

$$\frac{dS}{dx} \cdot \frac{d \delta S}{dx} = \frac{dY}{dx} \cdot \frac{d \delta Y}{dx}.$$ Let

$$\frac{d \delta S}{dx} = e \cdot \frac{dY}{dx};$$

then $\frac{d \delta Y}{dx}$ will $= e \cdot \frac{dS}{dx}$; also $\delta S = \int e \cdot \frac{dY}{dx}$. Substituting these in the equation $(e - ex + es) \frac{dy}{dx} = s - ey$, differentiating, and putting $W$ for

$$\frac{d}{dx} \left( S \frac{dY}{dx} - X \frac{dY}{dx} + Y \right),$$

it is found that

$$\frac{dp}{dx} = -\frac{2}{c} \cdot \frac{W}{e^x + e^{-x}}.$$
Substituting for $W$, integrating, substituting to form $\delta Y$ and $\delta S$, and determining the constants of integration to satisfy the primary equation and the condition that $\delta S = 0$ when $x = 0$, we find at length,

$$\delta S = e \times \left\{ -\frac{c}{4} \left( e^c + e^{-\frac{2x}{c}} + 10 \right) + \left( \frac{x^2}{4c} + \frac{3c}{2} \right)(e^c + e^{-\frac{x}{c}}) - \frac{x}{2} \right\} \left( e^c - e^{-\frac{x}{c}} \right),$$

$$\delta Y = e \times \left\{ -\frac{c}{4} \left( e^c - e^{-\frac{2x}{c}} \right) + \left( \frac{x^2}{4c} + \frac{3c}{2} \right)(e^c - e^{-\frac{x}{c}}) - \frac{x}{2} \right\} \left( e^c + e^{-\frac{x}{c}} + 2 \right),$$

$$T - a = \frac{c}{2} \left( e^c + e^{-\frac{x}{c}} \right) + e \left\{ -\frac{c}{4} \left( e^c - e^{-\frac{2x}{c}} \right) + \left( \frac{x^2}{4c} + c \right)(e^c - e^{-\frac{x}{c}}) - \frac{x}{2} \right\} \left( e^c + e^{-\frac{x}{c}} \right).$$

G. B. Airy.

II. On the Striae seen in the Electrical Discharge in vacuo.

By W. R. Grove, V.P.R.S. &c.

In a paper communicated to the Royal Society in January 1852, I published the first notice of transverse bands or striae which are seen in the discharge from a Ruhmkorff coil in the vacuum of an air-pump, wherein is placed a piece of phosphorus; and in a postscript to that paper (Phil. Mag. vol. iv.) I stated that the striae could be seen in several attenuated gases, probably in all. Some time after the publication of my paper, but without having seen it, M. Ruhmkorff observed the same phenomenon in the vapour of alcohol; and it has subsequently been experimented on by Messrs. Masson, Quét, Du Moncel, Robinson, and Gassiot, the last of whom has elaborately examined it in the Torricellian vacuum.

No satisfactory rationale of the phenomenon has been given. In my first paper I gave no opinion as to its cause; but subsequently, in a communication to the British Association at Hull, I stated my belief that it was closely connected with the interruptions of the contact breaker. The following is the notice of my communication in the Athenaeum, August 30, 1856:—"Mr. Grove has observed that the mode of breaking contact has a marked influence on the phenomenon. If, for instance, the arm of the contact breaker be made to rest on a slight spring placed underneath it, the bands become narrower. If a single breach of contact be effected, most observers have remarked that the effect is still perceptible: but it is very difficult to effect a single breach of contact; the fusion of the metals at the point of contact with the vibration accompanying the movement, occasions a double or triple disruption. The best mode is to place two stout copper wires across each other, and with a firm hand draw

* Communicated by the Author.
one over the other until the end of the former parts company with the latter; when this is well done the striae are, in the majority of cases, not observed. Of all the substances which have been tried, the vapour of phosphorus succeeds best; and with this is seen a remarkable effect on the powder or smoke of allotropic phosphorus (which is always formed when the striae are observed): this smoke traverses from pole to pole from the negative to the positive side, showing, unless there be some latent optical deception, a mechanical effect of the discharge under the circumstances."

Dr Robinson, who has a little misunderstood my meaning in this passage, says (Proc. Royal Irish Acad., December 1856), "Mr. Grove appears to think it arises from some vibration in the metal of a contact breaker, which produces a fluctuation in the inducing current." I did not mean to say that the effect was due to any peculiar vibration of the metal of the contact breaker, but to the interrupted and successive discharges of the apparatus itself,—that the changes in the character of the discharge attendant upon changes in the action of the contact breaker, as well as the frequent absence of striae when only a single disruption was effected, afforded evidence that the striae were connected with the multiplied discharges. My notice was, however, short and somewhat obscure, as, although I entered fully on the matter in the Section-room, I had prepared no memoir on the subject. I still retain the opinion I then expressed, though I do not assert it as a positive conviction; the difficulty of proving it arose from the circumstance that it was next to impossible to produce, by the ordinary modes, a single discharge from the induced coil, for the following reasons. When one extremity of the wire of the primary coil is drawn over the other, an induced current is produced at the moment of disruption in the secondary wire and a consequent discharge in the vacuum. But at this same moment of disruption, the extra or induced current in the primary wire itself finds a passage in the form of a spark immediately the contact is broken, and this extra current occasions necessarily a second induced current in the secondary wire, which having a ready path opened to it by the discharge of the previous current, would be discharged through that path, though it might not have tension enough to overcome any great resistance. Although, therefore, these discharges cannot be separated to the eye, it by no means follows that there are not two discharges when an apparently single disruption of the primary circuit takes place.

I had repeatedly and in vain attempted to get rid of this difficulty, and to produce what I could feel assured was a single induced discharge, when a very simple plan occurred to me,
which I am surprised did not more readily present itself: it is simply to make an interruption in the secondary circuit, besides that formed by the vacuum; such second interruption to be in air, and of the extreme length of the spark, so that not more than one spark should pass at a time across the intervening air, and consequently not more than one discharge through the vacuous space.

Using the air-pump with phosphorus and a stick of potash in the receiver, I connected by a wire one of the secondary terminals of a Ruhmkorff coil with the rod which entered the receiver at the top. The wire from the second terminal was attached to a glass stand or pillar, so that its end projected; to another glass stand was attached a wire with one end also projecting, and the other end was fixed to the brass work of the air-pump. The two projecting ends could be made to touch, or to remain at any desired distance from each other.

The ends of the wires being in contact, the coil apparatus was set at work, and the striæ very beautifully exhibited in the receiver; the projecting wires were now gradually separated, and the striæ for some time continued visible and until the points of the wires were so far apart that an occasional spark only passed from point to point when the striæ disappeared, and a uniform luminous cloud was produced in the receiver. Even in this extreme case, however, there is apt to be a double discharge, which any one who has not watched closely the phenomenon with eye and ear, would not detect, but would pronounce the discharge to be single.

From unavoidable minute differences in the action of the contact breaker, discharges will, all conditions being apparently the same, vary in strength and length; those accustomed to the apparatus will know that, by separating the terminals beyond the normal distance of the discharge, sparks will still pass at occasional, distant and irregular intervals, proving the slightly varying intensity of the apparatus.

Now, when the points are separated to the maximum for the experiment which I have detailed above, two classes of sparks will be perceived in the air,—the one a thin blue spark giving to the ear a single sharp sound, and the other a burred yellow spark with a sound not so clear and metallic, but attended with a slight whizz: this contrast of sound may be roughly compared to that presented by a whole or cracked piece of porcelain when struck; it requires attention and habit to distinguish these two classes; but when the power is attained, it will be seen that with the first or single discharge there are no striæ in the receiver, with the second, which I believe to be a double discharge, striæ are visible.
By careful adjustment I have been able to obtain sparks of the former class only, and then the experiment may be continued indefinitely with no striæ visible; but on the slightest alteration in the distance the striæ appear. It is, however, more frequently the case, that, notwithstanding all the care used, sparks of both characters will pass in irregular succession; and then the discharges will be without striæ in some cases, and with them in others.

It is necessary to mention the above distinctions, as otherwise any person repeating the experiments might be deceived, and not succeeding in obtaining discharges without striæ, though the discharges were apparently single, would imagine the results I have described to be illusions arising from an error of vision.

I repeated the above experiments in one of Mr. Gassiot’s vacuum-tubes, which showed the striæ beautifully under ordinary circumstances; but when the division in the circuit was carefully made and carried to its fullest extent, the discharges passed without any striæ, the tube being filled at each discharge with a uniform glow.

From the above experiments I am satisfied that in attenuated media, wherein the striæ are well seen when the ordinary interrupted currents are used, discharges may be made to pass which exhibit no striæ, and under such circumstances that there is every reason to believe single discharges only pass. Join to this the fact that the appearances of the striæ vary with any variation of the contact breaker, that the striæ are more numerous and narrower in proportion as the intermissions of the contact breaker are more rapid, which I have observed to be very notably the case, the evidence seems strong, if not conclusive, that the striæ or bands are due to the mechanical interference or reciprocal impulsions of two or more discharges, or rather of the medium affected by them.

I will take this opportunity of mentioning one experiment which I believe to be new and not devoid of interest. M. Plücker and Mr. Gassiot have published several interesting experiments on the effects produced by magnets on the striæ or on the electric discharge in highly attenuated media commonly called vacua.

In repeating some of these experiments with a vacuum-tube kindly given me by Mr. Gassiot, 2 feet 9 inches long between the platinum wires, and 1 inch internal diameter, I found that when the pole of a powerful bar magnet was brought close to the positive platinum in a certain direction, the visible discharges only passed occasionally and at intervals through the tube, notwithstanding the apparatus went on working, and the contact breaker vibrating as usual. After many experiments, I found
that I could stop the discharge entirely by placing the poles of two magnets opposite each other and in a particular direction with reference to the line of discharge. The two platinum wires in this tube are inserted perpendicularly to the axis, and penetrate half an inch into the tube. If, now, the tube be placed horizontally, the platinum wires uppermost and the observer looking along the tube from the positive to the negative wire, the north pole of a magnet should be placed on the right-hand side, and as nearly as can be to the platinum, i.e. touching, or nearly so, the glass tube, while the south pole of another magnet is similarly placed on the opposite or left-handed side. The discharges cease entirely, and there is no conduction through the tube, showing that it is not a case of dark discharge, but an arresting of the passage of the discharge through the attenuated medium. If the tube be turned one-fourth round its axis in either direction, the discharges reappear, as they also do if the magnets be reversed. The experiment requires care, and it seems to be necessary to make it with a power of coil just sufficient to enable the discharge to traverse the distance between the platinum terminals and with a certain magnetic power: if made with a powerful electro-magnet, the discharge is stopped, whichever side the poles be placed with reference to the platinum wires, but the discharge is only made intermittent when the platinum wires are placed in a line joining the poles of the magnet. In the above-mentioned tube, the portion of one end of the glass around one of the platinum wires was covered with a dark deposit of platinum, from the terminal at this end having been constantly used as the negative pole; when this was made positive instead of the clean end, the effect of arresting the current could not be produced: this circumstance, and the delicacy of the conditions necessary to ensure success in this experiment, lead me to think that the stoppage of the discharge is due to the diversion in direction of the electric line of force produced by the magnet; the discharge is, so to speak, blown out by the magnet: but I hesitate to express a positive opinion on this point.

When magnets are made to approach the negative platinum, there is no stoppage of the discharge, but attractions and repulsions are observed on the blue flame, which have been described by Mr. Gassiot, and which I need not therefore repeat.

I tried the effect of magnets on the spark from Ruhmkorroff's coil in air, but could not arrest them, though they appeared to pass with somewhat greater difficulty and longer intermissions; the difference was, however, very slight.

June 4, 1858.

On the equilibrium-figures of revolution which are neither spherical nor cylindrical.

HAVING completed the theoretical investigation of a jet of liquid in the preceding series†, I return, in the present series, to liquid masses without weight, and continue the examination of equilibrium-figures of revolution.

It will, in the first place, be remembered that the general condition satisfied by the free surface of a weightless mass of liquid in a state of equilibrium is expressed by

\[ \frac{1}{R} + \frac{1}{R'} = C, \]

where \( R \) and \( R' \) are the two principal radii of curvature at any point of the surface, and are considered positive or negative according as they are directed towards the interior or exterior of the mass. \( C \) represents a constant.

It is well known that, in surfaces of revolution, one of the radii \( R \), \( R' \) is the radius of curvature of the meridian curve at the point under consideration, and the other is the portion of the normal intercepted between this point and the axis of revolution, or, according to a simpler mode of expression, it is the normal at this point. On this account, and in order to avoid all ambiguity, I replace the letters \( R \) and \( R' \) by \( M \) and \( N \), whereby we shall be reminded that the former represents the radius of curvature which belongs to the meridian curve, and the latter the one which constitutes the normal. In this notation, therefore, the equation of equilibrium is

\[ \frac{1}{N} + \frac{1}{M} = C. \]

Mathematicians are aware that the quantities \( M \) and \( N \) can be expressed by differentials; thus transformed, the above equation is completely integrable by elliptic functions, so that the nature of the corresponding meridian curves can be deduced. This forms the subject of a recent memoir by M. Beer, wherein, for the second time, the author honours me by applying the calculus to the results of my experiments. The same object, too, may be attained without the aid of elliptic functions by means

* The original memoir will be found in the thirty-first volume of the Mémoires de l'Académie de Bruxelles. The abstract, of which a translation is here given, appeared in the Annales de Chimie et Physique, May 1858.
of a property discovered by M. Delaunay, by means of the calculus, and afterwards demonstrated geometrically by M. Lamarle. In another series I propose to speak of these analytical and geometrical resources; in the present one I arrive at a knowledge of the forms of the meridian curves, and of all their modifications and details, by means of experiment, and with the aid of simple reasoning applied to the relation between the radius of curvature and the normal which the equation of equilibrium establishes. This research, in which experiment and theory always march side by side, may, moreover, be considered as a verification of the latter.

I commence by demonstrating the following principle: amongst all the equilibrium-figures of revolution, the sphere is the only one whose meridian curve meets the axis; amongst the spheres is included, of course, the plane which is their limit.

To prove this I remark, in the first place, that the meridian curve of a surface satisfying the above general equation cannot attain the axis except in a direction perpendicular to the same; for if it cut the axis obliquely or touched it, the normal N would be zero at the point of intersection or contact, and consequently the quantity $\frac{1}{M} + \frac{1}{N}$, which equilibrium requires to be constant, would become infinite at this point, whilst it would be finite at neighbouring points.

I assume then a non-circular curve meeting the axis perpendicularly; I conceive its curvature to increase on leaving the point on the axis; and I take a small arc of the curve, with one extremity on the axis, such that the curvature increases throughout its length; lastly, through the extremities of this arc, I draw a circle having its centre on the axis. By reflecting upon this construction the following conclusions will easily be deduced:—

First. Since the arc of the curve and the arc of the circle both leave the axis in the same direction and, after immediately separating, rejoin each other at their other extremity, it follows that the curvature of the first is at the commencement less, but afterwards greater than that of the circular arc; at the point where the two arcs rejoin each other, then, the radius of curvature $M$ of the arc of the curve is less than the radius of the circular arc; the same is the case, too, with the normal $N$, for the latter evidently cuts the axis at a less acute angle than does the radius.

Secondly. If upon the arc of the curve and upon the circular arc two equally long portions be taken, having a common extremity on the axis, and so small that at the other extremity of the first the curvature has not yet ceased to be less than that of the circular arc, the normal $N$, corresponding to this extremity, will evidently be longer than the radius of the circle, and, in con-
sequence of the inferior curvature, the radius of curvature \( M \) at this extremity will also be greater than the radius of the circle. But since, with respect to an arc of a curve thus placed, the radius of curvature and the normal have a like direction, and consequently the same sign, it follows from the above that the quantity \( \frac{1}{M} + \frac{1}{N} \) would be greater at the point where this arc rejoins the circle than at a point very close to the axis, a result which is incompatible with the condition of equilibrium. If we were to suppose that the curvature diminishes from the axis outwards, the same mode of reasoning would show that the quantity \( \frac{1}{M} + \frac{1}{N} \) would be smaller at the point where the arc of the curve rejoins that of the circle than at a point very near the axis.

Equilibrium, therefore, is not possible unless the curvature be constant and, at the same time, the meridian curve meet the axis normally; in other words, this meridian curve must be a circle having its centre on the axis, or, in the case of an infinite radius, a line perpendicular to the axis; hence the figure generated is necessarily either a sphere or a plane.

From this principle, it follows that the meridian curves of other equilibrium-figures either stretch to infinity or close outside the axis; we shall afterwards find that the first of these conditions is alone fulfilled.

These points established, I proceed next to the examination of the curves in question. An iron cylinder, say 15 centimetres long and 2 in diameter, is maintained horizontally at a certain height in the alcoholic mixture, and a mass of oil of suitable volume is caused to adhere to the same. This mass spreads itself over the surface of the cylinder so as to envelope a portion of the length of the latter, and assumes the form of a figure of revolution whose meridian curve, convex in the middle, changes the nature of its curvature towards its extremities, where it becomes tangential to the generating line of the cylinder*.

The same figure formed entirely of oil, that is to say, without having a solid cylindrical core, may be obtained by attaching a mass of oil—at first in excess—to two vertical discs of the same diameter as the above cylinder, and placed opposite one another at a distance asunder equal to the length of the figure obtained with the cylinder, and afterwards by removing, with the small syringe, as much liquid as is found necessary in order to make the extreme elements of the meridian curve appear horizontal;

* M. Beer, in the memoir above-mentioned, suggests the same experiment as being suitable for the verification of one of the results of his calculus; I had, however, made the experiment long before.
this experiment, however, is not so easily made to succeed as the first.

It must be observed that this experiment, as well as all the following ones, must alike be made in the alcoholic mixture, and that the precautions, indicated in the first and second series, for reducing the two liquids to the same density, and causing them to exercise no sensible chemical action upon one another, must be attended to.

Since the complete meridian curve cannot terminate as above described, the preceding figure is only a partial one. I follow the meridian curve, therefore, beyond the extremities of this same figure.

In the first place, these extreme points cannot be points of inflection; for if they were, the radius of curvature would be there infinite, and the equation of equilibrium reduced to \( \frac{1}{N} = C \). But we have seen that, between its vertex and the points under consideration, the curve presents two points of inflection, at which, therefore, the equation really reduces itself to the form \( \frac{1}{N} = C \). Hence the normals at the two former points must be equal to those at the two latter, which is evidently impossible according to the respective positions of these two pairs of points, and the directions of the corresponding normals. I have, it is true, here supposed that at a point of inflection the radius of curvature is necessarily infinite, whereas it is well known that the same may also be zero. In our meridian curves, however, the vanishing of the radius of curvature is inadmissible, for the quantity \( \frac{1}{M} + \frac{1}{N} \) would be thereby rendered infinite at such a point. Beyond the extremities of the partial figure, therefore, our curve still preserves a curvature of the same kind as it had before reaching the same, that is to say it is still concave towards the exterior, and the points at these extremities are at a minimum distance from the axis.

This being established, I set off, from one of these minimum points and upon the production of the curve, an arc so small that throughout its whole length the curvature either continually increases or diminishes, and, from the same point but on the other, or near side of the same, I set off an arc of equal length. At all points of these two small arcs the radius of curvature and the normal have opposite directions, so that the quantity \( \frac{1}{M} + \frac{1}{N} \) constitutes a difference, and consequently cannot remain

which are neither Spherical nor Cylindrical.

constant on passing from one point to another unless M and N either increase or decrease simultaneously. But if we were to suppose that, upon the small arc which forms the production of the curve, the curvature increases more, or diminishes less rapidly than upon the other small arc, a little attention will show that at the extremity of the first the radius of curvature would be smaller, whilst the normal would be larger than at the extremity of the second; and on the other hand, if we were to suppose that beyond the minimum point the curvature increases less, or diminishes more rapidly than on this side, we should find that at the extremity of the first small arc the radius of curvature would be greater, and the normal less than at the extremity of the second.

Thus the constancy of the quantity $\frac{1}{M} + \frac{1}{N}$ necessitates the identity, or rather the perfect symmetry of the two small arcs; but this symmetry involves, as a consequence, that of the two whole portions of the curve situated on opposite sides of the minimum point. By repeated applications of the same reasoning to both extremities of the realized liquid figure, I conclude that the same reproduces itself indefinitely on both sides. The complete meridian curve, therefore, undulates indefinitely alongside the axis, from which it recedes and towards which it approaches periodically; its maxima and minima distances from the same being all equal (fig. 1), so that the generated figure consists of

Fig. 1.

an indefinite series of equal ventral segments and nodes. For the sake of brevity, and in consequence of the form of its meridian curve, I call this figure the unduloid (onduloiide).

When a greater diameter is given to the solid cylinder in the above experiment, the mass of oil remaining the same, this mass spreads itself out more over the solid surface, the meridian curvatures become feebler, and that the more, the larger the cylinder. From this I conclude that, within certain limits, the unduloid may change its form, and that one of these limits is the cylindrical form.

On the contrary, with thinner solid cylinders and the same mass of oil, variations of an opposite kind manifest themselves; that is to say, the mass spreads itself out less in the direction of the axis, and the meridian curvatures increase. In this manner
we may conclude that the second limit of the variations of a complete unduloid consists of an indefinite series of equal spheres touching each other on the axis.

In each of these experiments is realized that portion of a complete unduloid which is comprised between the middle points of two successive nodes, so that the figure of oil is composed of a whole ventral segment (renflement) adjacent to two semi-nodes (demi-étranglements). I have also realized a portion of an unduloid consisting of an entire node between two portions of ventral segments. For this purpose I attach a sufficiently large mass of oil to two solid vertical rings, 7 centims. in diameter, placed opposite to each other at a distance of 11 centims. asunder. By gradually absorbing portions of liquid, the figure soon becomes cylindrical with convex bases, and afterwards narrower and narrower in the middle. By operating cautiously, I can reduce the diameter of a node, at its middle point, to 27 millims. without causing the figure to lose its stability. As to the bases of the figure, they remain convex. I remark that in this experiment we witness the variations of the unduloid from the cylinder itself, which, as above mentioned, forms one of its limits.

From the fact that portions of an unduloid more extended than those above described cannot be realized, I conclude that the unduloid, like the cylinder, has a limit of stability.

I resume the experiments with the solid cylinder. If, whilst the diameter of this cylinder remains the same, the mass of oil be gradually increased, the liquid figure will at the same time increase more and more in length and in equatorial thickness. In this kind of variation the minima distances from the axis do not change, for they are always equal to the radius of the solid cylinder; but at the point of the meridian curve which is the most distant from the axis, that is to say at the equator of the realized figure, the radius of curvature and the normal both increase. Let us now suppose that whenever a fresh quantity of oil is added to the mass, one (say the left) extremity of the same is always brought to the same position on the cylinder, so that the right extremity alone recedes. If we imagine these variations pushed to their limit, it is clear that all the nodes of the complete meridian curve will then have receded to infinity, or, in other words, have disappeared, with the exception of that which corresponds to the left extremity of the figure of oil, and that this meridian curve will then be reduced to one having two infinite branches—like the parabola—having its vertex turned towards the axis of revolution, and its axis of symmetry perpendicular to the same.

It is, however, easy to determine the nature of this curve; for since, as above remarked, the radius of curvature and the normal
corresponding to the equator of the ventral segment increase as the volume of the mass of oil is augmented, it is clear that they and this volume will become infinite at the same time; at this limit, therefore, the quantity \( \frac{1}{M} + \frac{1}{N} \), which is constant at all points of the meridian curve, is equal to zero. But from this we deduce the relation \( M = -N \), which shows that at each point of the curve the radius of curvature is equal and opposite to the normal. Geometricians are well aware that the catenary is the only curve which enjoys this property.

From this it follows that the variations of the unduloid have a third limit, which is a new equilibrium-figure whose meridian curve is a catenary with its vertex turned towards the axis of revolution, and its radius of curvature at this vertex equal and opposite to the distance between the same and the axis. The form of its meridian curve here suggests the name catenoid for the corresponding equilibrium-figure.

In the last experiment above recorded, it was only by an imaginary extension of the operation that we arrived at the catenoid; a portion of this new figure, however, may be easily realized in the following manner:—In the first place a cylinder is formed between two solid rings, whose distance asunder does not exceed two-thirds of their common diameter, afterwards liquid is gradually withdrawn from the mass. In this manner, whilst the portion between the two rings becomes hollowed out more deeply to form a node, the bases are seen to lose gradually their convexity, until finally they become altogether plane. Now, when the figure of the oil has arrived at this phase, we have on the bases \( \frac{1}{M} + \frac{1}{N} = 0 \); for considering the plane as a surface of revolution, the radius of curvature and the normal are evidently infinite at each of its points; but equilibrium requires that the quantity \( \frac{1}{M} + \frac{1}{N} \) should have the same value at all points of the realized surface, consequently on the node we have \( \frac{1}{M} + \frac{1}{N} = 0 \), which relation, as we have just seen, characterizes the catenoid.

In the memoir, I show that as long as the bases have not entirely lost their curvature the node belongs to the unduloid; in this experiment, therefore, we witness the transition of a portion of an unduloid from the cylinder to the catenoid.

When the distance between the rings is much less than two-thirds of their diameter, when, for example, it is reduced to the half or the third of this diameter, then, after arriving at the plane bases, if we continue to absorb the oil, these bases are observed to become more and more concave as liquid is sub-
tracted, whilst at the same time the node becomes thinner. The node now belongs neither to the catenoid nor to the unduloid, but to a new figure whose meridian curve we must examine.

The above explanations respecting the sphere, the catenoid, and the unduloid are sufficient to give an idea of the nature of the reasoning applied to the equation of equilibrium; with respect to the new figure, therefore, I shall limit myself to the following brief indications.

In the first place, it must be observed that the reasoning by means of which, in the case of the unduloid, the perfect symmetry of the meridian curve on opposite sides of a minimum point was established, applies equally to a minimum point corresponding to any other equilibrium-figure, and consequently to that of the meridian arc of the node under consideration;—this conclusion, we may add, is confirmed by the apparent form of the node. From this it follows that the complete curve has an axis of symmetry perpendicular to the axis of revolution, and passing through the middle of the node; so that whatever feature is presented by the curve on one side of the first of these axes, is presented symmetrically on the other side.

Pursuing, afterwards, the meridian curve beyond the rings, I show that it passes through two points where its elements are parallel to the above axis of symmetry, and I verify this deduction by means of a mass of oil comprised between two discs, instead of two rings, the distance between the discs not exceeding about the third of their diameter. By gradually subtracting oil I succeed, finally, in rendering the meridian curve tangent, at its two extremities, to the planes of the discs.

I then show that the curve produced beyond these points returns towards the axis of symmetry, in a point of which its two branches cut so as to form a loop.

I have realized the portion of the figure generated by the whole of this loop; but before describing the experiment it will be well to recall a principle, demonstrated in the second series, according to which, when a surface satisfies the general condition of equilibrium, it is a matter of indifference on which side of this surface the liquid may be situated. In the two last experiments just indicated, the liquid was situated on the convex side of the meridian curve, so that in this case the portion of the figure generated by the whole loop would form a cavity in the interior of the mass of oil; now according to the above principle the oil may also be supposed to be situated on the concave side of the curve, in which case the portion generated by the loop will be in relief. It is thus that the figure is realized in the following experiment.

In the first place, a liquid biconvex lens is formed in a ring of
iron wire. This is done by introducing the ring into a sphere of oil, of sufficiently large diameter, and afterwards subtracting a portion of the liquid. When, by gradual absorption, the thickness of the lens has been reduced to about a sixth of the diameter of the ring, the former is pierced in its centre in a manner indicated in the memoir; the oil then takes the form of an annular figure, whose meridian curve, rounded on the side of the axis of revolution, has a sharp point on the side of the solid ring; it is in fact the meridian curve of the figure generated by our loop.

The liquid figure thus obtained, however, only exists for a few seconds, showing thereby that it is unstable; it soon becomes deformed, and finally disunited on one side.

On examining what becomes of the branches of the curve after their intersection at the point of the loop, I find that each, preserving the character of its curvature, recedes to a maximum distance from the axis of revolution, and afterwards returns towards this axis so as to form an arc convex outwards. I have realized the portion generated by this arc by introducing a mass of oil between two discs, rendering the same first cylindrical, and afterwards causing one of the discs to approach the other. The liquid figure then becomes bulged, and the meridian convexity increasing as the distance between the discs diminishes, it passes through the form of a portion of a sphere, after which it constitutes successively the figures generated by a portion of the arc in question, afterwards that generated by the whole arc, and finally it attains a figure generated by a more extended arc of the complete meridian curve. These successive arcs, however, belong to different cases of the equilibrium-figure.

I further show that the arc in question—in accordance with the appearance of the realized figures—is perfectly symmetrical on each side of its middle point, so that the curve has another axis of symmetry passing through this point, and perpendicular to the axis of revolution.

Hence we must clearly conclude that our arc which comes from a loop arrives at a second, identical with the first; that this second is followed in the same manner by a third, and so on indefinitely. The complete meridian curve, therefore, consists of an indefinite series of equal loops, connected by equal

Fig. 2.

arcs, and arranged alongside the axis of revolution (fig. 2).
consequence of this form I give to the generated figure the name

*nodoid* (*nodôide*).

The nodoid presents a remarkable peculiarity: it cannot,
in its complete state, be conceived to be formed by a liquid; in
fact, from the form of the meridian curve, it is at once seen that
the portions generated by the loops would be imbedded in the
interior of the mass; but these same portions, as well as other
suitably chosen ones, may, as I have described, be isolated.

On performing the above experiments, by means of which
these portions are realized, it becomes evident that the nodoid,
like the unduloid, is susceptible of variations. The following
example will elucidate my meaning. In the experiment which
commences with a biconvex lens, the thickness of the same may,
before piercing, be considerably reduced; the aperture thus
made then widens spontaneously, the meridian loop becomes
much shorter, and its point much more blunt. Thus, the
points of the loops of the complete meridian curve remaining at
the same distance from the axis of revolution, the loops them-
selves may assume very different forms.

On examining these variations and their limits, I arrive at the
following results:

In a first mode of variation, if we suppose, for simplicity, that
the distance between the axes of symmetry of the loops remains
constant, these loops become smaller and smaller, their vertices
approach the axis of revolution, and at the same time the curva-
ture of the intermediate arcs approaches more and more the
circular phase, ultimately the loops vanish, the points which form
their vertices attain the axis of revolution, and the arcs which
connected them become semicircles, so that the generated figure
is an indefinite series of equal spheres touching each other on the
axis. We have seen that this series of spheres is also one of
the limits of the variations of the unduloid; it forms therefore
the phase of transition from the unduloid to the nodoid.

In a second mode of variation, if, for simplicity, we conceive
the length of the loops—that is to say the distance from the
vertex to the point of each—to remain constant, the curve
recedes more and more from the axis of revolution, or rather,
since attention is directed to the latter, the axis recedes from
the curve; at the same time the loops become broader, approach,
and afterwards project over each other, until finally, when the
axis of revolution is at an infinite distance, and therefore ceases
to exist, the whole meridian curve becomes condensed into the
circumference of one circle, and the generated figure is a cylinder
placed transversely with respect to the axes of revolution of the
nodoids of which it is the limit.

In a last mode of variation, if—again for simplification—we
attribute a constant value to the distance between the vertex of
a loop and the axis of revolution, these loops become elongated
whilst they recede from each other; so that if we consider any
one in particular and pass to the limit, all the rest, together
with the intermediate arcs, are transported to infinity, or, in
other words, disappear; and this one becomes transformed into a
curve with two infinite branches, which is no other than the
catenoid placed in the manner before described; hence the
limit of this third mode of variation is the catenoid. Thus the
catenoid, itself one of the limits of the unduloid, forms a second
phase of transition between the unduloid and the nodoid.

The experiments indicated in this abstract, together with
others described in the memoir, exhaust all the cases of realization
between two equal rings or discs; if there is still another
equilibrium-figure, therefore, it must be such that isolated portions
of the same cannot be obtained under these conditions; from which fact we may conclude that its meridian curve presents no maxima or minima of distance from the axis. Moreover, since the meridian curve cannot cut the axis, it must commence from a point at infinity upon an asymptote parallel to the axis, and afterwards recede continually from the latter until it reaches a second point also at infinity. This granted, it may be observed that, at the first of these two extreme points, the radius of curvature is necessarily infinite and the normal finite,
so that the equation of equilibrium reduces itself to \( \frac{1}{N} = C \).

Now if the curve had anywhere a point of inflection, this equation
would again be reduced to \( \frac{1}{N} = C \), and consequently the
normals corresponding, respectively, to this and to the first extreme point would be equal, which is clearly impossible. The curve, therefore, if it exists, must everywhere turn its convexity towards the axis, and hence we see without difficulty that its second extreme point will be infinitely distant from this axis, and that the normal will be there infinite. But, evidently, the radius of curvature will also be infinite at this second extreme
point, so that the quantity \( \frac{1}{M} + \frac{1}{N} \) will be there equal to zero,
which is impossible, since at the first extreme point this quantity has a finite value. The condition of equilibrium, therefore, cannot be satisfied.

Thus the only equilibrium-figures of revolution are the sphere,
the plane, the cylinder, the unduloid, the catenoid and the
nodoid.

All these figures, the sphere excepted, having infinite dimensions in certain directions, it follows that amongst the equilibrium-figures of revolution the sphere alone can be realized, in its complete state, with a finite mass of liquid. Accordingly a mass of oil, abandoned freely in the middle of my alcoholic mixture, always assumes the spherical form.

IV. On the Atlantic Cable. By William Gravatt, Esq., F.R.S., F.R.A.S.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I CONSIDERED this subject, and, as is well known, arrived at my conclusions long ago; and notwithstanding the conclusions I see were come to at the last meeting of the British Association and at other meetings, and notwithstanding the Astronomer Royal’s two papers on the subject, I feel that my original conclusions are unshaken.

The subject is of great importance, and I will (if you will allow me) give you as brief an abstract of my ideas as I possibly can.

I have demonstrated (but without a diagram it is almost impossible to give the demonstration) that if a vessel be running in a straight course at any uniform speed whatever, and the cable is being paid out over the stern at the same velocity, then, in the case where the strain tending to break the cable as it leaves the stern is just equal to the weight in water of a portion of the same cable, whose length is the depth of the sea, the cable will be at any instant in the same condition as to tension and pressure as if it were simply lying straight on an inclined plane surface immersed in water; there will be no strain whatever on the particle which happens at any instant to be the bottom particle of the cable, and the cable will therefore sink down in a vertical plane with uniform velocity, and be laid (not stretched) straight along the bottom of the sea without any waste.

Let

\[ \alpha = \text{the angle the cable forms with the horizon.} \]
\[ t = \text{the terminal normal velocity in feet per second, or the velocity of the cable at right angles to its length.} \]
\[ V = \text{the velocity of the ship in feet per second.} \]
\[ w = \text{the weight in pounds of one foot of the cable, in water.} \]
\[ d = \text{diameter of the cable in feet.} \]
\[ D = \text{the depth of the sea in fathoms.} \]
\[ L = \text{length in fathoms, from the stern of the ship to where it touches the bottom of the sea.} \]

Now the resistance expressed in pounds to each foot in length
of a rough cylinder moving sideways in sea water is \(0.8d^2v^2\) pretty nearly (\(d\) being the diameter in feet, and \(v\) the velocity in feet per second).

We must have therefore \(w \cos \alpha - 0.8d^2 = 0\), or \(t = \sqrt{\frac{w \cos \alpha}{0.8d}}\).

We must also have \(V = \frac{t}{\sin \alpha} = \frac{1}{\sin \alpha} \sqrt{\frac{w \cos \alpha}{0.8d}}\), and also \(L = D \cosec \alpha\).

Now, taking as an example \(w = 32\) and \(d = 0.05\), as is pretty nearly the case in the present Atlantic cable, we get

\[
\log t = \frac{1}{3} (\log 8 + \log \cos \alpha),
\]

\[
\log V = \frac{1}{2} (\log 8 + \log \cos \alpha) - \log \sin \alpha.
\]

From whence, taking \(\alpha = \) successively 10°, 20°, &c., we get immediately the following Table, which we can easily extend:

<table>
<thead>
<tr>
<th>Values of (\alpha)</th>
<th>Velocity of ship in feet per second</th>
<th>Velocity of ship in fathoms per hour</th>
<th>Values of (L/D) in taken unity</th>
<th>Terminal normal velocity, in feet per second, or values of (\frac{t}{L/D})</th>
<th>Tension in pounds on leaving the ship, the pounds of (D) in fathoms at 2 lbs. to the fathom.</th>
</tr>
</thead>
<tbody>
<tr>
<td>10°</td>
<td>16.18</td>
<td>9.71</td>
<td>5.76</td>
<td>2.31</td>
<td>2D</td>
</tr>
<tr>
<td>20°</td>
<td>8.92</td>
<td>4.81</td>
<td>2.92</td>
<td>1.74</td>
<td>2D</td>
</tr>
<tr>
<td>30°</td>
<td>5.26</td>
<td>3.16</td>
<td>2.00</td>
<td>2.68</td>
<td>2D</td>
</tr>
<tr>
<td>40°</td>
<td>3.85</td>
<td>2.31</td>
<td>1.56</td>
<td>2.48</td>
<td>2D</td>
</tr>
<tr>
<td>50°</td>
<td>2.96</td>
<td>1.78</td>
<td>1.30</td>
<td>2.27</td>
<td>2D</td>
</tr>
<tr>
<td>60°</td>
<td>2.31</td>
<td>1.39</td>
<td>1.15</td>
<td>2.00</td>
<td>2D</td>
</tr>
</tbody>
</table>

*Note.—The slip is obviously \(V\) vers \(\alpha\) in feet per second, giving a resistance (as will be afterwards shown) of \(0.012\) (\(0.06\) vers \(\alpha\)) pounds per fathom, or \(0.012\) (\(0.06\) vers \(\alpha\)) \(\frac{1}{18}\) L pounds on the whole, which ought in strictness to be subtracted from the weight of \(D\), but which is too small a quantity to be noticed in practice, at least in any cable similar to this.*

Now, taking the greatest depth of the sea to which the cable in question is to be exposed at 2000 fathoms, and each fathom to weigh in water \(6 \times 32 = 192\) lb., say 2 lbs. per fathom, we have 4000 lbs. for the strain on the cable, whatever may be the velocity of the ship; or as the cable is said to break with about \(4\frac{1}{4}\) tons or 9500 lbs., the greatest necessary strain in smooth water is about four-tenths the breaking weight.

But if the cable be paid out at a greater velocity than the horizontal velocity of the ship, the cable will be (so to speak) slipping down the supposed inclined plane, and we must, in that case, find the resistance due to that slipping velocity.

Now this resistance is not exactly as the square of the velocity, nor is it indeed exactly as the diameter of the cable.
Upon the whole, I consider from a comparison of experiments, principally those of Colonel Beaufoy, that when the velocity in knots per hour is $v$, the resistance in pounds per fathom will, for the cable here supposed, be nearly enough represented by $0.012v^{1.8}$.

For the sake of brevity I shall take only the example of this cable at an angle of $20^\circ$, where the velocity is, we see, 4.8 knots per hour. Now, taking the depth of the sea at 2000 fathoms, we have $L = D \csc \alpha = 2000 \times 2.92 = 5840$ fathoms.

Taking, for example, the slipping velocity of the cable equal to the velocity of the ship, or paying out twice the necessary length of cable, we find for a slipping velocity of 4.8 knots per hour, a resistance of about 2 lb. per fathom, or the total slipping resistance of the cable 1168 lbs., by which amount the strain on the cable at the ship’s stern will be reduced, or the greatest strain will be $4000 - 1168 = 2832$ lbs., or to reduce the strain little more than one-fourth, we should at this velocity have to waste half the cable. Again, a waste of 1 knot of cable per hour, or a little more than 20 per cent. of the cable, would only reduce the strain about 70 lbs.

From the above, it appears that we are not able materially to reduce the strain upon the cable without occasioning excessive waste, unless at ‘Leviathan’ speed.

Now if, on the other hand, we increased the tension of the cable beyond that due to the simple depth of the sea, the cable would form a curve concave to the horizon, and we might find the form of this curve when the strain was 2D, 3D, or any multiple whatever of D; but it would be such very bad engineering to stretch or strain a cable over the bottom of the sea, when we ought, on the other hand, really to allow waste (according to circumstances) to sink into its irregularities, that although I have considered this state of the cable, I think it practically necessary to observe only, that if from any accident the paying out of the cable is obliged to be discontinued, then, in order to avoid a catenarian strain that might be fatal, the motion of the ship should instantly be reversed, and if in very deep water she should run back until the cable hung vertically, or nearly so, a distance which is expressed by $D - L \text{ vers } \alpha$ or $D\left(1 - \frac{\text{vers } \alpha}{\sin \alpha}\right)$.

A cross current could only curve the free part of the cable sideways without adding to the strain, or occasioning waste, as we readily see if we recur to the idea of the cable lying upon the supposed inclined plane surface; for although we made it curve in any manner by pressing against it sideways, it would, as before, be prevented from slipping down by the weight of a portion of the same cable, whose length was the perpendicular height of the plane.
I may just remark, that if the ship moved with an accelerated velocity, the cable would form a curve with the convexity upwards, and on the other hand, if she moved with a retarded velocity, the cable would form a curve with the convexity downwards.

With a brake-apparatus properly contrived and worked by practised seamen, the sending and pitching of the vessel in moderate weather ought not materially to increase the strain on the cable, and the nearer the brake-apparatus can be made to approach in its action that of (that exquisitely delicate and simple apparatus) the fly-rod and winch when in the hand of a fisherman, the more perfect it will be.

Whilst for very great depths it is very proper to consider the means we have, or may find out, of forming efficient cables of small specific gravity, yet, on the other hand, we ought not to forget that in steel we have a material whose modulus of tension is 6700 fathoms, whilst for iron wire the modulus is only 4000 fathoms.

The subject is very interesting, and I could go on much further, but that I fear I have already trespassed too much on your valuable space, and so with hearty good wishes for the success of the undertaking,

I remain, Gentlemen,
Your very obedient Servant,

15 Park Street, Westminster, S.W.,

William Gravatt.

June 15, 1858.

V. Remarks on the Doctrine of Equivalents.
By William Odling, M.B., F.C.S.*

The theory of polyatomic radicals appears to depend for its due appreciation upon a mode of viewing the doctrine of equivalents, suggested in one form by Gerhardt some ten years ago, and since modified considerably by the researches of chemists belonging to his school. At the present moment, when the polyatomic radical theory is acquiring such great prominence, a re-enunciation of these hitherto somewhat neglected views does not seem uncalled for. I propose briefly to explain these views, to illustrate their bearings upon the polyatomic radical theory, and to contrast them, not only with those which prevailed among the older chemists, but also with those maintained by Laurent and Gerhardt.

The word equivalent was originally employed by Wollaston as a substitute for Dalton’s word atom. Wollaston’s notion of equivalency appears to have been derived chiefly from the phæno-

* Communicated by the Author.
Dr. W. Odling on the Doctrine of Equivalents.

mena of what was then considered chemical combination, as instanced more particularly in the case of neutralization. Thus, a given proportion of potash united with an equivalent quantity of oxalic acid to form a neutral salt, and with twice its equivalent to form an acid salt, and so forth. But this idea of equivalency, which accords much the same signification to the words atom and equivalent, has been greatly called in question. The controversy was begun by Gerhardt and Laurent, who first clearly established the difference between the atomic weight of a body and its equivalent value. Subsequently Williamson and I have published papers on the subject; but it is only within these recent days of polyatomic radicals, that the distinction has begun to receive the general recognition which it merits, as a fundamental doctrine in chemical philosophy.

We know that no two substances in nature are absolutely equivalent, but that those are most nearly equivalent which manifest the greatest resemblance as regards some special function taken as the test of comparison; and that the prime indication of equivalency between any two or more substances consists in their capability of being exchanged for one another. Hence we look for typical illustrations of chemical equivalency, not so much among dissimilar bodies which can neutralize one another, as among similar bodies which can replace one another. The idea of direct or indirect substitution forms the basis of the modern doctrine of chemical equivalents. Thus we find in a great number of compounds, that 1 part of hydrogen may be replaced directly by 23 parts of sodium, or by 39 parts of potassium, or by 32·5 parts of zinc, or by 55·7 parts of cadmium, &c.; and as the resulting compounds manifest a marked mutual resemblance of constitution and properties, and a general resemblance to the hydrogenized body from which they were obtained, we say that the atoms of sodium, potassium, zinc, and cadmium are equivalent to one another, and also equivalent to an atom of hydrogen. Similarly, we find in a great number of compounds, both organic and inorganic, that one or more atoms of hydrogen may be replaced by an equal number of atoms of chlorine, or bromine, or iodine; and as the resulting compounds correspond closely with one another, and present a marked similitude to the original hydrogenized body, we say that the atoms of chlorine, bromine, and iodine are equivalent to one another, and also equivalent to an atom of hydrogen, whence it follows that the atoms of chlorine, bromine, iodine, hydrogen, potassium, sodium, zinc, cadmium, &c., are mutually equivalent. We are acquainted with numerous bodies of comparable constitution, containing respectively an atom of chlorine or an atom of metal, thus*:

Dr. W. Odling on the Doctrine of Equivalents. 39

H H O Water.
Cl Cl O Hypochlorous anhydride.
H Cl O Hypochlorous acid.
K Cl O Hypochlorite of potash.
K H O Hydrate of potash.
K K O Oxide of potash.

H₂S Sulphhydric acid.
H₂S² Bisulphide of hydrogen.
Fe²S Sulphide of iron.
Fe²S² Bisulphide of iron.
Cl² S Sulphide of chlorine (chloride of sulphur).
Cl²S² Bisulphide of chlorine (dichloride of sulphur).

C²H₅Cl Chlor-ethyle.
C²H₅H Hydr-ethyle.
C²H₅Zn Zinc-ethyle.
Cl²Sb Chloro-stibamine.
H²Sb Stibamine.
Ag₅Sb Stibio-silver.

That we are not acquainted with a very great number of such instances of replacement, appears to arise from the circumstance that, in few hydrogenized bodies, can the hydrogen be replaced with equal facility by chlorine and by potassium. As a general rule, we find that the facility of replacement by potassium is directly, by chlorine inversely, as the degree of oxidation of the compound under examination, and consequently that the same body does not always afford both chlorine and metal derivatives. Chlorine and potassium are two elements possessing in the highest degree those opposite properties which we denominate chlorous and basylous respectively. They are, however, connected by a great number of intermediate links, and constitute, as it were, the extremities of a very long chain. From the antagonism in properties possessed by chlorous and basylous elements, we find that the correspondence in properties between chlorous and basylous derivatives of the same hydrogenized body is confined within narrow limits; it is, however, manifested in the phenomena of double decomposition, thus*:

\[ C₇H₆O²⁺ + HKO = C₇H₅KO²⁺ + H₂O \]
\[ C₇H₆O²⁺ + HClO = C₇H₅ClO²⁺ + H₂O. \]

The chloro-benzoic acid produced is not uniform in composition.

In the instances of equivalent substitution hitherto adduced, the replacement has been effected by interchange of atom for atom. But we frequently find that the atoms of different elements have different representative values; and moreover,

* A very excellent illustration of this relation has recently been afforded by Hofmann, thus:

\[ 3EtZn + PCl³ = 3ZnCl + PEr³ \]
\[ 3EtBr + PHg³⁺ = 3HgBr + PEr³. \]
that an atom of the same element under different conditions has several different representative values. Thus, if we take three atoms of chlorhydric acid, \( H^3 Cl^3 \), we can replace the three atoms of hydrogen by three atoms of potassium, to obtain three atoms of chloride of potassium, \( K^3 Cl^3 \). But in three atoms of chlorhydric acid, \( H^3 Cl^3 \), we can also replace the three atoms of hydrogen by one atom of bismuth, and thereby obtain the body \( Bi Cl^3 \), or terchloride of bismuth; whence it is evident that the atom of bismuth is equivalent to, or is the representative of, three atoms of hydrogen or of potassium; and we find in general that the different elements have different replaceable values or equivalents. Taking the atom of hydrogen as the unit of equivalency, the atom of bismuth has a threefold equivalency. To facilitate comparison, I proposed some few years back, that mode of indicating differences in equivalent value which has now come into very general use, namely, by the use of one or more dashes placed to the right or left of the symbol, so as to establish a difference to the eye between the atom of a body and its equivalent or substitution value, as shown in the following formulae:—

\[
\begin{align*}
\text{Ag}^{13} & \text{PO}^4 \quad \text{Phosphate of silver.} \\
\text{Bi}^{111} & \text{PO}^4 \quad \text{Phosphate of bismuth.}
\end{align*}
\]

It not unfrequently happens that the same element has more than one equivalent. For instance, an atom of platinum, with the atomic weight 98.5, sometimes replaces one, sometimes two atoms of hydrogen. Thus we have the compounds—

- Platinous chloride \( \text{Pt}^t \text{Cl} \) analogous to \( \text{HCl} \).
- Platinic chloride \( \text{Pt}^{11} \text{Cl}^2 \) analogous to \( \text{H}^2 \text{Cl}^2 \).
- Platosamine \( \text{NH}^2 \text{Pt}^t \) analogous to \( \text{NH}^3 \).
- Platinamine \( \text{NH} \text{Pt}^{11} \) analogous to \( 2\text{NH}^3 \).
- Diplatosamine \( \text{N}^2 \text{H}^5 \text{Pt}^t \) analogous to \( 2\text{NH}^3 \).
- Diplatinamine \( \text{N}^2 \text{H}^4 \text{Pt}^{11} \) analogous to \( 2\text{NH}^3 \).

Again, the atom of iron, with the atomic weight 28, is sometimes equivalent to one, sometimes to \( \frac{3}{2} \) of an atom of hydrogen, thus:—

- Ferrous chloride \( \text{Fe}^t \text{Cl} \) analogous to \( \text{HCl} \).
- Ferric chloride \( \text{Fe}^{111} \text{Cl}^3 \) analogous to \( \text{H}^3 \text{Cl}^3 \).
- Ferro-sulphate of potash \( \text{Fe}^t \text{K}^t \text{SO}^4 \) analogous to \( \text{H}^2 \text{SO}^4 \).
- Iron-alum \( \text{FFer}^{111} \text{K}^t \text{S}^2\text{O}^8 \) analogous to \( 2\text{H}^2 \text{SO}^4 \).

And similarly with several other metals.

As a further illustration of equivalents, three atoms of hydrogen may be equally well replaced by

\[
\begin{align*}
\text{Three atoms of potassium} & \quad \text{K}^3 \\
\text{One atom of potassium + one atom of platinum} & \quad \text{K}^t \text{Pt}^{11} \\
\text{Two atoms of iron (ferricum)} & \quad \text{FFer}^{111} \\
\text{One atom of bismuth} & \quad \text{Bi}^{111} \\
\text{Six atoms of mercury (dimercurosum)} & \quad \text{Hg}^6
\end{align*}
\]
In the above formulæ, despite the different numbers of the atoms, the same equivalent or exponential value is maintained throughout. Equivalency is quite independent of an equality of atoms, but relates solely to functions and relations. When the same body has several functions, it is likewise possessed of several equivalents. The majority of chemists have heretofore confounded the equivalent value of a body with its atomic weight, its equivalent with its atom, which is, I conceive, unwarrantable. Thus K represents an atom or proportion of potassium, and KH SO\textsubscript{4} an atom or proportion of acid sulphate of potash; but there is no equivalency between the two bodies. On the other hand, Bi\textsuperscript{3+} represents three atoms of potassium, and Bi\textsuperscript{11} one atom of bismuth, but the two formulæ correspond to the same equivalent or representative value. In order to have equivalency, we must have some analogy in constitution or properties, to a definite standard of comparison.

But our idea of equivalency is not by any means confined to elementary bodies. It is applicable to all the varieties of compounds, real or hypothetical, with which chemists are acquainted, and more particularly to those groupings which are denominated compound radicals. Whether or not we admit the independent existence of preformed compound radicals, it is indisputable that certain groupings of atoms are capable of being transferred from one combination to another in exchange for an atom of hydrogen, or chlorine, or metal; or in other words, are capable of replacing hydrogen, chlorine, or metal, by equivalent substitution. Thus we can transfer the molecular groupings that we denominate cyanogen, CN; amidogen, NH\textsubscript{2}; peroxide of hydrogen, HO; benzoyle, C\textsubscript{7}H\textsubscript{5}O; othyle, C\textsubscript{2}H\textsubscript{3}O; ethyle, C\textsubscript{2}H\textsubscript{5}; amyle, C\textsubscript{3}H\textsubscript{11}, &c., from one compound to another, in exchange for one another, or for hydrogen, or its representatives, with a facility by no means inferior to that with which we effect actual elementary substitutions. For a long time past these groupings have been looked upon as the representatives or equivalents of one atom of hydrogen, but it is only of late years that there has been recognized among compound radicals that multifold equivalency, which forms the basis of the so-called polyatomic radical theory, and which has recently been so much employed by Hofmann and Cahours, Wurtz, Berthelot, and others, as the exponent of many brilliant researches.

The idea of mult-equivalent or polyatomic radicals was, I believe, first announced by Williamson in a paper "On the Constitution of Salts," published in the year 1851. Some time afterwards he found a disciple in myself, who, in 1854 and 1855, extended his original notion, illustrated it by a variety of formulæ, and, moreover, rendered it more precise by apply-
Analogues of the double atom of water,
\[ \text{H}^2 \text{H}^2 \text{O}^2 \].

Analogues of the treble atom of water,
\[ \text{H}^2 \text{H}^3 \text{O}^3 \].

Analogues of ammonia, \( \text{H}^2 \text{H} \text{N} \).

Analogues of the double atom of ammonia, \( \text{H}^2 \text{H}^4 \text{N}^2 \).

There is a point of considerable interest connected with the

* Williamson, ‘Chemical Gazette,’ 1851.
† Odling, ‘Journal of Chemical Society,’ 1855.
‡ Notes to Cavendish Society’s Translation of Laurent’s Méthode de Chimie, 1855.
§ Journal of Royal Institution, 1855.
Dr. W. Odling on the Doctrine of Equivalents.

43

Doctrine of mult-equivalent radicals, to which I believe I was the first to direct attention some three years ago*. In a lecture delivered at the Royal Institution, I contended that certain chemical compounds, more particularly the hydrocarbons, did not each contain some one special, pre-formed, self-existent, compound radical, but might be considered to contain exactly as many compound radicals as they contained atoms of hydrogen; and that the abstraction of one atom of hydrogen left a monobasic radical, of two atoms of hydrogen a bibasic radical, and of three atoms of hydrogen a tribasic radical, and so on. I showed that marsh-gas contained a monobasic radical, methyle; a bibasic radical, methylene; a tribasic radical, formyle; and a quadribasic radical, carbon; as instanced by its chloro-derivatives, the chloride of methyle, the dichloride of methylene, the trichloride of formyle, and the tetrachloride (dichloride) of carbon; and I contended that in "benzine there was no more reason for believing in the existence of the monobasic radical, phenyle, C₆H₅, than there was for believing in the bibasic and tribasic radicals C₆H⁴ and C₆H³ respectively," as illustrated in the following Tables:

Muriatic acid HCl

<table>
<thead>
<tr>
<th>Water</th>
<th>H₂O</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Eurhyzene).</td>
<td></td>
</tr>
</tbody>
</table>

Ammonia    | H₂N |
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>(Amidogen).</td>
<td></td>
</tr>
</tbody>
</table>

Marsh-gas  | H₃CH |
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>(Methyle).</td>
<td></td>
</tr>
</tbody>
</table>

And in reference to the phenine derivatives—

<table>
<thead>
<tr>
<th>H₂Cl</th>
<th>PtCl₂</th>
</tr>
</thead>
<tbody>
<tr>
<td>HgCl</td>
<td>PtAd²</td>
</tr>
<tr>
<td>AuCl³</td>
<td>AuAd³</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>C₆H₅. H</th>
<th>C₆H⁴. H²</th>
<th>C₆H³. H³</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Benzine).</td>
<td>(Benzine).</td>
<td>(Benzine).</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>C₆H₅. Cl</th>
<th>C₆H₄. Ad²</th>
<th>C₆H³. Cl³</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Chloride of Phenyle).</td>
<td>(Semi-benzidine).</td>
<td>(Trichloro-benzine).</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>C₆H₅. Br</th>
<th>C₆H⁴. X²</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Bromide of Phenyle).</td>
<td>(Nitro-benzine).</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>C₆H₅. Ad</th>
<th>C₆H⁴. XAd</th>
<th>C₆H³. AdX²</th>
</tr>
</thead>
</table>

<table>
<thead>
<tr>
<th>C₆H₅. X</th>
<th>C₆H⁴. ClAd</th>
<th>C₆H³. X²Cl</th>
</tr>
</thead>
</table>

For my own part I did not then, and do not now, attach any great importance to compound radicals, whether monoatomic or polyatomic, as representatives of the actual constitution of

bodies, but simply as useful means of manifesting their relations and modes of formation.

Kekulé has recently published a paper in which these views are brought forward more definitely, and with an amount of illustration heretofore not possible. He has shown how the abstraction of hydrogen increases the basicity of the radical, and how compounds containing mult-equivalent radicals may be rationally represented by several formulæ. Semi-benzidine, for instance, may be represented either as a di-ammonia, in which two atoms of hydrogen are replaced by the bi-equivalent radical \((\text{C}^6 \text{H}^4)^{\prime\prime}\), or else as benzine, in which two of hydrogen are replaced by two of amidogen, &c. *

The circumstance that the single atoms of certain elements and groupings have each the property of replacing two, three, or more atoms of hydrogen by equivalent substitution, that they are, in fact, mult-equivalent, is now, I believe, generally admitted. But, prior to the recognition of mult-equivalent radicals, Gerhardt, who first pointed out the difference between the atomic weight and equivalent of an element, contrived to make all atomic weights correspond with the hydrogen equivalent, by according to several of the elements two distinct atomic weights. Thus, for iron we had two atoms, ferrosium = 28, and ferricium = 18 7; for platinum we had two atoms, platinosum = 98 5, and platinicum = 49 2; for bismuth, antimony, and aluminium we had bismithicium, antimonicum, and aluminicum respectively. Gerhardt certainly employed ferricum and its congeners in an atomic sense, and Laurent distinctly argued that they had every right to be considered as atoms. Each of these atoms was respectively equivalent to an atom of hydrogen, and was represented by its own particular symbol, as illustrated in the following chlorides:—

\[
\begin{align*}
\text{Fe Cl} & \quad \text{ferrous.} \\
\text{fe Cl} & \quad \text{ferric (}\text{FFe}^{\prime\prime} \text{Cl}^3 = 3 \text{ fe Cl}) . \\
\text{Pt Cl} & \quad \text{platinous.} \\
\text{pt Cl} & \quad \text{platinic (}\text{Pt}^{\prime\prime} \text{Cl}^2 = 2 \text{ pt Cl}). \\
\text{sb Cl} & \quad \text{antimonic} \\
\text{bi Cl} & \quad \text{bismuthic} \\
\text{al Cl} & \quad \text{aluminic (}\text{AAI}^{\prime\prime} \text{Cl}_3 = 3 \text{ al Cl}).
\end{align*}
\]

These formulæ are, I conceive, perfectly admissible, and occasionally very commendable, if used merely to represent equivalent quantities; but if used to represent the atoms of the elements, or the smallest indivisible proportions of the elements which can enter into combinations, they are, I conceive, incorrect. From my acquaintance with Gerhardt, I believe that latterly, after he had begun to employ mult-equivalent radicles, he was

* Annalen der Chemie und Pharmacie, civ. 129, 1857.
clearly disposed to regard his symbols as mere expressions of equivalent quantities, not of atomic weights. But inasmuch as a writer* in this country has recently advocated the use of Gerhardt's equivalents in what I conceive to be their incorrect sense, it may be worth while to state the objections. It is evident that the iron in ferric salts is in a different state from the iron in ferrous salts, and that we may call the former ferricum, and the latter ferrosum; but what I maintain is, that the atom of ferricum, the smallest indivisible proportion of ferricum that can enter into a combination, is twice as heavy as the atom of ferrosum, and not two-thirds as heavy. Similarly, the smallest proportion of platinicum that can enter into a combination has the same weight, and not one-half the weight of an atom of platino- sum. By reducing all chlorides and oxides to protochlorides and protoxides, we ignore the following facts:—

1. That there is a great distinction in properties between protosalts and sesquisalts, or bisalts. Aluminium, for instance, forms but one set of salts; these might very well be considered as protosalts, but they are not so considered, simply because their properties are altogether different from those of protosalts.

2. That the analogy which binds bismuth to antimony, to arsenic, to phosphorus, and finally to nitrogen, must forbid the trisection of the atomic weights of bismuth and antimony, unless accompanied by a trisection of those of phosphorus and nitrogen.

3. That in well-defined molecules we rarely find so small a quantity of platinicum, or of ferricum, as corresponds with Gerhardt's atomic weights, but twice the former, and three times the latter quantity, thus:—

<table>
<thead>
<tr>
<th>Chemical Formula</th>
<th>Atomic Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>H Cl . NH pt²</td>
<td></td>
</tr>
<tr>
<td>H Cl . N² H⁴ pt²</td>
<td></td>
</tr>
<tr>
<td>NH₄ Cl . pt² Cl²</td>
<td></td>
</tr>
<tr>
<td>Fe³ K(SO₄)²</td>
<td></td>
</tr>
<tr>
<td>Fe³ O²</td>
<td></td>
</tr>
</tbody>
</table>

4. That twice pt, and three times fe, cr, al, &c., are usually the smallest quantities of pt and fe, &c. that can result from, or effect, those reactions in which the reacting quantities are determined by the amount of some other element concerned, thus:—

\[ K² O \cdot Cr² O³ + 8 HI Cl = 2 K Cl + 3 cr Cl + 4 HI² O + 3 Cl. \]

VI. On the Nature of Ozone. By R. Clausius†.

In the January Number of the Philosophical Magazine (p. 24), there is a communication by Schönbein, in which the discoverer of ozone describes a new property of this remarkable

* Griffin's Radical Theory in Chemistry.
† Translated by Dr. F. Guthrie, from Poggendorff's Annalen for May.
Prof. Clausius on the Nature of Ozone.

modification of oxygen. While, namely, ozone exerts a strong oxidizing action upon oxidizable bodies, and even upon the noble metals, nevertheless Schönbëin has found that strips of paper, coloured with peroxide of lead, become bleached when introduced in a moist condition into strongly ozonified air, owing to the reduction of the peroxide into the protoxide of lead. Such reduction Schönbëin has shown in another place to be accompanied with a destruction of the ozone, that is, its conversion into ordinary oxygen.

This communication confirmed me in the view which I had previously entertained concerning the nature of ozone; and I therefore trust I may be allowed now to bring forward my view as a hypothesis.

In my paper "On the Nature of the Motion which we call Heat*," I have endeavoured to explain the relations which exist between the volumes of simple and compound gases by the assumption that in simple gases, as well as in compound ones, more than one atom are combined together to form a molecule; that, for instance, a molecule of oxygen consists of two atoms†. I believe now that, under peculiar circumstances, it may happen that a small portion of the great number of molecules which exist in a given quantity of oxygen may be decomposed into two atoms, which then distribute themselves in the separate state among the remaining undecomposed molecules. These isolated

† Appended to an abstract of this paper in the Annales de Chimie et de Physique, and to a translation of it in the Archives des Sciences Phys. et Nat., are some remarks by MM. Verdet and Marignac, pointing out that the idea that even in simple gases the molecules are composed of more than one atom, had been already advanced by Dumas, Laurent and Gerhardt; a fact with which I was not then acquainted. Gerhardt has developed his theoretical views principally in the second half of the fourth volume of his "Organic Chemistry." These portions of his work were not yet published in the German translation by Wagner when my paper appeared; indeed, the last sections are not published. Inasmuch, therefore, as the French original is not at my command, I have hitherto been only able to read the first part of Gerhardt's theoretical views. I have there certainly found that Gerhardt has, from totally different, purely chemical considerations, arrived at the conclusion, that free hydrogen and free chlorine should be viewed as hydride of hydrogen and chloride of chlorine (H, H and Cl, Cl). Also of oxygen it is said, that a molecule consists of several (at least two) atoms (vol. iv. p. 612). I have not yet been able to obtain the works of Dumas and Laurent, but I do not doubt that they contain similar ideas. It has been very gratifying to me to have been made acquainted with the existence of these theoretical views by the above-mentioned remarks. For although it follows that my hypothesis is not so original as I imagined, yet I can feel nothing but pleasure in seeing my ideas thus coincide with those of such excellent investigators; especially on a subject which, on the one hand, extends into so many provinces of science, that the same result may be arrived at by many different ways, and which, on the other hand, is still of so hypothetical a nature that every confirmation of a result is desirable.
oxygen atoms, which, in their relation to foreign bodies, must naturally differ from those atoms which are combined, two and two, to molecules, are, in my opinion, ozone.

Let us now consider, in the first place, the most important modes of formation of ozone.

If electricity passes out into oxygen or atmospheric air, or if electric sparks are discharged through either of these gases, ozone is formed; and this formation is independent of the nature of the electricity, that is, whether it be positive or negative. This action may probably be attributed simply to the repulsive power of the electricity, by virtue of which the two atoms of a molecule, being charged with the same kind of electricity, are driven apart in the same manner as is observed with larger bodies.

Oxygen, when separated from its combinations by electrolysis, under favourable circumstances is obtained in an ozonified state. This is explained thus:—at the moment of disengagement the atoms of oxygen are separate. Most of them combine immediately upon the electrode, two and two together, to molecules; and here perhaps the electrode itself, when, for instance, it is formed of platinum, exerts an auxiliary action. A small portion of the atoms, however, remain in the separate condition, and this constitutes the ozone with which the oxygen is mixed.

Finally, a third mode of formation occurs when atmospheric air is in contact with moist phosphorus. This process may perhaps be imagined to proceed as follows:—As the phosphorus combines with the surrounding oxygen, a number of oxygen molecules, in contact with the phosphorus, must be decomposed into their two constituent atoms; and it may happen that the phosphorus does not combine with both of such atoms, but that one of them, being removed from the sphere of activity of the phosphorus through the motion caused by the heat developed, remains in the separate condition. We know from electrolysis that in the combination of heterogeneous atoms to a molecule, one part of the molecule is positively, and the other negatively electrical. This may perhaps also be the case in the combination of two homogeneous atoms, as for instance of two oxygen atoms, one of them becoming positively, and the other negatively electrical. Inasmuch, now, as, by the oxidation of the phosphorus, the oxygen doubtless enters into combination as the electronegative constituent, it may come to pass that of the two oxygen atoms, resulting from the splitting up of a molecule, the negative one is that especially retained by the phosphorus, and the positive one may be free to move away, or at least may be less hindered from doing so. Even after such an atom, in the course of its motions, and through contact with other molecules, or the walls of
the vessel, has lost its electro-positive state, thus becoming more adapted for combination with the phosphorus, yet such combination cannot take place until its motions bring it again into the sphere of action of the phosphorus.

Certain remarkable phenomena in connexion with ozonification by means of phosphorus have been observed. Thus, rarefied oxygen is more easily ozonified than denser oxygen, and oxygen mixed with hydrogen or nitrogen is more easily ozonified than when it is in the pure state. I believe that probable, or, at all events, possible explanations may be given for many of these secondary phenomena. I shall not discuss these, however, in this place.

The circumstance which was mentioned before as being probable, that, namely, in the combination of two oxygen atoms to a molecule the two atoms have opposite electrical states, may be made use of to explain certain other phenomena. The fact that the ozone, which is formed in a quantity of oxygen, does not disappear again in a short time of its own accord through the formation of molecules by the recombination of the separated atoms, may perhaps be due to the diminished tendency which such free atoms have to combine, through the loss of their electrical condition; just as oxygen, even when ozonified, may be mixed with hydrogen without combination resulting.

When ozonified oxygen is heated, the ozone is destroyed. This may perhaps be explained by supposing that the high temperature determines the combination of the separated oxygen atoms, just as it may that of oxygen with hydrogen and other oxidizable bodies.

Becquerel and Fremy have shown experimentally that the ozonification of a given quantity of oxygen by electric sparks can only be carried to a certain extent if the ozone remain mixed with the oxygen; but if the ozone be removed as soon as formed, for instance, by the oxidation of silver, the whole of the oxygen may be gradually converted into ozone. This points to the conclusion that if too many separate atoms be contained in the gas, they recombine with one another; and perhaps the electric sparks themselves may have the power of exerting the inverse effect under altered circumstances, that is, they may assist the combination of separated atoms in the same manner as they can determine the combination of oxygen and hydrogen.

Let us now consider some of the effects of ozone.

The principal action, namely the strongly oxidizing power, may be considered as self-evident, after the description of ozone given; for it is clear that separated oxygen atoms can more easily enter into combination with foreign bodies than such as are already combined with one another two and two, and which
must be first freed from such combination before they can be in a state fit for combination with other substances.

In this respect ozone is comparable with oxygen in the nascent state, with the exception that with the latter the electrical condition must be taken into account in addition; for when oxygen is evolved out of a compound in which it was electro-negative, it will for two reasons easily enter into another combination in which it has also to play an electro-negative part: first, because the atoms are still in the separate state; secondly, because they already are in the proper electrical condition. Hence oxygen in the nascent state may in many cases surpass ozone in activity.

The galvanic polarization of a plate of platinum, by immersion in ozonified oxygen, is related to the above action. It is known that the two electrodes, which serve for the galvanic electrolysis of water, become thereby polarized in such a manner as to be capable of giving rise by themselves to a current in the opposite direction. This is explained by supposing the one electrode to be covered by a layer of hydrogen, and the other with a layer of oxygen; and such explanation accords with the fact that a plate of platinum, when immersed in hydrogen, acquires thereby also a positive polarization. But if a platinum-plate be immersed in common oxygen, the corresponding phenomenon, which might perhaps be expected, namely the acquisition of negative polarity by the plate, does not occur; and this appears to contradict the above-given explanation. I imagine, however, that this difference may be accounted for as follows:—Inasmuch as a molecule of water consists of two atoms of hydrogen and one atom of oxygen, the atoms of hydrogen, which, like the atoms of oxygen, are also combined two and two to molecules, may enter into combination with oxygen without separating from one another. The atoms of oxygen, on the contrary, as long as they are combined together as molecules, are not in a suitable condition for combination with the hydrogen. Hence oxygen, in its ordinary state, is incapable of causing galvanic polarization, but acquires this power by ozonification.

Besides an oxidizing action, ozone may exert an opposite or deoxidizing one, as Schönbein has proved in the case of peroxide of lead; the ozone itself being converted thereby into ordinary oxygen. Now, as this transformation of ozone into oxygen occurs also when it is brought into contact with other peroxides, it immediately suggests itself that the deoxidation of the peroxide is also not confined exclusively to the peroxide of lead. This action may be explained without difficulty. If we imagine an oxide which readily gives up the whole, or a part of its oxygen, in contact with a gas in which separate oxygen-atoms are moving about, seeking to combine with second atoms,
these moving atoms, on coming into contact with the oxide, are able to withdraw the atoms of oxygen which are only feebly combined. This accounts at the same time for the double effect,—the reduction of the oxide and the disappearance of the ozone.

The behaviour of ozone is in many respects similar to that of the peroxides. Peroxide of hydrogen, for instance, has, as is well known, a strong oxidizing action, by reason of the facility with which it gives up its second atom of oxygen. If, on the contrary, peroxide of hydrogen be brought into contact with the oxides of the noble metals, or with certain metallic peroxides, a reciprocal reduction takes place. We may in such a case presume that the oxygen-atoms liberated from the peroxide of hydrogen combine and form molecules with those which are given off from the metallic oxides or peroxides.

In considering the above-mentioned phenomenon, the question may arise, why the atoms of ozone, or the easily separable oxygen-atoms in a single oxide or peroxide, should not unite with one another with as great a facility as the atoms of two heterogeneous substances combine together. Various secondary reasons, however, may be of influence. In the first place, the state of aggregation must be considered. In a solid metallic oxide or peroxide, the several parts are fixed in position with respect to one another; and we may therefore presume that the oxygen-atoms do not come into that contact with one another which is necessary for combination. A fluid body, on the contrary, adapts itself better upon a solid one, and its particles possess at the same time the necessary mobility. The same is the case with a gaseous body; and such a one, in addition, undergoes a condensation on the surface of the solid body. It may, moreover, be the case, that the equally electrified condition of the oxygen-atoms of a definite compound renders them less disposed to combine with one another than with the non-electric ozone, or with the oxygen-atoms of another compound, whose electrical state may possibly be a different one. Moreover, the electrical conductibility of the substance may be of influence, inasmuch as those alterations of the electrical condition which are necessary for combination, may take place more easily in contact with metallic bodies than in the interior of badly-conducting bodies. Probably still further reasons might be given in answer to the question proposed; but those already adduced may suffice, at all events, to show how numerous the influencing circumstances may be, and how vain it must be to expect to find the phenomena following some simple law which holds good in all cases.

Finally, I must remark that the density of ozone, as given by Andrews and Tait*, according to whom it would appear to be

nearly four times as great as that of ordinary oxygen, is contradictory of my hypothesis concerning the nature of ozone. If, however, we reflect that the experiments could only be tried with oxygen containing comparatively little ozone, and that, in order to convert this ozone into ordinary oxygen, the whole was heated to 230° C. or more, it is easy to perceive how extremely difficult it must have been to remove the disturbing influences so far as to attain the requisite degree of accuracy. For this reason, then, without in the least calling in question the skill and care of the experimenters, I have, nevertheless, hesitated to attach sufficient weight to the results they have found, to induce me to suppress my hypothesis.

---

VII. Proceedings of Learned Societies.

ROYAL SOCIETY.

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

June 18, 1857.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On Hourly Observations of the Magnetic Declination, made by Captain Rochfort Maguire, R.N., and the Officers of H. M. Ship 'Plover,' in 1852, 1853, and 1854, at Point Barrow, on the shores of the Polar Sea." By Major-General Edward Sabine, R.A., D.C.L., Treas. and Vice-President R.S.

Point Barrow is the northern cape of that part of the American continent which lies between Behring Strait and the Mackenzie River. It was the station, from the summer of 1852 to the summer of 1854, of H.M.S. 'Plover,' furnished with supplies of provisions, &c. for Sir John Franklin's ships, or for their crews, had they succeeded in making their way through the land-locked and ice-enumerated channel by which they sought to effect a passage from the Atlantic to the Pacific. In this most dreary, and apparently uninteresting abode, Captain Maguire and his officers happily found an occupation in observing and recording, for seventeen months unceasingly, the hourly variations of the magnetic declination and of the concomitant auroral phenomena, in a locality which is perhaps one of the most important on the globe for such investigations. Their observatory, placed on the sand of the sea-shore, was constructed of slabs of ice, and was lined throughout with seal skins. The instruments had been supplied from the Woolwich establishment, with the requisite instructions for their use, and the observations were made and recorded precisely in the same manner as those in the Colonial Magnetic Observatories. The observations were sent by Captain Maguire to the Admiralty, and were in due course transmitted to General Sabine, by whom they were subjected to the same processes of reduction as those in the colonial observatories: the results are given and discussed in this communication.
A sufficient body of the larger disturbances to permit an examination of their laws having been separated from the rest of the observations, it was found at Point Barrow, as elsewhere wherever a similar investigation has been made, that in regard to the frequency of their occurrence, and to the mean amounts of easterly and westerly deflection produced by them, the disturbances follow systematic laws depending on the hours of solar time. The laws of the easterly and of the westerly disturbances were also found, at Point Barrow as elsewhere, to be distinct and dissimilar. On further instituting a comparison between the disturbance-laws at Point Barrow and Toronto, it was found that although the laws of the deflections of the same name at the two stations did not correspond, there existed, on the other hand, a very striking and remarkable correspondence between the laws of the easterly disturbances at Point Barrow and of the westerly at Toronto, and between the laws of the westerly disturbances at Point Barrow and easterly at Toronto. The correspondence is traced in much detail, for the purpose of showing that it is manifested, not in slight and unimportant particulars, but in the most marked characteristics of both classes of phenomena. From the correspondence in the hours at which opposite disturbance-deflections prevail, it follows, that the portion of the diurnal variation which depends upon the disturbances, has opposite, or nearly opposite characteristics at the two stations.

In former papers the author considers that he has shown that, for the purpose of obtaining a correct knowledge of the phenomena of the regular solar diurnal variation, it is necessary to eliminate the influence of that portion of the observed diurnal variation which proceeds from the disturbances; and he now adduces the observations at Point Barrow as strongly confirmatory of this. When the diurnal variation is derived from the whole body of the observations at Point Barrow, retaining the disturbances, the westerly extreme of the diurnal excursion, which, as is well known, occurs generally in the extra-tropical part of the northern hemisphere at a little after 1 p.m., is found to take place at 11 p.m.; but when the larger disturbances are omitted, the westerly extreme falls at the same hour as elsewhere, viz. a little after 1 p.m. The author takes occasion to suggest the probability that the anomalies which have been supposed to exist elsewhere in the turning-hours of the solar diurnal variation in high latitudes may be susceptible of a similar explanation.

It appears, therefore, by the comparison of the Point Barrow and Toronto observations, that in the regular solar diurnal variation the progression at the two stations is similar, the easterly and westerly extremes being reached nearly at the same hours; whilst in the disturbance diurnal variation the progression is reversed, the easterly extreme at the one station coinciding very nearly with the westerly extreme at the other. This contrariety seems the more remarkable, since both variations appear to have the same primary or exciting cause, viz. the sun; inasmuch as in each the period is a solar day. The author draws the probable inference, that whilst the primary
cause is the same in both, the mode of operation is different in the two cases.

Another important distinction between the phenomena of the solar diurnal variation and of the disturbance variation at Point Barrow and Toronto, is shown by the author to consist in the comparative magnitude of their range. The increase in the range of the solar diurnal variation between Toronto and Point Barrow is, as nearly as may be, in the inverse ratio of the values of the horizontal force of the earth at the two stations (which is the antagonistic force opposing all magnetic variations); whilst on the other hand the increase in the range of the disturbance variation is many times greater than it would be according to the same proportion. It would appear therefore that the absolute disturbing force must be much greater at Point Barrow than at Toronto; suggesting the question, by what physical or other conditions is the locality distinguished at which the disturbing force is a maximum.

In correspondence with the great amount of the absolute disturbing force at Point Barrow is the frequency of the concomitant aural manifestations, which greatly exceed that of any previous record known to the author. It was the custom at Point Barrow to attach a distinguishing mark to all the hourly magnetic observations which were made when the Aurora was visible. Taking the months of December, January, and February as those in which, in the latitude of Point Barrow, there is nearly a constant absence of day-light, there were, in those three months in 1852-1853, 1788 hourly observations, at 461 of which the Aurora was visible; and, in the same three months of 1853-1854, there were 1837 hourly observations, at 616 of which the Aurora was seen. There were therefore in the six months 3625 hourly observations, at 1077 of which the Aurora was seen contemporaneously, and at 2548 it was either not present or obscured by clouds: the presence of the Aurora was thus recorded at between one-third and one-fourth of the hours of observation. Or possibly the frequency of its appearance may be more easily judged of by stating, that during these six months the Aurora was seen on six days out of every seven.

The record thus furnished by Capt. Maguire has enabled the author to treat the Aurora, for the first time, in the same way as the corresponding and connected phenomena of the magnetic disturbances are treated, viz. by distributing its recorded appearances into the several hours of their occurrence. A table, in which the particulars of this distribution are contained, shows that 1 A.M. is the hour of their most frequent occurrence at Point Barrow, there having been 102 recorded appearances in the six months at that hour. From 11 A.M. to 3 P.M. is the epoch of minimum, not a single instance of Aurora at any of those hours being recorded in the same six months. The increase from the minimum to the maximum, and the decrease from the maximum to the minimum, are both continuous progressions, with only such very slight occasional interruptions as might assuredly be expected to disappear in mean numbers taken from a longer interval of time.
Royal Society:—

When the frequency of the Aurora at the different hours is compared with the respective amounts of easterly and of westerly disturbance-deflection at the different hours at Point Barrow, a very considerable approximation towards accordance is perceived between the frequency of the Aurora and the amount of the westerly deflections; whilst, on the other hand, the auroral hours appear to have little or nothing in common with the turning-hours, or with the progression, of the easterly deflections.

November 26, 1857.—Major-General Sabine, R.A., Treas. and V.P., in the Chair.

The following communications were read:—


In pursuing the researches of which abstracts have been given in the 'Proceedings' for January 29 and June 18*, the author found that the heat evolved by compressing wood, cut either in or across the direction of the grain, was nearly that due to the application to the particular case of Professor Thomson's formula. Exact agreement could not be expected, on account of the discordant results arrived at by different experimenters on the expansion of wood. On investigating the subject, the author finds that the expansion of wood cut in the direction of the grain, is greatly influenced by the tension to which it is exposed, as well as by its humidity. A rod of well-seasoned and dried bay-wood, $\frac{3}{8}$ths of an inch in diameter, and exposed to the tension of 26 lbs., gave an expansion of $0.0000461$ per degree Centigrade, but when a weight of 426 lbs. was hung to it, its coefficient of expansion was increased to $0.0000566$. In conformity with this result, it was found that the elasticity of the rod was considerably diminished by an increase of its temperature. On investigating the effect of humidity, the author found that it occasioned a diminution in the expansibility by heat. After the rod of bay-wood with which the above experiments were made had been immersed in water until it had taken up 150 grains, making its total weight 882 grs., its expansion with a tension of 26 lbs. was found to be only $0.00000436$. Experiments with a rod of deal 33 inches long, and weighing when dried 425 grs., gave similar results. Its expansion when dry, with 26 lbs. tension, was $0.0000428$, and with 226 lbs. $0.0000438$; but when made to absorb water, its coefficient of expansion gradually decreased, until, when it weighed 874 grs., indicating an absorption of 449 grs. of water, expansion by heat ceased altogether, and, on the contrary, a contraction by heat equal to $0.00000636$ was experienced.

"On the Partitions of the $r$-Pyramid, being the first-class or $r$-gonous $x$-edra." By the Rev. T. P. Kirkman, M.A., F.R.S.

Partitions proper of the $r$-pyramid are made by drawing diagonals none crossing another in the $r$-gonal base, and diapeds (intersections of non-contiguous faces) none enclosing a space, in the $r$-edral vertex.

The object of the memoir is to enumerate the number of such partitions that can be made with $K$ diapeds in the vertex and $k$ diagonals in the bases of the pyramid. By the drawing of $k$ diagonals, the pyramid becomes a $(r+1)$-acral $(r+k+1)$-edron, which by the introduction of $K$ diapeds becomes a $(r+K+1)$-acral $(r+k+1)$-edron. Such a figure is termed an $r$-gonous $(r+K+1)$-acral $(r+k+1)$-edron of the first class. The definition of an $r$-gonous $x$-edron of the first class is that it contains a discrete $r$-gony, i.e. $K$ diapeds and $k$ diagonals of which no diaped meets a diagonal, and such that the evanescence of the $K$ diapeds will form an $r$-ace, and the evanescence of the diagonals forms an $r$-gon.

If the summits upon the $k$ diagonals be, one or more of them partitioned by $K'$ diapeds, or the faces about the $K$ diapeds be partitioned by $k'$ diagonals, there arises a mixed $r$-gony, in which are one or more angles made by a diaped and a diagonal. If such a figure has not a discrete $r$-gony as well as that mixed one, and has no $(r+r')$-gony, by the vanescence of which the $(r+r')$-pyramid can be obtained, it is an $r$-gonous $x$-edron of the second class. And $r$-gonous $x$-edra of the third class can be obtained by partitioning the faces about the $K'$ diapeds and the summits upon the $k'$ diagonals, in such a manner that no $(r+r')$-gony shall be introduced; and so on for higher classes of $r$-gonous $x$-edra.

It is proved that every partition proper of the $r$-pyramid, that is, any $(1+K)$-partitioned $r$-ace laid on a $(1+k)$-partitioned $r$-gon, is an $r$-gonous $(r+k+1)$-acral $(r+k+1)$-edron. The number of the $(1+k)$-partitions of the $r$-gon, and of the $(1+K)$-partitions of the $r$-ace is known by the formulæ given in the author's memoir "On the partitions of the $r$-gon and $r$-ace," in the Philosophical Transactions, 1857. The present memoir gives the formulæ whereby the partitions of the pyramid are determined in terms of those of the $r$-gon and $r$-ace.

Thus the entire first class of $r$-gonous $x$-edra is enumerated, without descending to any classification of polyedra according to the rank of their faces and summits. The enumeration of the second and higher classes will require such classification, which will introduce so vast a complexity as to render the further prosecution of the theory of the polyedra, in the opinion of the author, practically impossible by any method deserving the name of scientific generality.

"Researches on the Cinchona Alkaloids." By W. Bird Herapath, M.D. Lond., F.R.S.E.

In the first part of this paper, the author examines the existing tests for discriminating between the various cinchona alkaloids, and points out their insufficiency. In the present part, he shows that the optical characteristics of the iodo-sulphates of the alkaloids quinine and quinidine are sufficiently well marked to render the existence of either one of these alkaloids certain, and that although the iodo-sulphate of cinchonidine is very closely related optically and chemically to the homologous salt of quinine, yet there are sufficient points of dissimilarity to enable us to diagnose between the two; and, moreover, that the production of this salt is a beautiful means
of deciding readily whether cinchonidine is present in specimens of
chinonine or cinchonine; all evidence of quinine or its allies
having been decided in the negative by the results of the previous
tests, as proposed by Brandes, Vogel, Pelletier, Leers, or the author.
The cinchonidine of Wittstein has also, by the same method, been
proved by the author to be totally different from the cinchonidine of
Pasteur.

Acetic acid and chloroform may also be employed for discriminating
between chinonine and cinchonidine.

The chemical characters of all these iodo-salts furnish no means of
discrimination, for as a class they all agree in being more or less
soluble in spirit, giving a deep sherry-brown solution, from which
water precipitates them in an amorphous form, as dark brown, cin-
namon-brown or purplish-brown coloured precipitates; they are
only very slightly soluble in dilute spirit, and scarcely at all in
water, ether, turpentine, or chloroform: acetic, dilute sulphuric, or
hydrochloric acid have but little action upon them, whilst concen-
trated hydrochloric or sulphuric acid decomposes them. Nitric
acid rapidly acts upon them, even in the cold, with violent evolution
of nitrous acid and production of heat, iodine being oftentimes libe-
rated in the crystalline form.

Alkalis also decompose them.

Sulphuretted hydrogen, soluble sulphides, sulphurous acid and
sulphites, together with chlorine-water, instantly decolour their
alcoholic solution, with the production of hydriodic acid.

In dilute alcoholic solutions, starch gives immediate evidence of
iodine, and nitrate of silver gives a yellowish-white precipitate of iodide
of silver, and some organic basic compound which can only be re-
moved by the action of concentrated boiling nitric acid; this reac-
tion, although commencing at the ordinary temperature, with violent
disengagement of nitrous acid vapours, must be perfected by boiling.

Baryta salts exhibit the existence of sulphuric acid, which in all
instances is an essential constituent in their formation.

The quinidine and chinonine salts dissolve with more difficulty, in
consequence of their greater thickness and less extent of surface.

Since the author had the honour of communicating his discovery
of the optical salt of chinonidine to the Royal Society, he has
ascertained that its primary form is, like that of the quinine salt,
that of a right rhombic prism, and usually very thin, but having
for its acute angles 43°, and 137° for its obtuse, with the rectangular
axes \[ M^\alpha T^{\gamma} P^n \] ; \[ T^{\gamma} \] — the quantity for \( P^n \) being variable
and very minute. In a former communication to the Royal Society,
the quinine salt was shown to have a primary rhombus, having 65°
for the acute, and 113° for the obtuse angles, with the three rectan-
gular axes, thus related :-\( \frac{M^\alpha T^{\gamma}}{T^{\gamma} P^n} \) — the quantity for \( P^n \) being variable
and very minute. In both salts the optical characters are usually examined through
the shortest axis, \( P^n \); in some recent observations on the quinine
salt, the author has discovered that it transmits a blood-red beam of
plane-polarized light through the axes \( M^\alpha \) and \( T^n \), and this is also a
beam polarized in a plane parallel to that of the axes \( M^\alpha \) and \( T^n \).
TABULAR VIEW.

Both the quinine and cinchonidine salts are derivable from the primary rhombic prism, and crystallize as rhomboids and \( \beta \)-prisms which obstruct plane-polarized light, when their longer diameters \( (M^\circ) \) are parallel to the plane of the polarized ray, and \( \alpha \)-prisms which obstruct the same beam when their longer diameters \( (T^\circ) \) are perpendicular to the plane of the polarized ray.

<table>
<thead>
<tr>
<th>Quinine salt</th>
<th>Rhomb obtuse 115° ( 1.57 = M^\circ = 2.482 )</th>
<th>Cinchonidine salt</th>
<th>Rhomb 137° ( \alpha )</th>
<th>43°</th>
</tr>
</thead>
<tbody>
<tr>
<td>, acute 65°</td>
<td>( 1.00 = T^\circ = 1.000 )</td>
<td>( \cdot 00001 = P^\circ = \cdot 00001 )</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Transmitted rays or body-colours.**

1. Polarized parallel to axis \( (T^\circ) \).
   - \( P^\circ \), colourless, greenish-white, yellowish-green.
2. \( M^\circ \) and \( T^\circ \), blood-red, polarized in the plane of those axes.
3. Polarized perpendicular to axis \( (T^\circ) \).
   - \( P^\circ \), pink, ruby-red, blood-red, sienna-brown.

**Reflected rays or surface-colours.**

1. Polarized perpendicular to axis \( (T^\circ) \).
   - Cantharidine-green, blue-green, grass-green.
2. Polarized parallel to axis \( (T^\circ) \).
   - Dull olive-green, or vitreous and colourless on a dull black surface.

1. \( P^\circ \), greenish-white, yellowish-green, dark olive-green.
2. \( M^\circ \) and \( T^\circ \) not observed.
3. \( P^\circ \), violet, light blue, indigo-blue.
5. Dull olive, or vitreous and colourless on a dull black surface.
In the foregoing comparative Chart of the physical properties of the two salts, the axis has been assumed to coincide with a line drawn through the short diagonal of the primary rhombic crystal, which will coincide with the long diameter of the $\alpha$-prism, and the plane of the breadth of the $\beta$-prism, and is therefore the $T^e$ of the three rectangular crystallographic axes.

It has been compiled from the observations of Professors Stokes and Haidinger and the author. It appears to form a complete optical description of the two salts, as far as they are at present known.

Whilst in both salts the indicative body-colours, or those due to the more absorbed pencils (3), are only to be seen in the thinnest crystals, it is evident that the reflected rays may be seen indifferently in crystals of all thicknesses; and the author is inclined to believe that the cinchonidine salt possesses even greater tourmaline absorbent powers upon ordinary light, inasmuch as much thinner plates are required in order to obtain the indicative body-colours,—perfect absorption, and therefore total obstruction, being more early arrived at than in the case of the quinine salt.

The author's more recent analyses of the cinchonidine salts have produced the following results:

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>III</th>
<th>IV</th>
<th>Mean.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Iodine</td>
<td>39·727</td>
<td>39·462</td>
<td>39·246</td>
<td>38·488</td>
<td>39·478</td>
</tr>
<tr>
<td>Sulph. acid</td>
<td>8·390</td>
<td>8·673</td>
<td>8·882</td>
<td>8·593</td>
<td>8·701</td>
</tr>
<tr>
<td>Carbon</td>
<td>34·936</td>
<td>35·73</td>
<td>35·792</td>
<td>.</td>
<td>35·486</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>4·321</td>
<td>4·301</td>
<td>.</td>
<td>.</td>
<td>4·311</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>2·976</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>2·976</td>
</tr>
<tr>
<td>Oxygen</td>
<td>9·650</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>9·048</td>
</tr>
</tbody>
</table>

100·000

which lead to the following composition:

<table>
<thead>
<tr>
<th></th>
<th>Theory.</th>
<th>Mean of Experiments.</th>
</tr>
</thead>
<tbody>
<tr>
<td>57 Carbon</td>
<td>$35·367$</td>
<td>$35·486$</td>
</tr>
<tr>
<td>40 Hydrogen</td>
<td>$4·147$</td>
<td>$4·311$</td>
</tr>
<tr>
<td>2 Nitrogen</td>
<td>$2·884$</td>
<td>$2·976$</td>
</tr>
<tr>
<td>12 Oxygen</td>
<td>$10·052$</td>
<td>$9·048$</td>
</tr>
<tr>
<td>3 Iodine</td>
<td>$39·297$</td>
<td>$39·478$</td>
</tr>
<tr>
<td>2 Sulph. acid</td>
<td>$8·294$</td>
<td>$8·701$</td>
</tr>
</tbody>
</table>

967 100·000 100·000

which probably give the following formula:

$$C_{37}H_{52}N_2O_8^+ \quad \text{F} \quad 2\text{SO}_3, \Pi\Pi + 5\Pi\Pi = 967.$$  

One other remarkable difference exists between the quinine and cinchonidine salt, which is, that the optical crystals of the last salt,
Dr. Herapath on the Cinchona Alkaloids. 59

if allowed to remain in the mother-solution with an excess of less than 1 per cent. of sulphuric acid, undergo a transformation, and become long, golden, silky aciculae, radiating in beautiful globose tufts; this salt has some doubly absorbent powers also, but very feeble. When this salt is attempted to be redissolved in boiling spirit, in order to be recrystallized, it does not re-form, but the optical crystals are then produced; when the silky crystals are carefully air-dried, they retain their yellow colour, but if exposed over sulphuric acid at 62° Fahr., or if attempted to be dried at 212° Fahr., they lose 5·32 per cent. water = 6 atoms, and become a dark greenish-black residue, which is a tri-hydrate and contains the following by analysis:—

<table>
<thead>
<tr>
<th>I.</th>
<th>II.</th>
<th>III.</th>
<th>IV.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Iodine</td>
<td>40·504</td>
<td>40·407</td>
<td>..</td>
</tr>
<tr>
<td>Sulph. acid</td>
<td>9·064</td>
<td>8·324</td>
<td>..</td>
</tr>
<tr>
<td>Carbon</td>
<td>..</td>
<td>..</td>
<td>36·082</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>..</td>
<td>36·404</td>
<td>36·28</td>
</tr>
</tbody>
</table>

numbers which very closely correspond with the following:—

<table>
<thead>
<tr>
<th>Carbon</th>
<th>6</th>
<th>342</th>
<th>36·037</th>
<th>35·835</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hydrogen</td>
<td>1</td>
<td>38</td>
<td>4·004</td>
<td>4·047</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>14</td>
<td>28</td>
<td>2·950</td>
<td>2·851</td>
</tr>
<tr>
<td>Oxygen</td>
<td>8</td>
<td>80</td>
<td>8·433</td>
<td>8·063</td>
</tr>
<tr>
<td>Iodine</td>
<td>127</td>
<td>381</td>
<td>40·147</td>
<td>40·455</td>
</tr>
<tr>
<td>Sulph. acid</td>
<td>40</td>
<td>80</td>
<td>8·429</td>
<td>8·699</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Theoretical</th>
<th>Experimental</th>
</tr>
</thead>
<tbody>
<tr>
<td>949</td>
<td>100·000</td>
</tr>
<tr>
<td>100·000</td>
<td>100·000</td>
</tr>
</tbody>
</table>

and the formula may be provisionally given as—

\[
\frac{C^{27}H^{33}N^2O^5}{I^3} + 2SO_3 \cdot HO + 3HO = 949,
\]

which closely corresponds with the optical salt, but contains 2 atoms less water.

If this olive-coloured residue be boiled in dilute spirit, the optical crystals deposit on cooling.

From the addition of 5·32 per cent. water to this dry residue, we find that the silky crystals contain dry residue,—

\[
94·678 = 949 = 1\text{ atom}
\]

Water . . . . . 5·322 = 54 = 6 atoms

\[
100·000 \quad 1093
\]

and we have thus the following formula for the silky salt, which corresponds most closely with the result of analysis,—

\[
\frac{C^{27}H^{33}N^2O^5}{I^3} + 2SO_3 \cdot HO + 9HO = 1003,
\]
as may be seen by the following comparison:

<table>
<thead>
<tr>
<th></th>
<th>Theory</th>
<th>Experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td>57 Carbon</td>
<td>6 \times 342 = 34 \cdot 097</td>
<td>33 \cdot 947</td>
</tr>
<tr>
<td>44 Hydrogen</td>
<td>1 \times 44 = 4 \cdot 386</td>
<td>4 \cdot 423</td>
</tr>
<tr>
<td>2 Nitrogen</td>
<td>14 \times 28 = 2 \cdot 791</td>
<td>2 \cdot 700</td>
</tr>
<tr>
<td>16 Oxygen</td>
<td>8 \times 128 = 12 \cdot 764</td>
<td>12 \cdot 800</td>
</tr>
<tr>
<td>3 Iodine</td>
<td>1 \times 381 = 37 \cdot 986</td>
<td>37 \cdot 914</td>
</tr>
<tr>
<td>2 Sulph. acid</td>
<td>40 \times 80 = 7 \cdot 976</td>
<td>8 \cdot 216</td>
</tr>
<tr>
<td></td>
<td>1003</td>
<td>100 \cdot 000</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>100 \cdot 000</th>
</tr>
</thead>
</table>

giving—

<table>
<thead>
<tr>
<th></th>
<th>Theory</th>
<th>Experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbonic acid</td>
<td>125 \cdot 22</td>
<td>124 \cdot 472 per cent.</td>
</tr>
<tr>
<td>Water</td>
<td>39 \cdot 474</td>
<td>39 \cdot 807</td>
</tr>
</tbody>
</table>

Consequently the silky salt will be the optical salt + 4 atoms of water, which, under the influence of excess of sulphuric acid and prolonged delay at 62° Fahr., are assimilated by that salt; and which additional water, on boiling in spirit, is lost, and the optical salt recrystallized on cooling.

If the temperature be not too high at first, the silky crystals may be produced without the appearance of the optical. And the silky crystals at 212°, or at 62° Fahr. over sulphuric acid, become the dry residue or tri-hydrate, which, when boiled in spirit, becomes the optical, by assimilating 2 atoms of water, as may be seen by comparing the three proposed formulæ:

a. Optical.

\[ C^{57}H^{33}N^{2}O^{5} \P_{2} \} 2SO^{3}HO + 5HO = 967 \]

β. Silky Salt.

\[ C^{57}H^{33}N^{2}O^{5} \P_{2} \} 2SO^{3}HO + 9HO = 1003. \]

γ. Dry Residue.

\[ C^{57}H^{33}N^{2}O^{5} \P_{2} \} 2SO^{3}HO + 3HO = 949. \]

The results of the analysis of the cinchonidine salt having been so remarkably different from those of the formulæ generally adopted for the pure alkaloid, the author was induced to prepare some perfectly pure quinine, taking especial pains to exclude all cinchonidine; and having from that prepared some iodo-sulphate of quinine, to submit it to equally rigid analysis.

The results are the following:

These optical crystals lose 2.49 per cent. water by prolonged drying at 212° in Liebig's drying apparatus.
Dr. Herapatli on the Cinchona Alkaloids.

The residue contains the following:

<table>
<thead>
<tr>
<th>I.</th>
<th>II.</th>
<th>III.</th>
<th>IV.</th>
<th>V.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Iodine...</td>
<td>30·195</td>
<td>30·033</td>
<td>30·50</td>
<td>31·729</td>
</tr>
<tr>
<td>Sulph. acid..</td>
<td>10·246</td>
<td>9·352</td>
<td>..</td>
<td>9·631</td>
</tr>
<tr>
<td>Carbon ....</td>
<td>41·554</td>
<td>..</td>
<td>41·34</td>
<td>41·456</td>
</tr>
<tr>
<td>Hydrogen ..</td>
<td>4·766</td>
<td>4·762</td>
<td>..</td>
<td>4·54</td>
</tr>
<tr>
<td>Nitrogen....</td>
<td>3·711</td>
<td>..</td>
<td>..</td>
<td>3·380</td>
</tr>
</tbody>
</table>

Carbonic acid mean of 4 = 151·614
Water ................. = 42·326

leading to the following composition:

<table>
<thead>
<tr>
<th></th>
<th>Theory.</th>
<th>Experimental means.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbon</td>
<td>57</td>
<td>342 = 41·606</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>38</td>
<td>38 = 4·623</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>26</td>
<td>28 = 3·409</td>
</tr>
<tr>
<td>Oxygen</td>
<td>10</td>
<td>80 = 9·730</td>
</tr>
<tr>
<td>Iodine</td>
<td>254</td>
<td>254 = 30·900</td>
</tr>
<tr>
<td>Sulph. acid</td>
<td>80</td>
<td>80 = 9·732</td>
</tr>
<tr>
<td></td>
<td>822</td>
<td>100·000</td>
</tr>
<tr>
<td></td>
<td>840</td>
<td>100·000</td>
</tr>
</tbody>
</table>

which, with 2 atoms water, constitute the optical salt dried over sulphuric acid at 62° Fahr., thus:

1 atom dry residue .................. 822
2 atoms water ...................... 18

and these results may be expressed by the following formula:

\[
\text{C}^{57}\text{H}^{33}\text{N}^{2}\text{O}^{4}\text{I}^{2} + 2\text{SO}_3\text{HO} + 5\text{HO} = 840,
\]

which appears to be the constitution of the optical salt dried at 62° Fahr. over sulphuric acid.

From this it appears that the optical salt of quinine differs in chemical atomic numbers merely in the possession of 1 atom less iodine, the cinchonidine salt having 3, the quinine salt 2 atoms iodine; but in each case 2 atoms of sulphuric acid, and 5 water, with an organic base of C^{57}H^{33}N^{2}O^{4} common to both. How this is derived from C^{40}H^{24}N^{3}O^{4} in the one case, or C^{40}H^{24}N^{3}O^{3} in the other, it is difficult to point out in the present state of the question.

Were these views correct, it might naturally be imagined that the two salts may be mutually convertible. The author has undertaken numerous experiments with this object in view; and whilst he has proved that it is possible (by boiling the quinine salt in spirit surcharged with iodine) to communicate the golden tint of the reflected ray and the blue tint of the body-colour to the crystals on their re-formation, yet this modified salt retains the crystallographic forms of the true quinine salt; whilst, by treating the cinchonidine salt by spirit and aqueous sulphurous acid, that salt is modified also, becomes fibrous in character, and assumes the red body-colour of quinine salt, yet is at once to be distinguished from the true quinine
salt even by the naked eye alone; and on redissolving these in
spirit, the blue body-coloured salt again recrystallizes with its ordi-

nary golden reflected tint. The effect of diluted sulphuric acid in
converting the cinchonidine salt into the golden silky fibrous variety,
is a striking distinguishing characteristic between the two alkaloids.

These facts lead to the conclusion, that the grouping of the con-

stituent molecules in the two salts differs materially; that closely as
the quinine and cinchonidine salts agree amongst themselves, they
differ widely from the quinidine and cinchonine compounds.

The quinidine salt, after recrystallization, presents itself as long
quadrilateral acicular prisms, having a deep ruby or garnet-red
colour, with a bluish-violet or light purplish reflexion-tint; it is
sometimes deposited in thin flat plates, or long, flat, acicular prisms;
these, when thin, transmit a pure yellow colour, but in thicker
plates it becomes reddish, with a tinge of brown.

There is scarcely any appearance of double absorption in this
salt; the thicker crystals alone exhibit it, when their usual tint be-
comes darkened on analysis with a Nicol.

This salt requires 31 parts of boiling spirit, and 121 parts at 62°
to dissolve 1; water precipitates it as a cinnamon-brown powder.

Its deep marone-coloured large aciculae had a specific gravity of
1:7647 at 62°.

These large crystals, exposed whole to a temperature of 212°, de-
crepitate afterwards on exposure to the air, but dried at 212°, they do
not appear to lose further water after prolonged exposure to the
drying bath.

The author having supplied Dr. Sheridan Muspratt with a quan-
tity of this salt, has been most obligingly furnished with the
results of his examination; from which it will be seen that those
previously obtained by the author have been confirmed.

| Carbon   | 32.890 | 32.615 | 32.787 |       | 31.998 | 32.311 |
| Nitrogen | 4.400 | 4.140 | 4.440 |       |       |       |
| Oxygen   | 12.912 | 13.027 | 12.697 |       |       |       |

The formulae derivable from these analyses are the following:—

<table>
<thead>
<tr>
<th>Substance</th>
<th>Theory</th>
<th>Herapath.</th>
<th>Muspratt.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbon</td>
<td>35</td>
<td>210</td>
<td>32.967</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>25</td>
<td>25</td>
<td>3.924</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>2</td>
<td>28</td>
<td>4.395</td>
</tr>
<tr>
<td>Oxygen</td>
<td>10</td>
<td>80</td>
<td>12.559</td>
</tr>
<tr>
<td>Iodine</td>
<td>2</td>
<td>254</td>
<td>39.874</td>
</tr>
<tr>
<td>Sulph. acid</td>
<td>1</td>
<td>40</td>
<td>6.274</td>
</tr>
</tbody>
</table>

637 100.000 100.000
and give—

\[ C^{35}H^{19}N^{2}O^{4} + I^{2}SO_{3}H + 5H_{2}O = 637, \]

which differs from Gerhardt's formula for quinidine by the loss of \( C^3H^3 \); but at this stage of the question it is scarcely possible to arrive at a solution of the manner in which it is produced.

The cinchonine salt differs much from all those previously described; it exists in long, acicular, quadrilateral prisms, of a deep purplish-black colour, like that of elder-berries.

Thin crystals transmit a yellow tint—pure gamboge-yellow when very thin; soon passing through a deep sherry-brown to a blood-red colour, then a deep port-wine colour, and then becoming opaque.

These crystals reflect a deep steel-blue colour when analysed with a Nicol’s prism, and generally across the short diameter of the prism, which is the analogue of the a-prism of the quinine salt. The cinchonine salt possesses doubly absorbent powers, much more powerfully so than the quinidine salt, but inferior to all the others; the body-colour is deep sienna or bistre-brown.

This salt furnished the following analytical results:

<table>
<thead>
<tr>
<th>I.</th>
<th>II.</th>
<th>III.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Iodine</td>
<td>50·34</td>
<td>50·587</td>
</tr>
<tr>
<td>Sulphuric acid</td>
<td>5·247</td>
<td>5·217</td>
</tr>
<tr>
<td>Carbon</td>
<td>28·156</td>
<td>27·57</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>3·523</td>
<td>3·485</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>3·306</td>
<td>.</td>
</tr>
</tbody>
</table>

which lead to the following composition:

<table>
<thead>
<tr>
<th>Theory.</th>
<th>Experiment.</th>
</tr>
</thead>
<tbody>
<tr>
<td>35 Carbon ( \times 6 = 210 = 27·7410 )</td>
<td>27·698</td>
</tr>
<tr>
<td>26 Hydrogen ( \times 1 = 26 = 3·4346 )</td>
<td>3·487</td>
</tr>
<tr>
<td>2 Nitrogen ( \times 14 = 28 = 3·7000 )</td>
<td>3·306</td>
</tr>
<tr>
<td>9 Oxygen ( \times 8 = 72 = 9·5103 )</td>
<td>9·8674</td>
</tr>
<tr>
<td>3 Iodine ( \times 127 = 381 = 50·3301 )</td>
<td>50·4096</td>
</tr>
<tr>
<td>1 Sulph. acid ( = 40 = 5·2840 )</td>
<td>5·232</td>
</tr>
</tbody>
</table>

| 757 | 100·000 | 100·000 |

giving—

\[ C^{35}H^{19}N^{2}O^{2} + I^{2}SO_{3}H + 6H_{2}O = 757, \]

which, on comparison with the quinidine salt, will be found to possess 1 atom additional iodine, and 1 atom more water, but a deficiency of 2 atoms oxygen, the latter, apparently, in consequence of the original difference in the type of the alkaloids employed; and, like that salt, it differs in its organic base by the loss of \( C^3H^3 \) from
the constitution of the alkaloid originally employed, if we take the formula $C^{10}H^{24}N^2O^2$, as given by Gerhardt, for that of cinchonine.

The cinchonine and quinidine salts further agree in containing only 1 atom sulphuric acid, whereas the quinine and cinchonidine salts contain 2 atoms.

These investigations appear to show that the alkaloids in each instance undergo some modification, but not analogous to substitution; it appears more like a splitting-up into different molecular groups, and a rearrangement of these amongst themselves, as the formulae of the organic bases differ much from those of the original alkaloids.

All these iodo-salts possess double refractive properties.

When the acid sulphates of the mixed alkaloids, quinine, quinine, cinchonine and cinchonidine, are dissolved in dilute spirit, and the temperature increased to $80^\circ$ or $120^\circ$, treatment with tincture of iodine readily separates the quinine salt first.

Subsequent further treatment in the same manner produces the cinchonidine salt, more or less mixed with the quinine salt.

On still further treatment, the quinidine salt is formed with its well-marked characters.

The cinchonine salt is by far the most soluble in spirit; and when a large quantity of cinchonine exists, this compound will also appear along with the quinidine salt.

This test is a beautiful and ready method of proving the presence of cinchonidine in cinchonine, which would otherwise be considered pure, Brandes' test having shown the absence of quinine and quinidine. In the same way, this test is an easy method of detecting mixtures of quinine and quinidine, the optical characters of the two salts being so well marked, that no difficulties can exist in their discrimination.

It does not offer such facilities for the separation of quinine from cinchonidine; the two salts go down together, especially if large quantities of cinchonidine exist with mere traces of quinine.

For the success of this test, a small portion only is necessary: with quinine and quinidine $\frac{1}{30}$th part of a grain has furnished evidence of the two alkaloids; one grain would be abundant to detect all the alkaloids.

The foregoing method of examination has enabled the author to prove that the substance which Rosengarten, of Philadelphia, called quinudine, was really the cinchonidine of Pasteur, and the details of his cures of fever, therefore, by quinidine are rather to be ascribed to cinchonidine.

The cinchonidine of Wittstein, of Munich, is a totally different alkaloid, giving, with sulphuric acid and iodine, a salt at once to be distinguished by the eye from either of the two iodo-sulphates described, but yet possessing optically doubly absorbent powers. This salt has a deep orange-yellow colour by transmitted light merging into sienna-brown in thicker plates, which are generally flat and much imbricated in the method of crystallization, and also
derived from a rhombic prism. The reflected tints are brownish-
olive, not unlike dead leaves, or brown beech-leaves. These crystals
are more doubly absorbent than either the quinidine or cinchonine
salt, but less powerfully optical as tourmalines than the quinine or
cinchonidine compounds. When polarized, they transmit a sienna-
brown body-colour if moderately thick, and thicker plates are bistre-
brown, but when sufficiently thick, they are wholly impervious to
plane-polarized light. The substance was not in sufficient quantity
to admit of any analysis.

It is well known that quinine and quinidine, under the continued
effect of heat and dilute sulphuric acid, undergo a molecular change
into quinicine, which M. Pasteur has asserted to be isomeric with the
original alkaloids, but hitherto no complete analysis has been
made of the metamorphosed alkaloids.

The author has produced an iodo-sulphate of quinicine, but it is
no longer a crystalline compound; it presents itself as a deep blood-
coloured resin, very soluble in spirit and readily precipitated by water
from its spirituous solution. This substance has not yet been sub-
mitted to analysis. During the production of the iodo-sulphate of
quinicine a certain portion of the alkald becomes converted into
quinicine, as may be demonstrated by the production of this resin-
ous compound from the mother-liquid on the addition of further
proportions of iodine.

Cinchonine and cinchonidine become converted into cinchonine
by similar treatment, and this amorphous alkald also forms a
resinous iodo-sulphate; its colour is deep purple-black, and it
deposits itself on spontaneous evaporation of the spirit, or on the
cooling of a highly concentrated spirituous solution, in small drops,
highly tenacious at 100° Fahr., but becoming solid at 60° Fahr.
This compound has, in a fine state of division, a beautiful purplish-
blue colour, and such a film generally forms around the edge of the
vessel in which it is produced.

Cinchonine appears to be one of the products during the
manufacture of the iodo-sulphate of cinchonidine, but there is a
much larger production of it during the formation of the cinchonine
salt.

From the foregoing reactions, the author appears to be justified
in asserting that eventually it will be found, when we know more
of the rational grouping of the constituent atoms of the vegeto-
alkaloids, that the construction of the formula for the cinchonine
of Pasteur will have a much greater similarity to the arrangement
of the molecular groups of quinine than of cinchonine. And there
is also great probability that the grouping of the atoms of cinchonine
and the quinidine of Pasteur will be found to present more points of
similarity; but in each case he sees no reason to doubt the existence
of more oxygen in the cases of both quinine and quinidine than there
is in cinchonine and cinchonidine. He also ventures to suspect that
cinchonine and quinicine will eventually be found to contain more
carbon than the original alkaloid, the elements of water probably being
separated by the sulphuric acid during the process of formation.

December 10.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Chemical Action of Water on Soluble Salts." By Dr. J. H. Gladstone, F.R.S.

Before extending my researches on chemical affinity among substances in solution, it seemed desirable to ascertain, if possible, what specific chemical action water exerts on a salt. This inquiry is beset with unusual difficulties, and unfortunately my experiments have not led to any conclusive result. Yet some of the observations made during the course of the inquiry have a value independent of theory, and a brief notice of them may not perhaps be deemed unworthy of a place in the Proceedings of the Royal Society.

It is well known that many anhydrous salts will absorb water, and still remain solid bodies, either amorphous or crystallized. In such a case the water combined is always in simple atomic relation with the salt itself; great heat is often evolved, and a change of colour frequently ensues. These "hydrated" salts (as they are usually considered) are generally soluble in water; and it is the condition of such a body when dissolved that opens a wide field for speculation. The water may act merely as a solvent; or it may unite without decomposition with the dissolved salt, becoming an integral part of the compound in solution; or reciprocal decomposition may ensue, each electro-positive element combining with each electro-negative one in certain proportions; or the ultimate result may be due to two or more of these modes of action in conjunction.

When a "hydrated" salt is dissolved in a minimum of water, nothing is usually observed beyond the new physical properties resulting from the change in its state of aggregation and the absorption of heat. No change of colour, as far as I can find, ever ensues, though a change in the amount of fluorescence may occur. When an anhydrous salt, which will not combine with water to form a solid compound, dissolves, a change of colour does sometimes ensue. Sometimes, however, an evident decomposition takes place, the hydrogen and oxygen of the water combining each with one of the elements of the other binary compound, and the products of this action remaining uncombined. Chloride of bismuth and citrate of ammonia are instances. But in the vast majority of instances, the salt MR and the water HO do not suffer reciprocal decomposition, unless indeed, as has been contended, the resulting MO, HR remain combined together in solution.

If a reciprocal decomposition of this character actually occurs, it may be anticipated by analogy, that by increasing the amount of HO, more MR will be decomposed. Now, if additional water be added to saturated aqueous solutions of pentachloride of antimony, ferric sulphate, ammoniacal nitrate of copper, or nitrate of bismuth, decomposition results, and a precipitate forms proportional within certain limits to the amount of water added; but not one of these is a salt of the simplest constitution. Sometimes, however, a change is rendered apparent in simple salts by a change of colour without the formation of a precipitate.
On the Chemical Action of Water on Soluble Salts.

This was closely examined. It might be expected, à priori, that a certain amount of salt would have the same absorbent effect on a given quantity of light, whether it were dissolved in much or little water, and that as the absorbent power of water is practically nil, it would appear to the eye of precisely the same depth and character of colour in the two cases. And this actually holds good in the majority of instances; but to prove it a special contrivance was necessary, in order to make the same quantity of light impinge upon the solution before and after dilution. This was effected by means of colourless cylindrical glasses of uniform diameter and the same size, closed at one end with a flat plate of glass, so that when placed upright they could hold liquids: they stood in a case so contrived that all the light which passed through the strong or diluted solution, as looked through from above, had to enter by the flat plate at the bottom. Every experiment was performed by a comparative method, two glasses being placed side by side, one containing the solution to be diluted, the other a similar quantity of the same solution which served as a standard.

In this manner it was determined that the following salts absorbed the same light whether dissolved in much or in little water:—

| Ferrous Sulphate. | Terchloride of Gold. |
| Ferric Nitrate. | Terbromide of Gold. |
| Ferric Meconate. | Protochloride of Platinum |
| Ferric Comenate. | (in hydrochloric acid). |
| Ferric Comenamate. | Bichloride of Platinum. |
| Ferric Gallate. | Bichloride of Palladium. |
| Nitrate of Nickel. | Chromate of Potash. |
| Nitrate of Cobalt. | Ferroxyanide of Potassium. |
| Sulphate of Cobalt. | Ferrideyanide of Potassium. |
| Chloride of Chromium. | Nitroprusside of Sodium. |
| Acetate of Chromium. | Sulphindigotic Acid. |
| Chromate of Chromium. | Sulphindigotate of Ammonia. |
| Nitrate of Uranium. | Carbazotate of Copper. |
| Chloride of Uranium. | Pentasulphide of Potassium. |
| Sulphate of Ceric Oxide. | |

The following salts were affected in regard to their absorption of light, by adding water to their saturated solutions:—

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Ferric Acetate.</td>
<td>Red.</td>
<td>Darker.</td>
</tr>
<tr>
<td>Ferric Tartrate.</td>
<td>Red.</td>
<td>Slightly paler.</td>
</tr>
<tr>
<td>Ferric Chloride.</td>
<td>Orange-red.</td>
<td>Orange-yellow.</td>
</tr>
<tr>
<td>Ferric Citrate.</td>
<td>Red.</td>
<td>Orange and paler.</td>
</tr>
<tr>
<td>Ferric Sulpho cyanide.</td>
<td>Intense red.</td>
<td>Orange.</td>
</tr>
<tr>
<td>Chloride of Cobalt.</td>
<td>Red.</td>
<td>Paler and less pure.</td>
</tr>
<tr>
<td>Acetate of Cobalt.</td>
<td>Red.</td>
<td>Paler and more orange.</td>
</tr>
<tr>
<td>Sulpho cyanide of Cobalt.</td>
<td>Intense purple.</td>
<td>Pale red.</td>
</tr>
<tr>
<td>Chloride of Copper.</td>
<td>Green.</td>
<td>Blue.</td>
</tr>
<tr>
<td>Bromide of Copper.</td>
<td>Green.</td>
<td>Blue.</td>
</tr>
<tr>
<td>Acetate of Copper.</td>
<td>Greenish blue.</td>
<td>Pale and purer blue.</td>
</tr>
<tr>
<td>Permanganate of Potash.</td>
<td>Purple.</td>
<td>Paler and redder.</td>
</tr>
<tr>
<td>Chromic Acid.</td>
<td>Red.</td>
<td>Orange.</td>
</tr>
</tbody>
</table>
That these changes of colour are due to the action of the water, and not to any merely physical cause, is proved by the fact that alcohol does not occasion them. Quantitative experiments were instituted with acetate of copper and sulphocyanide of iron, to determine whether the effect of successive additions of water is in a decreasing ratio. It was found to be so on the whole, but the results showed certain irregularities that do not usually occur in cases of reciprocal decomposition, where the mass of one of the compounds is successively increased.

A prismatic examination of the rays absorbed by these salts in different states of solution revealed two very suggestive facts. The one is, that in every case (except ferric acetate) the salt in dilute solution not only transmits every ray that was transmitted by it in saturated solution, but also some rays which it then absorbed. The other is, that strong solutions of the chlorides, bromides, and iodides of copper, cobalt, nickel, and iron—alas, analogous metals—exhibit not only the absorption due to the respective bases, but another absorption which can be identified with that produced by the halogens themselves when simply dissolved in water; while, when these solutions are diluted, they cease to produce this second absorption, and give precisely the same prismatic image as any compound of the same base with a colourless acid. The amount of water required to effect this change depends on the temperature. That the phenomena indicate some difference of arrangement among the elements of the dissolved salt and the water, cannot, I think, be doubted, but they fail to show in any distinct manner what that difference is.

The action of water on double salts is a still more complicated problem; but the question as to whether water separates the two components did not prove so difficult of decision. While on the one hand the physical properties of many double salts, as for instance the potassio-chloride or iodide of platinum, prove that they are not decomposed by water, the experiments of Graham, on the other hand, show that some salts, as for instance alum, suffer at least a partial decomposition in diffusion.

The iodide of mercury and potassium, and the sulphocyanide of silver and potassium, dissolve in a small quantity of water, but the addition of more causes the separation of the insoluble component. The double sulphates of copper, nickel, or chromium with potash, the sulphate of copper and ammonia, the chloride of platinum and potassium, the iodides of platinum or gold with potassium, and the hydrochlorate of chloride of gold, do not change in colour on the dilution of their aqueous solutions; but this does not prove that no separation has taken place, for the colour of these double salts in solution is precisely that of an equivalent amount of that component to which the colour is due. But bichlorate of potash and bicominate of iron likewise exhibit no change of colour on dilution, though such must ensue, if they be converted into neutral salt and free acid. On the other hand, the red potassio-oxalate of chromium varies in intensity of colour on the addition of water, and the different double chlorides of copper undergo the same change as the simple salt. If hydrochlorate of terchloride of gold be added to the terbro-
mide of that metal, a reduction in colour ensues, and an analogous result is obtained when the double sulphate of copper and potash acts on the acetate of copper—facts which point to a decomposition of the double salt in solution. Indeed it is evident that some double salts are resolved more or less into their components by water, while others are not so affected.

The general tendency of my observations has led me to the opinion, that water does not act upon a salt dissolved in it in a manner analogous to that of the hydracids, but I hesitate to draw any conclusion as to the rational constitution of a dissolved salt.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xiii. p. 218.]


Feb. 23.—A paper was read "On the Theory of Polarized Fasciculi, commonly known as Haidinger's Brushes." By the Rev. J. Power, M.A., Librarian of the University.

In this paper the view taken of the subject is similar to that which had occurred to M. Jamin, and which will be found in Poggendorff's Annalen, 1849, p. 145, and in the Comptes Rendus, tome xxvi. p. 197. The author arrived, however, at the present theory quite independently in the course of last summer, and before he had acquainted himself with the literature of the subject. M. Jamin had taken as an essai de calcul the particular semi-visual angle 20°, which lies far beyond the limits within which the phænomenon is visible; and he has not attempted to give the general law for small angles, which was the real problem to be solved.

This is what the author has attempted in the present communication, availing himself of the experimental researches of Chossat given in the Bulletin de la Soc. Philomatique, 1818, p. 94.

The subject was rendered more complicated by the circumstance that the formulae for the intensities of the refracted pencils are given differently by Neumann, Airy, and the author of this paper. Instead of taking any one set of formulae, the author managed to take them all into consideration by previously showing that Airy's formula result from Neumann's by multiplying them by \( \frac{\tan \theta}{\tan \theta_i} \), which is equivalent to \( \frac{1}{\cos \theta} \); while his own result from the same by multiplying them by \( \frac{\cos \theta}{\cos \theta_i} \); \( \theta \) and \( \theta_i \) being the angles of incidence and refraction.

It follows from thence that, whichever set of formulae we may prefer, we shall have after refraction, in all cases, the following simple relation for two oppositely polarized incident pencils of equal intensity; namely,

\[
\text{intensity of pencil polarized in the plane of incidence} = \cos^2 (\theta - \theta_i),
\]

\[
\text{intensity of the pencil polarized at right angles} = \cos^2 (\theta).\]
This fraction decreases as the deviation $\theta - \theta_1$ increases; it is therefore less for the violet rays than the red, for the indigo than for the yellow; and this serves to explain in a general way the dingy yellow stripe in the plane of polarization, and the bright violet stripe in the plane at right angles.

The author has also considered the effect in a plane making an angle $\phi$ with the plane of greatest polarization, and arrives at the following result: that, provided we attribute to the distribution of the optic nerve such a variation of sensibility as, taken in conjunction with the action of the iris, shall produce a field of view uniformly bright from the centre outwards when common daylight is viewed (a condition which the author believes is common to all eyes with his own), we shall have for the brightness at any point of the field of view the following expression,

$$M \cdot (1 - e^{\gamma \frac{\theta^2}{2} \cos 2\phi}),$$

where $M$ is the central brightness, $e$ the degree of polarization (being 0 for common daylight, and 1 for completely polarized light), and $\gamma = 0.07309$ for rays of mean refrangibility.

The last expression gives us without difficulty the form of the curves of equal brightness.

Assuming this constant brightness to be $cM$, and putting

$$x = \theta \cos \phi,$$
$$y = \theta \sin \phi,$$

we find for the equation sought

$$x^2 - y^2 = \frac{2(1 - c)}{e^{\gamma}}.$$

The curves are therefore equilateral hyperbolas having the lines in octants for their common asymptotes, which confound themselves with the curves themselves when $c = 0$, the case of mean brightness.

The yellow fasciculi have their vertices in the plane of polarization, and the violet fasciculi have their vertices in the plane at right angles.

It will be seen that for a given value of $\theta$, the brightness, for rays of all degrees of refrangibility, that is for all values of $\gamma$, is least in the plane of greatest polarization and greatest in the plane at right angles—contrary to the idea of Moigno, who, for insufficient reasons, imagined that the maximum occurred in the plane of polarization and the minimum in the plane at right angles.

The yellow tint in the position of minimum intensity, and the violet tint in the position of maximum intensity, is nevertheless perfectly accounted for by the consideration that $\gamma$ is greater for the violet and indigo rays than for the red and yellow.

The paper further contains some observations respecting a subjective centre of the eye, distinct from the usual objective centre, which may be read with interest, as they remove some difficulties connected with the theory of vision, which had often occurred to the author, and may have occurred to others.
March 9.—Mr. Hopkins gave an account of some experiments on the conductivity of various substances, and pointed out the bearing of the results on theories of terrestrial heat.

April 27.—Mr. Humphry read a paper “On the Proportions of the Human Frame.”

May 11.—A paper was read by Professor Stokes, “On the Discontinuity of Arbitrary Constants which appear in Divergent Developments.”

In a paper “On the Numerical Calculation of a class of Definite Integrals and Infinite Series” printed in the ninth volume of the ‘Cambridge Philosophical Transactions,’ the author succeeded in putting the integral $\int_{0}^{\infty} \cos \frac{\pi}{2} (w^{3} - mw)dw$ under a form which admits of receiving every numerical calculation when $m$ is large, whether positive or negative. The integral is obtained in the first instance under the form of circular functions for $m$ positive, or an exponential for $m$ negative, multiplied by series according to descending powers of $m$. These series, which are at first convergent, though ultimately divergent, have arbitrary constants as coefficients, the determination of which is all that remains to complete the process. From the nature of the series, which are applicable only when $m$ is large, or when it is an imaginary quantity with a large modulus, the passage from a large positive to a large negative value of $m$ cannot be made through zero, but only by making $m$ imaginary and altering its amplitude by $\pi$. The author succeeded in determining directly the arbitrary constants for $m$ positive, but not for $m$ negative. It was found that if, in the analytical expression applicable in the case of $m$ positive, $-m$ were written for $m$, the result would become correct on throwing away the part involving an exponential with a positive index. There was nothing however to show a priori that this process was legitimate, nor, if it were, at what value of the amplitude of $m$ a change in the analytical expression ought to be made, although the occurrence of radicals in the descending and ultimately divergent series, which did not occur in ascending convergent series by which the function might always be expressed, showed that some change analogous to the change of sign of a radical ought to be made in passing through some values of the amplitude of the variable $m$. The method which the author applied to this function is of very general application, but is subject throughout to the same difficulty.

In the present paper the author has resumed the subject, and has pointed out the character by which the liability to discontinuity in the arbitrary constants may be ascertained, which consists in this, that the terms of an associated divergent series come to be regularly positive. It is thus found that, notwithstanding the discontinuity, the complete integrals, by means of divergent series, of the differential equations which the functions treated of satisfy, are expressed in such a manner as to involve only as many unknown constants as correspond to the degree of the equation.

Divergent series are usually divided into two classes, according as
the terms are regularly positive, or alternately positive and negative. But according to the view here taken, series of the former kind appear as singularities of the general case of divergent series proceeding according to powers of an imaginary variable, as indeterminate forms in passing through which a discontinuity of analytical expression takes place, analogous to a change of sign of a radical.

A communication was likewise made by the Rev. W. T. Kingsley, "On the application of Photography to Wood Engraving."

GEOLOGICAL SOCIETY.

[Continued from vol. xv. p. 553.]

April 28, 1858.—Professor Phillips, President, in the Chair.

The following communications were read:


Deposits indicating the existence of the Coal-flora and its associated freshwater fauna at the beginning of the Carboniferous period are well developed in Nova Scotia and New Brunswick, with a clearness and fulness of detail capable of throwing much light on the dawn of the terrestrial conditions of the Coal-period, and on the relations of these lower beds to the true coal-measures. This lower series comprises shales and sandstones (destitute of marine remains, but containing fossil plants, fishes, entomostraca, worm-tracks, ripple- and rain-marks, sun-cracks, reptilian footprints, and erect trees) and great overlying marine limestones and gypsums. These are distinct from the true coal-measures by their position, mineral character, and fossil remains. In the western part of Nova Scotia (Horton, Windsor, &c.) the true (or Upper and Middle) coal-measures are not developed; and here the Lower Carboniferous marine deposits attain their greatest thickness. The lower coal-measures (or Lower Carboniferous freshwater or estuarine deposits) have here a thickness of about 600 feet. These beds are traceable as far as the Shubenacadie and Stewiacke Rivers. They outcrop also on the south side of the Cobequid Mountains, where the marine portion is very thin, owing perhaps to the fact of these mountains having been land in the coal-period.

Along the northern side of the Cobequid Range the upper and middle coal-measures and the marine portion of the Lower Carboniferous series are of great thickness. The freshwater beds are absent here, though brought up on the northern side of the coal-trough of Cumberland, where, as well as in New Brunswick (Petitecodiac River, &c.), they are remarkable for their highly bituminous composition, their well-preserved fish-remains, and the almost entire absence of plants. To the north, at the Bay of Chaleurs, the great calcareous conglomerate, with sandstone and shale, 2766 feet thick, described by Logan, and containing a few plant-remains, probably represent the Lower Coal-measures of Nova Scotia. In eastern Nova Scotia and Cape Breton the Middle Coal-measures are found
at Caribou Cove and elsewhere; the marine limestones and gypsuns, and the underlying sandstones and shales, are seen at Plaister Cove; also at Right's River, and St. Mary's River.

In Nova Scotia these older coal-measures, as compared with the true coal-measures, are more calcareous, more rich in remains of fishes, and have fewer vegetable remains, and indications of terrestrial surfaces. They occur generally along the margins of the coal-areas, near their old shores; and, as might be expected under such circumstances, they are associated with or replaced by beds of conglomerate derived from the neighbouring highlands of Devonian or Silurian rocks. When the conglomerates are absent, alternations of sandstones with sandy and calcareous shales occur, with frequent changes in character of the organic remains, the general aspect being that of muddy estuarine deposits, accumulated very slowly, and discoloured by decaying organic substances. The supply of sediment, and the growth and preservation of vegetable matter, appear to have been generally on a smaller scale in this early Carboniferous period than subsequently. In those districts where the true coal-measures are least developed the lower series is most important; showing that the physical and vital conditions of the Coal-measures originated as early as those of Mountain-limestone; and that locally these conditions may have been contemporaneous throughout the whole period; but that in some localities the estuary and swamp deposits first formed were completely submerged and covered by oceanic deposits, whilst in others early marine beds were elevated and subjected to the conditions of gradual subsidence and vegetable growths indicated in the great coal-measures of the South Joggins, Pictou, and Sidney.

In Nova Scotia the Lower Coal-measures are characterized by a great preponderance of Lepidodendra (especially L. elegans) and Poacites. The Middle Coal-measures are rich in Sigillariae and Ferns, as well as Lepidodendra. The Upper Coal-measures especially abound in Conifers, Calamites, and Ferns. Palæoniscus, Gyrolepis, or Acrolepis, Centrodes, Rhizodus, and Ctenoanthus are the chief fossil fishes of this Lower Carboniferous series. Unio-like shells are nearly the only remains of Molluscs.


The author, having lately met with several specimens of this singular root which exhibit good structural characters, was enabled in this paper to throw some additional light on the origin of the medullary rays in Stigmaria, the nature of the vascular bundles which were believed to be dispersed throughout its pith, and the structure of the central or vascular portion of its rootlet,—points of considerable interest, and which up to the present time have not been well determined. Mr. Binney confirms Dr. Hooker's view of the medullary rays having originated in the cellular axis of the stem. The axis, in one of the author’s specimens, is seen to be filled with eleven or twelve large vessels, each about \(\frac{1}{10}\) th of an inch in diameter, and having very thick walls: these elongated utricles lie close together,
are not dispersed in a cellular tissue, and are somewhat similar to those described by M. Brongniart as occurring in the woody part of Sigillaria elegans. In a rootlet Mr. Binney finds evidence of an outer ring of fine cellular tissue, three or four cells broad, and in the centre of the middle space twenty-seven large and eleven smaller vessels, forming a mass of vascular tissue $\frac{3}{4}$th of an inch in diameter, the vessels or utricles being distinctly marked with transverse striae.


A few fragments of fern-leaflets, found by Mr. G. Roberts, of Kidderminster, in a micaceous sandstone about two miles north-east of Bewdley, exhibit a reticulate venation; a character comparatively rare in the Coal-measures. These specimens are but fragments of the frond; and the one particularly referred to is probably the terminal part of a pinna. The author doubtfully referred it to the genus Woodwardites, with the specific name Robertsi.

May 12.—Prof. Phillips, President, in the Chair.

The following communications were read:


The author first described the evidently glacialized condition of the great Laurentine Chain of mountains, on the north side of the St. Lawrence, which for an extent of 1500 miles exhibit, often in spite of the forest, unequivocal signs of glacial abrasion, being mammillated (or moutonnées), as if by the action of ice.

On the south side of the river, the country is low, and covered with boulders and other drifts, derived from the Laurentine Chain and other tracts, in accordance with the observations of Bigsby, Hitchcock, and others. The plains and the Thousand Islands exhibit a general glacialization. These conditions are traceable down the Valley of the Hudson to New York.

During the period when the boulders and the associated clays and gravels were being deposited, the Catskill Mountains appear to have been under water, and at about the same period to have been subjected also to very extensive glacial action. The striae left by ice-borne rocks on the eastern flank of the Catskills have a north and south direction, and are found up to nearly the height of 3000 feet above the sea, excepting in the east and west gorges near the top, where the striae run in a cross direction—E. and W. The sea of the drift-period in the Valley of the Hudson was then from 3000 to 4000 feet deep. The deep valleys on the western side of the mountains were observed by Prof. Ramsay to be often charged with drift, which had not been ploughed out by glaciers of a date subsequent to the upheaval of the Catskills, as is the case with some of the valleys, once occupied by drift and afterwards by glaciers, in Wales, the Highlands, the Vosges, and in the Alps.

Prof. Ramsay then referred more particularly to the drift-deposits forming terraces in the neighbourhood of Montreal, which have lately
been described in detail by Dr. Dawson; and pointed out his reasons for regarding the Leda-clay of Dawson to be of contemporaneous formation with the Nucula-clay of the eastern side of Lake Champlain, and with the clay-beds of Albany and elsewhere on the Hudson. The clay of the Hudson Valley lies on boulder-beds, and rarely contains boulders. Along Lake Champlain similar clays overlie old drift, at about 150 feet above the sea-level, and contain fossils similar to those of the Montreal clays, at 140 feet above the sea, and of other contemporaneous beds on the Ottawa. Prof. Ramsay assumed Dr. Dawson’s conclusion as to the age of the Leda-beds, which were deposited, in a thickness of from 100 to 300 feet, over the boulder-clay, whilst the sea covered the Ontario basin, and came up against the great escarpment of Niagara limestone, which, now stretching across this region, formed the southern coast of the glacial sea.

The author then inferred that, the Erie plateau having been elevated, the falls of Niagara commenced, by the drainage of the upper lake-area, a little before the close of the drift-period, falling first into the sea over the edge of the escarpment above Queenstown and Lewistown. If the 35,000 years suggested by Sir C. Lyell as the minimum for the time occupied in the erosion of the gorge of Niagara be approximately correct, though probably below the reality, we have an idea of the amount of time that has elapsed since the close of the drift-period. And, if it be ever found possible to accurately determine the ancient rate of recession, we shall have data for a first approach to an actual measurement of a portion of geological time. This subject is intimately connected with the synchronism of the mastodon-bearing freshwater strata of Niagara and those of the bluffs of the Mississippi.

2. "On Lamination and Cleavage occasioned by the mutual friction of the particles of rocks while in irregular motion." By G. Poulett Scrope, Esq., M.P., F.R.S., F.G.S.

The author referred to a former paper read by him before the Society in April 1856, in which this subject was touched upon, and proposed to carry on the inquiry as to the probable effect, upon the internal structure of rocks, of the mutual friction of their component parts, when forced into motion under extreme and irregular pressures. He commenced by examining the laws that determine the internal motions of substances possessing a more or less imperfect liquidity, whether homogeneous, or consisting of solid particles suspended in, or mixed with, or lubricated by, any liquid, under unequal pressures; and showed that unequal rates of motion must result in the different parts of the substance, and that in the latter case, there will be more or less separation of the solid and coarser from the finer and liquid particles, into different zones or layers; those composed of the former moving less readily than those composed of the latter; and also that the former will, by the friction attending this process, be turned round so as to bring their major axes into the line of direction of the movements; and, if susceptible of tension or disintegration, will be elongated or drawn out in the same direction.

In illustration of this law, specimens of marbled paper were pro-
duced, being impressions from superficial films of coloured matter floating upon water in circular or irregular forms, after they had been subjected to motion in one or more directions by lateral pressure,—the appearances produced bearing a very exact resemblance to those presented by the lamination and occasionally sinuous or contorted structure of the ribbed lavas of Ponza, Ischia, the Ascension Isles, &c., as well as that of gneiss and mica-schist.

The author proceeded to state that the expansion of a subterranean mass of granite by increase of temperature, to which all geologists agree in ascribing the elevation of overlying rocks, must be accompanied by great internal movements and consequent mutual friction among the component parts, and even among the individual crystals; that, if a lubricating ingredient, such as water holding silex in solution, or gelatinous silex, be intimately mixed up with the more solid crystals (as there is great reason to believe to have been the case in granite), the friction will be lessened, especially in the central or inferior parts of the mass, where the expanding movement, or intumescence, may be supposed nearly uniform in all directions. But in the lateral and higher portions directly exposed to the resistance and pressure of the overlying rocks shouldered off on either side by the expanding granitic axis, the movement will probably have been so predominant and extreme in a direction at right angles, or nearly so, to the pressure, as to give rise to a lamellar arrangement of the solid crystals, in the manner before indicated. In this manner he supposes the foliation or lamination of gneiss and mica-schist to have been produced through the "squeeze and jam" of the lateral and superficial portions of a granitic mass expanding by increase of temperature, and the giving way of the overlying rocks, those portions being forced to move in the direction of the lamination while subject to intense pressure at right angles, or nearly so, to that direction. The author argues that it is not inconsistent with this view, to suppose that a certain amount of recrystallization may have accompanied or followed this lamellar arrangement, in which case also the major axes of the crystals would be likely to take a direction perpendicular to the pressure, since the mobility necessary to the crystalline action will have been freer in that than in any other direction. He likewise points out that the influence of internal friction accompanying motion under extreme and irregular pressures, must have been equally operative in the case of aqueous as of igneous rocks, under similar circumstances of imperfect liquidity, and irrespective of changes of temperature. And he suggests that to this cause may be attributable the internal structure of some veined marbles, calcareous breccias, serpentines, &c., as well as the cleavage of the slaty rocks, as, indeed, the experiments of Mr. Sorby and of Professor Tyndall have already indicated. He concludes by suggesting to all geologists engaged in the examination of rocks the above mechanical considerations, as likely to lead to more definite views than at present prevail as to the origin of the metamorphic schists, and the internal structure of many of the older and more disturbed rocks of all characters.
VIII. Intelligence and Miscellaneous Articles.

MICROSCOPIC OBSERVATIONS ON THE ELECTRIC SPARK.

BY R. FABBRI.

To study the causes which determine the figure of the electric spark, I undertook to examine it with the microscope. When a spark is evolved between two conductors, two facts are usually observed with regard to its form:—1. It is composed of different rays which unite at their extremities at more or less acute angles, so as to form in the whole a broken line; and 2, these rays themselves present various curves.

These two phenomena appear to arise from different causes, some acting in a non-continuous manner at the points of inflection, the others having a continuous action, at least in the space where the spark exhibits no inflection.

For the examination of these phenomena, I thought it advisable to employ the microscope, with which I observed a small spark passing between the extremities of two very fine metallic wires, fixed with shell-lac to a plate of crystal. I adopted this arrangement for the following reasons:—As the medium in which the spark is propagated must necessarily have some influence on its form, there will be a much greater chance of homogeneity in this medium, if we operate upon a small stratum, and, consequently, with small sparks, for the examination of which the microscope must be employed; moreover this instrument, which causes the disappearance or great diminution of irradiation, renders the limits of the spark more distinct, and allows small differences of form to be distinguished more readily. I have never made use of high powers, which would have been superfluous and even inconvenient.

The first thing that I observed on examining the spark under the microscope, was its small breadth, which is certainly much less than it appears to the naked eye; it is sufficient to say that, although the irradiation was not completely destroyed, I have seen sparks of several millimetres in length, the apparent breadth of which under the microscope was certainly less than \(\frac{1}{100}\) th of a millimetre. The spark of a small Leyden jar appeared much broader and more luminous.

When the extremities are very close together, no point of inflection is ever observed in the spark, which then appears invariably curved; and it appeared to me that, when the spark started from the same two points, it presented the same curve. But if the density of the interposed medium be increased by placing between the two extremities of the conductor a drop of a non-conducting fluid, such as olive oil, and compelling the spark to traverse it, the spark is seen to take the form of a broken line, even when the explosive distance is very small.

It appears that we may conclude from these experiments that the zigzag of the spark is probably due to the medium in which it is produced, whilst its curvature arises from the position of its extremities in relation to the other parts of the conductors between which it springs, and perhaps also to the surrounding bodies.

In the air the spark appears of a violet colour under the microscope, as with the naked eye; but when the source of electricity is
strong and the conductors are sufficiently close together, the lower part becomes disseminated and converted into a plume; a brilliant light like that of the ordinary spark is then observed at the two extremities of the conductors; this light gradually diminishes as it advances towards the middle, whilst the plume becomes developed round it, so that it might be said that the spark, starting from the extremities, becomes converted by degrees into a plume, in such a way that the internal parts are the last to undergo this change.

With two iron or steel wires, small globes of reddish light are seen darted in all directions, probably due to particles of metal detached or projected by the discharge, and ignited in the air. This proves that the electric spark not only transports matter, but also projects it in all directions.

When the electricity is passed between two very acute points, such as those of two small sewing needles, a great part of the electricity passes almost continuously from one point to the other, but at the same time a great number of sparks are seen rapidly uniting with the luminous thread which exists between the two conductors. These small sparks are recognized principally at the luminous points formed at the places from which they start; and it is remarkable that these points are always at a certain distance from the apex of the pointed conductor.

On attentively observing the extremities of the spark, a very luminous point is seen in each, surrounded by a halo, the diameter of which is sometimes greater than that of the spark, and which acquires different appearances according to the nature of the metal of which the conductors are composed.

The halo is small and white with platinum points; it is blue with iron, and of a fine green with copper; lastly, with two wires of amalgamated metal, very large white haloes, with but little brilliancy at their margins, are obtained. These haloes are much larger and more brilliant if the metals between which the spark is produced be covered with a stratum of olive oil. This result is easily obtained by placing at the two extremities a drop of oil which covers a small portion of the wires, and causes the passage of the spark from points at a greater distance but nearly in contact with the oil, so that they are constantly covered with a layer of that liquid. By operating in this way with very fine copper wires, the brilliant points are surrounded by two large haloes of a magnificent green colour, which produces a very pretty effect of contrast with the violet colour of the spark. The cause of this large halo is probably the combustion of a small quantity of oil, the flame of which contains metallic particles detached by the spark. Lastly, it must be observed that with other isolating liquids more combustible than oil, such as oil of turpentine, the phenomenon is not so well produced.—*Il Nuovo Cimento*, vii. p. 182; *Bibl. Univ. de Genève*, May 20, 1858, p. 58.

---

ON THE ORIGIN OF FELSPARS, AND ON SOME POINTS OF CHEMICAL LITHOLOGY. BY T. STERRY HUNT.

In a communication to the Royal Society of London, read on the
7th of May, 1857 *, I showed that solutions of alkaline carbonates may give rise to the formation of silicates of lime, magnesia, and protoxide of iron, when heated to 212° F. with mixtures of the carbonates of these bases with quartz, a silicate of the alkali being first formed, and then decomposed by the earthy carbonates. Shortly after, in my report of the Geological Survey for 1856, I suggested that the alkaline silicates might combine with silicate of alumina to form those felspathic and micaceous minerals which are so generally associated with the silicates of protoxide bases; I further suggested that these minerals might be crystallized by the aid of heated alkaline solutions under pressure, and I thus endeavoured to explain the development of crystals of felspar and mica, in sedimentary rocks, even where the organic remains are still preserved.

While arranging an apparatus in which I proposed to heat under pressure a solution of carbonate of potash with silica and kaolin, in the hope of obtaining a double silicate of alumina and potash, M. Daubrée announced to the French Academy of Sciences (Comptes Rendus, Nov. 16, 1857) that he had succeeded in obtaining crystalline felspar, mixed with crystals of quartz, by heating during a month, a mixture of kaolin and silicate of potash to 400° C. He has moreover shown that felspars and pyroxenes are very stable in presence of heated alkaline solutions, and that crystallized diopside and Wollastonite are formed when artificial glasses, containing lime and iron, are heated in the same way to 400° C. in the presence of a small amount of water. The alkaline silicate which separates from the decomposition of the glass is resolved into quartz, which forms regular crystals, and a soluble silicate having the formula SiO, KO.

These results of M. Daubrée serve in the most remarkable manner to confirm my theory of the normal metamorphism of sedimentary rocks at temperatures below ignition, by the intervention of solutions of alkaline silicates, which convert mixtures of quartz and earthy carbonates into the corresponding silicates, and clays into felspars and mica, the intervention of alumina sometimes generating chlorite, epidote and garnet.

Daubrée remarks that glass when thus heated in presence of water swells up, indicating a softening and a plasticity of the mass; and he observes that his experiments enable us to understand the part which water may have played in the formation of the igneous rocks. His observations go far to support the views of Poulett Scrope and Scheerer, who maintain the aqueo-igneous fusion of granites and lavas, a theory which is further sustained by the curious microscopical investigations of Mr. Sorby lately presented to the Geological Society of London †.

Sir John F. W. Herschel many years since put forward a theory of volcanos, in which he suggested that all volcanic and plutonic rocks were no other than sedimentary deposits, melted down with their included water. I have endeavoured, in a paper read on the 6th of March before the Canadian Institute at Toronto, to show that this theory is the one most in accordance with the present state of

* Phil. Mag. vol. xv. p. 68.
† Ibid. p. 152.
chemical and geological science. We are probably acquainted with no rocks not of aqueous origin; and the intrusive form so often assumed by granites, syenites, dolerites, and even by ophiolites and limestones, is not essential but accidental.

In my report for 1856, p. 485, I have insisted that the separation of oxide of iron from certain strata, and its accumulation in others, is to be ascribed to the reducing and solvent action of organic matters. This is exemplified in the fire-clays and iron-stones of the coal formation, as well as in the series of the Hudson-river group described in my report, in the fire-clays and greensands of the cretaceous formation of New Jersey, and many other instances. It is by the alteration of such materials that the white felspathic rocks of metamorphic regions have been formed; and it is probable that beds of iron ore always owe their origin to the intervention of organic matters, so that the presence of such ores, not less than that of graphite, points to the existence of organic life even during the Laurentian or so-called Azoic period.

The waters which dissolve out the oxide of iron from sediments, also remove lime and magnesia, especially if these bases are present in the condition of carbonates, and hence these bases, especially the latter and more soluble, are generally absent from white clays and felspars rocks. If, however, the quantity of lime be large as compared with the iron, this may be removed while a portion of lime remains; if, on the other hand, the reducing agency of organic matters be excluded, carbonated waters may remove lime and magnesia, leaving the peroxide of iron behind.

From my own and others analyses of the alkaline mineral waters derived from argillaceous rocks, it will be seen that the salts of potash in these waters are generally in very small quantity when compared with the salts of soda, although potash predominates in argillaceous shales and clay-slates. The soda is therefore gradually removed from these rocks by infiltrating waters, while the potash remains behind, and hence it happens that when these rocks, from which the lime, magnesia, and oxide of iron have been dissolved, are subjected to the process of metamorphism, potash-felspar will predominate, together with quartz, from the deficiency of bases, while silicates like cyanite and staurolite may be formed from the excess of alumina. The more quartzose sediments, other things being equal, are most permeable to water, and hence will have lost greater proportions of soda, lime, magnesia, &c., than the finer clays and marls.

I have here indicated a few principles which must I think for the future serve as guides in investigating the chemistry of rocks, whether stratified or plutonic. You will see how the action of these laws necessarily divides the silico-aluminous sedimentary rocks into the two great classes recognized by Bunsen and Durocher in their investigations of igneous rocks. In the trachytic and granitic division the silica and potash predominate, and the soda, lime, magnesia and iron are present only in small quantities, while in the pyroxenic rocks silica and potash are less abundant, and soda-felspars with more or less basic silicates of lime, magnesia, and oxide of iron predominate.—Silliman's American Journal for May 1858.
IX. On certain Results of Magnetical Observations.

By John Allan Broun.

To Sir David Brewster.

My dear Sir,

I HAVE just had a sight of your article on Magnetism in the new edition of the Encyclopædia Britannica (vol. xiv. part 1), and I have observed that you notice my results with reference to the lunar influence. Most of the results which were obtained by me are so mixed up with large masses of figures in the volumes of the Makerstoun Observations, that I was requested by some scientific friends, before leaving Europe, to publish a popular résumé of them. This I deferred from the desire to confirm and render them more general, if possible, by observations to be made near the magnetic equator. As I have now obtained some results from my observations here, you will perhaps allow me to offer you an account of some of those obtained by me in Scotland, and here near Cape Comorin.

It is not my wish to occupy your attention with the results I had obtained with reference to the lunar magnetic influence; but I may notice, as an evidence of how little is known of the Makerstoun Observations, the statement by General Sabine, "that Toronto is the first and only station at which the numerical values at every lunar hour of the lunar-diurnal variations of the three elements have been published" (Proceedings of the Royal Society, March 5, 1857).

The numerical values at every lunar hour during each lunation in the years 1844 and 1845 for the declination, horizontal force, and vertical force, were given in the Makerstoun Observations published in 1848 and 1850. Also the numerical values of the declination, horizontal force, and vertical force, were given in the Makerstoun Observations published in 1848 and 1850. Also the numerical values of the declination, horizontal force, and vertical force, were given in the Makerstoun Observations published in 1848 and 1850.
for the winter groups and summer groups of lunations for the magnetic inclination and total force were given at each lunar hour in the same volume. The results for the lunations of both years for the two components of force were projected and will be found in the Edinburgh Transactions, read in January and April 1846. General Sabine’s investigation, which I have not yet seen, dates, I believe, in 1856.

Besides the results for these elements, I have given in the general results (published in 1850) the projections of the diurnal curve, due to lunar influence, described by the north end of a needle freely suspended in the direction of the magnetic dip. As I have not noted in the volume referred to the results deducible from this curve, I shall state them shortly here.

1st. The amount of motion due to lunar influence of a freely suspended needle is a maximum twice in a lunar day, namely during the hours before and after the moon’s transit of the inferior meridian, and at the transit of the superior meridian. It is a minimum twice; a principal minimum six hours after the superior transit, and a secondary minimum six hours after the inferior transit.

2nd. The greatest amount of motion occurs while the moon is below the horizon. This result, if confirmed, is very curious, as it is wholly opposed to all that we know of the solar action.

3rd. The direction of motion is, on the whole, the opposite to that produced by the solar influence, from one hour after the moon’s passage of the superior meridian to one hour after the passage of the inferior meridian; and it is the same as that due to the sun in the remaining twelve hours.

The following are the approximate angular movements of the needle due to the moon’s action for different intervals, deduced from observations at Makerstoun during twenty-five lunations, 1844, 1845.

<table>
<thead>
<tr>
<th>Moon’s inferior transit</th>
<th>h</th>
<th>m</th>
<th>h</th>
<th>m</th>
<th>Angular motion</th>
</tr>
</thead>
<tbody>
<tr>
<td>12 0 to 13 55</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0·16</td>
</tr>
<tr>
<td>13 55</td>
<td>15</td>
<td>50</td>
<td></td>
<td></td>
<td>0·13</td>
</tr>
<tr>
<td>15 50</td>
<td>17</td>
<td>45</td>
<td></td>
<td></td>
<td>0·04</td>
</tr>
<tr>
<td>17 45</td>
<td>19</td>
<td>40</td>
<td></td>
<td></td>
<td>0·04</td>
</tr>
<tr>
<td>19 40</td>
<td>21</td>
<td>35</td>
<td></td>
<td></td>
<td>0·07</td>
</tr>
<tr>
<td>Moon’s superior transit</td>
<td>0</td>
<td>0</td>
<td>2</td>
<td>25</td>
<td>0·10</td>
</tr>
<tr>
<td>2 25</td>
<td>4</td>
<td>20</td>
<td></td>
<td></td>
<td>0·08</td>
</tr>
<tr>
<td>4 20</td>
<td>6</td>
<td>15</td>
<td></td>
<td></td>
<td>0·01</td>
</tr>
<tr>
<td>6 15</td>
<td>8</td>
<td>10</td>
<td></td>
<td></td>
<td>0·07</td>
</tr>
<tr>
<td>8 10</td>
<td>10</td>
<td>5</td>
<td></td>
<td></td>
<td>0·12</td>
</tr>
<tr>
<td>10 5</td>
<td>12</td>
<td>0</td>
<td></td>
<td></td>
<td>0·08</td>
</tr>
</tbody>
</table>

I shall now refer to the laws of magnetic disturbance.

Diurnal law.—The fact that the movements of the declination-
needle were most irregular in the evening was remarked by Cassini (and probably by earlier observers); but, as far as I am aware, the first careful investigation is due to M. Kreil. This physicist concluded that the perturbations were a maximum in the evening from 8th to 10th P.M., and a minimum in the morning from 8th to 10th A.M. The law for the disturbance of force was found to have different epochs. These conclusions were verified by different persons, and by myself to a certain extent.

I found that these results were on the whole due to a combination of different and even opposite laws; my conclusion was as follows:

"It appears from these results that the diurnal law of mean disturbance is not constant throughout the year, as has been supposed; in fact, the law for the summer is nearly the reverse of that for winter, while that for autumn is nearly intermediate between the two" (Trans. Roy. Soc. Edinb. vol. xix. part 2. p. xxviii).

In 1856 I communicated the fact to Lieut.-Colonel Sykes and others, that the law of disturbance near the equator was the inverse of the so-called mean law for Europe; that is to say, near the equator the diurnal law of disturbance throughout the year resembles on the whole the law for Europe in summer, the maximum occurring about 10 A.M. and the minimum about 10 P.M. I have therefore not only given the diurnal law of disturbance for each season in Europe, but I have also found that this law varies both with season and latitude. These results refer to the mean disturbance without reference to sign. When the sign or direction of movement was considered, it was found that the maximum positive disturbance coincided on the whole with the minimum negative disturbance. This is not the case near the equator; the law for both signs is the same. It is probable, therefore, that for intermediate latitudes the relation of the law of signs will vary. We may expect in some intermediate latitude that the mean disturbance of declination will be equal at 10 A.M. and 10 P.M., and that the hour of maximum disturbance of force will occur gradually later after noon as we leave the equator and approach the poles.

Monthly laws of disturbance.—I found that the mean departure of the declination-needle from its monthly mean positions at the corresponding hours, was greatest two or three days after the moon's opposition, and was least about the time of conjunction (Trans. Roy. Soc. Edinb. vol. xix. part 2. p. xx). I also concluded that the diurnal range of the declination-needle was greatest when the moon was near the equator (Ibid. p. xix): similar laws were found for the components of force.

Annual laws of disturbance.—The mean departure of the de-
clination-needle from the normal position for the corresponding hour was found by me to be greatest near the equinoxes, and least at the summer and winter solstices (Ibid. p. xxii). This law was published by me in 1846. At that time some investigations had been made to determine the law. M. Kreil and General Sabine had found the epoch of maximum disturbance to be in winter, and Dr. Lloyd had ascertained it to be in summer. General Sabine’s result, it is true, was derived from disturbance above a certain arbitrary limit,—a fact, however, which I showed could not have affected the general conclusion. Since then General Sabine has verified on the whole my result by his own process.

I may add here, to complete these laws, that first indicated by Dr. Lamont, namely that the diurnal movement seems to have been a maximum in 1838 and 1848, and a minimum in 1843–44, or that the amount of the magnetic variation seems to obey a decennial law; a result which General Sabine has also found to exist in his discussion for the value of disturbance.

It may be seen from the results of the Makerstoun Observations (Trans. Roy. Soc. Edinb. vol. xix. part 2. pp. xii, xxxi, xlv), that the secular change seems on the whole to obey a similar law. This fact I noticed at the time (Ibid. No. 5. p. xii.); but as the changes were probably in part due to instrumental causes, especially in the first years of the series, I could not offer any general conclusion. An examination by me of observations made in other places, however, seems to confirm the results obtained at Makerstoun, which may be stated thus:—

The secular rate of increase of horizontal force was a maximum in 1843–44, and a minimum in 1847–48.

The secular rate of diminution of the vertical force was a minimum in 1848. As 1842 was the first year for which a result has been obtained, the epoch of maximum is uncertain.

The rate of yearly change of magnetic declination seemed to attain a maximum in 1847–48.

As already mentioned, the results in the first years (1842–44) were probably affected by instrumental errors; but this could not be the case in the latter years; and this connexion with the decennial period for three instruments cannot be accidental.

These results were evidently connected with the increasing amount of disturbance; and at first sight it might appear that the variation of the secular change was due to disturbance: thus the disturbance tends to diminish the value of the horizontal force, the secular change being a yearly increase of force; the greater the amount of disturbance, the less the yearly increase should appear. This relation, however, does not appear to apply either to the vertical force or to the declination; and I am in-
duced to conclude that the variations of the secular change are not due to disturbance, though both apparently, obeying the same law, are due to the same cause. This is the first appearance of a law affecting the secular variations.

I have endeavoured to show that the frequency of the appearances of the aurora borealis obeys the same laws as the amount of the magnetic disturbance.

The general connexion betwixt the appearance of the aurora borealis and magnetic disturbances was known in the time of Graham, Celsius, and Wargentin. The fact that the needle was even disturbed sometimes on the approach of an aurora (that is to say, when invisible at the place of the needle), is noted by Mairan as a result obtained by Wargentin (Mairan, p. 450, 2nd edit.). It is to M. Arago that we owe a distinct statement, and a considerable amount of evidence of the fact, that when there is a magnetic disturbance in Europe, there is generally an aurora borealis visible in higher latitudes, if not visible at the place where the needle is disturbed.

My own observations proved that there was always an irregularity in the movement of the needle, even when the aurora was most feeble; in some cases slight disturbances caused me to search for the aurora and discover its existence, when otherwise it would probably never have been remarked. I have noticed several times in the Makerstoun Observations a curious phenomenon occurring with faint magnetic disturbance, which suddenly covered the whole sky as with a thin milky veil, that I have designated milky aurora. I never, however, observed a magnetic disturbance at Makerstoun without finding an aurora also visible, if the state of the sky was favourable. The following are the laws of aurora deduced from my own observations:

**Diurnal law.**—The maximum frequency in the winter months occurs about 9 or 10 p.m. This fact is as old as the corresponding one for magnetic disturbance.

**Monthly law.**—As I lost no opportunity of searching for aurora during the years I had the direction of Sir Thomas Brisbane's observatory, I conceived, if a monthly law existed, it might be deduced from my observations. I think I have shown strong reasons to conclude that the maximum frequency of the aurora occurs near full moon. It is curious that this is the result that Mairan finds should follow from his theory (Mairan, p. 280). This is the epoch I have found for the maximum disturbance also.

**Annual law of variation of frequency of the Aurora Borealis.**—This law, that the maximum frequency occurs at the equinoxes, has been attributed lately to M. Hansteen (Proc. Roy. Soc. vol. vii. p. 436). It was attributed by M. Kacmutz to Mairan.
An examination of the first edition of Mairan's work induced me to refuse the credit to that philosopher. M. Secchi, who has since examined the same edition as myself (1733), has repeated Mairan's claim. A careful perusal of the second edition of Mairan's work induces me still to believe that Mairan did not announce this fact distinctly in his first edition; but in the supplement to the second edition (1754), Mairan includes the observations of Celsius (1733), Frobesius (1739), Delisle (1738) and others, all of which show this law distinctly. Mairan then refers to it in the following manner:—"On a pu remarquer en jetant les yeux sur les sommes pour les mois de la Table composée No. 77, et de la plupart de celles qui la composent, que la fréquence des apparitions de l’aurore boréale autour des équinoxes ou environ un mois avant et après, est aussi grande et quelquefois plus grande qu’autour du périhélie; ce qui paroit dit-on, infirmer les inductions que nous avons tirées de la fréquence du phénomène autour du périhélie." (De Mairan, Traité de l’Aurore Boréale, 2nd edit. 1754, p. 532.)

The law, therefore, is as old as the time of Mairan, whether its clear enunciation be due to that philosopher or not. The law has been verified by every moderately long series of observations since then, and latterly by M. Hansteen and by myself. It coincides with the annual law of magnetic disturbance found by me.

According to these results, I think we might perhaps be allowed to suppose that for every law of magnetic disturbance there is a corresponding law for the aurora borealis.

In this case we might conclude that the number of aurore seen in summer in Europe is much less than it ought to be, not only because of the strong twilight, but also because the evening before midnight is the epoch of minimum disturbance, and therefore probably the epoch of minimum intensity and visibility of the aurora.

Further, the decennial law of disturbance should also have its corresponding law of aurora; and if we could prove that previously the decennial law of frequency of aurora did not exist, we might perhaps conclude that the decennial law of disturbance is not continuous. On examining the numbers of aurore collected by Mairan from all sources, I find that we can draw no conclusion till the year 1699; before that time two or three yearly were all that had been registered. In 1699, however, we find a large number (40); 1719, again, is in the middle of a group of years, when an increase commences, which, however, continues till 1732. If we take the means of every three years to eliminate accidental variations, there is evidence of nothing but a gradual increase of frequency from one in 1710, to ninety in
1731, and diminution thence to *four* in 1746. The decennial period is certainly not marked in that part of the last century.

The mean diurnal variation of magnetic declination is, however, a maximum, both by Cassini’s and Gilpin’s observations, in 1787. It is perhaps worth noting the results I have been able to find on this question; they are as follow:

| Year  | 1759 | 1760 | 1761 | 1762 | 1763 | 1764 | 1765 | 1766 | 1767 | 1768 | 1769 | 1770 | 1771 | 1772 | 1773 | 1774 | 1775 | 1776 | 1777 | 1778 | 1779 | 1780 | 1781 | 1782 | 1783 | 1784 | 1785 | 1786 | 1787 | 1788 | 1789 | 1790 | 1791 |
|-------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|
|       | 10.7 | 7.2  | 7.2  | 7.3  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  | 7.4  |
| 1771-73 | 7.2 | 1792 | 9.3  | 1810-11 | 8.1 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 | 8.2 |

There are evidences in this list of a maximum diurnal oscillation in 1787, 1829 and 1837: 1847 or 1848 was also an epoch of maximum. The mean period is therefore about 10 years; the interval from 1829 till 1847 gives about 9 years; but the interval 1787 to 1847, or 60 years, may either give six periods of 10 years, or seven periods of 8-6 years.

It would appear that the maximum is not always exhibited; thus it does not appear at all in the period after 1787; on the contrary, there appears a tendency to a maximum in 1803. Again, the maximum in 1829 gives an interval of less than 8 years to the maximum in 1836-37; the next interval is, however, upwards of 10 years, as the following will probably be found to be.

If we were to assume the interval as 885 years, the period of a revolution of the lunar apsides, we should have had the maximum in the following years, counting back from 1848, 1839, 1830-32, 1821-24, 1812-15, 1803-07, 1794-99, 1786-90. The exact period is therefore not quite certain†.

* 1759, Canton, London. 1771-73, Van Swinden, Franeker. 1778-80, Cotte, Montmorency. 1784-88, Cassini, Paris. 1786-1804, Gilpin, London. 1810-11, Bowdich, Salem, U.S. 1817-20, Beaufroy, London. 1820-34, Arago, Paris. 1835-44, Gauss, Göttingen, and Lamont, Munich. These ranges are not quite comparable, as in a few cases the range is that of the mean oscillation, and they should vary slightly at the different places. The ranges from Gilpin’s observations are derived from two to twelve months’ observations in each year; but where the observations were not made in all the months of the year, a correction has been applied to obtain the range for the year.

† It is not a little curious, that the observations of M. Coulvier Gravier
I shall now offer you some remarks on the diurnal variation of the magnetic declination.

In a paper on this subject, read before the Royal Society of Edinburgh, May 3, 1847, while showing that the movement of the needle relative to midnight was of the same character as that relative to noon, I suggested that the apparent smallness of the night maximum might be due to the variation being a resultant of two opposite motions; one, the greatest, having its maximum near noon and its minimum near midnight; the other, the smaller, having its maximum near midnight and its minimum near noon. This supposition I illustrated by the superposition of two curves.

I have also shown in the Makerstoun Observations for 1844 (Trans. Roy. Soc. Edinb. vol. xviii. p. 354), that if we subtract the ordinates of the curve representing the diurnal movement of the needle for the northern hemisphere (employing the curve for summer at Makerstoun for this purpose) from the curve for the winter at the same place, the differences will be the ordinates of a curve representing the movement of the needle in the southern hemisphere.

The conclusion I drew from this fact was, that the reversal of movement with the sun north and south of the equator observed between the tropics was also shown in high latitudes; and that the midwinter curve at Makerstoun might be considered due to the superposition of the two opposite curves representing the movements in high north and south latitudes. M. Secchi has since then made a similar proposition with the same object, employing preferably the mean of the year as the typical movement (Monthly Notices, Roy. Ast. Soc. vol. xv. p. 27). I have little doubt still that the hypothesis of superposed opposed movements may be employed to obtain all the variations observed in different places at the same time, and in the same place at different times.

When M. Arago wrote his conclusion that there should be a line of no diurnal range for the declination-needle, the following were the facts in his possession. In the northern hemisphere, the north end of a needle moves towards the west from the morning till afternoon, and towards the east after noon till the evening, with a secondary oscillation at night.

In the southern hemisphere, if we consider the north end of the needle, its movements are in the opposite directions for the same periods of the day:

show a maximum in the number of meteors on the 9th, 10th, and 11th of August for the year 1848. The number increases from 1845, the time of commencement of his observations, and diminishes till 1854.—Ann. de Chim. January 1855, p. 46.
In both hemispheres the diurnal range varies with the sun's declination, being greatest in the northern hemisphere, on the whole, when the sun is furthest north, at which time it is least in the southern hemisphere; and least in the northern hemisphere when the sun is furthest south, the diurnal range being then greatest in the southern hemisphere.

The conclusion that M. Arago drew from these facts was, that there must be a line on the earth's surface where in the morning the needle moves neither to the east nor to the west, that is to say, remains stationary. This conclusion is evidently too general, and so far inaccurate. The legitimate conclusion would have been, that if there was a period of the year for which the two movements were equal in amount in two hemispheres, then there should be a line of separation where for that period the needle would rest stationary. Even this conclusion assumes not only that the amounts were equal, but that the law of the movements was exactly the same with reference to the two poles. It is not a little curious that M. Arago has cited* the "dit avoir observer" of M. d'Abbadie, "que la variation diurne de declinaison a completement change a Fernambuco du moment oü le soleil a passe d'un côte du zénith à l'autre," as a confirmation of his conclusion, while General Sabine has pointed out the same result from the St. Helena observations as proof of its inaccuracy. There can be no doubt that the statement of M. d'Abbadie gives no evidence of a line of no movement. The facts seem to have disproved M. Arago's supposition. The movement, it is concluded, is inverted; but it does not pass from one form to the other through a period of no movement, but rather by a sliding transfer of the epochs about the period of the equinoxes.

I think that the only conclusion that could have been deduced from the facts long known is the true one; namely, that for a given position of the sun there should be a line of no movement, if the movements in the two hemispheres follow the law of opposite directions, and equal amounts for similar positions of the needle and the sun. This proviso is not strictly true; and therefore, instead of a line of no movement for a given position of the sun, we should substitute a line of minimum movement. St. Helena is too far from both the terrestrial and the magnetic equators (especially from the latter) to show this result clearly; but the observations here (in 8½° N. lat. 2° south dip) will, I think, prove this, and render it probable that there are at least points of no diurnal movement for one position of the sun (probably near the nodes of the magnetic equator). The following are my conclusions as to the range of the diurnal oscillation.

The mean diurnal range of magnetic declination is a minimum

* Notices Scientifiques, vol. i. p. 491 (1854).
in high north latitudes in December; as we proceed south, it will be found, when we approach the tropics, that December is no longer the month of minimum, but the months of November and January or February; further south, near the magnetic equator, March and October are the months of least diurnal range; still further south, near the tropic of Capricorn, April or May, and September or August; and finally, June and July in high south latitudes. This curious shift of the epoch of minimum has been hitherto unknown.

The months of maximum oscillation are before and after the summer solstice in high north latitudes; before and after the winter solstice in high south latitudes; and in August and January near the magnetic equator at Trevandrum.

The amount of the daily range therefore has no relation to temperature, as indeed I have already shown from the observations at Makerstouw alone.

According, then, to the conclusion that I have shown might be drawn from the facts previously known, if all things were equal for both hemispheres, we might expect for some one position of the sun and of the needle that the sums of the positive and negative forces producing the lateral movements would be zero. If, however, from any cause (such as the unequal and unsymmetrical distribution of magnetism on the two hemispheres) the two laws should not be exactly similar, the result should at least be a line of minimum movement. The Trevandrum results will confirm this supposition. The following are the ranges of the mean diurnal variation for the months when the range is a maximum and minimum:

<table>
<thead>
<tr>
<th></th>
<th>1854</th>
<th>1855</th>
<th>1856</th>
<th>1857</th>
</tr>
</thead>
<tbody>
<tr>
<td>January</td>
<td>2.05</td>
<td>2.08</td>
<td>1.90</td>
<td>2.32</td>
</tr>
<tr>
<td>March</td>
<td>0.57</td>
<td>0.63</td>
<td>0.85</td>
<td>0.97</td>
</tr>
<tr>
<td>August</td>
<td>3.22</td>
<td>3.13</td>
<td>3.20</td>
<td>3.12</td>
</tr>
<tr>
<td>October</td>
<td>1.36</td>
<td>1.05</td>
<td>1.08</td>
<td>0.99</td>
</tr>
</tbody>
</table>

Thus the movements for August and January differ little from those of the free needle in Europe for the same months, while those for March and October are only about one-fourth of the movements of the free needle in Europe in the same months.

It will be remarked that the small range of the mean diurnal oscillation for March and October might be wholly due to the movement changing sign in the course of the month, without any diminution of the daily range. This, however, is not the case; the range diminishes from January to February, and from February to March. The range in some days of March, it is true, is frequently above 2'; but this is evidently due to disturbance, which is a maximum in that month. The real diminution of range, however, is shown quite distinctly when we take the
means for a week: thus all the oscillations of the declination-needle in the week March 20 to 25, 1854, were comprised within limits of 2′5; and all the oscillations during the three days, March 23, 24, 25, within the limits of 1′3. The tendency to an extinction of the diurnal movement is therefore certain, since these limits include all the oscillations due to irregular causes: the effect of disturbance is shown when we take the mean hourly positions for a few days, as in the week March 20 to 25, for which the range of the mean variation was only 0′49.

It is probable that the line of minimum movement for the sun near the equator is the magnetic line of no-dip, where the free needle rests nearly parallel to the axis of the earth; and, as already suggested, that the points of minimum movement on this line are those where the terrestrial and magnetic latitude are zero. It seems probable, from other considerations, that the minimum movement does not occur simultaneously over the whole line of no-dip, but that the time of minimum varies somewhat with the latitude.

Before I leave this subject I wish to draw your attention to a curious, and I think important fact, deducible from the ranges I have given. You will observe that the range in the month of March increases from 1854 till 1857; so that if March 1858 also shows a slight increase, the range for that month will be nearly doubled within four years. January also shows some increase in 1857: not so for the months of October and August; the range for these months is either constant, or appears to diminish slightly. The relation of the increase in March* to the decennial period is evident; but what shall we say of October and August? The decennial period only affects certain months at the equator in the period 1854–58. When the longitude of the sun was 0° and 270°, the increase was marked, and not at all when it was 180° and 90° in the semi-period 1854–58. I believe that other months show the increase in other periods.

I shall not at present enter into the laws of the variations of magnetic dip, nor of the force. I may say, generally, that the diurnal variation of the magnetic dip near the magnetic equator is greatest near the equinoxes when the variation of declination is least, and that it is as great in June and greater in December than in Europe. On the whole, therefore, the variations of dip are greater on the magnetic equator than in Europe.

The mean diurnal motion of the magnet freely suspended in the direction of the magnetic dip near the equator is as great as, or greater than, that in high latitudes in every month of the year.

* An increase in March might be produced by a variation of the epoch of minimum movement.
I would also refer to a curious fact with reference to the diurnal law of variation of total force, which I have never seen noticed, or indicated as remarkable by any physicist; on the contrary, it seems to have been so little marked, that Dr. Faraday, in his paper on atmospheric magnetism, has attempted to explain what is exactly the reverse of the fact (Phil. Trans. 1851, p. 115). Whether this is due to the doubt as to the accuracy of the temperature correction, or the effects of local influences, I cannot say; but as the diurnal variation of temperature for the instrument which indicates the variations of intensity in this observatory is less than three-tenths of a degree Fahrenheit, there can be no doubt here as to the fact that the total intensity is a maximum near 11 a.m., the time when it is a minimum in high latitudes. The fact is shown in all the intertropical observations, and was remarked many years ago by the late Mr. Caldecott in this observatory.

Of course the epoch of maximum intensity varies as we proceed northward and southward. In Scotland it occurs latest in summer and winter (6 p.m.), and earliest in autumn and spring, or when the sun is near the equator.

I shall at present only note the fact of the coincidence, in the inversion of the European mean laws of disturbance and total intensity at the equator, the maximum force and maximum disturbance of force occurring at the same times.

To enter in detail into the annual laws would occupy too much space, I shall therefore limit myself to a few brief observations.

Annual law of Magnetic Declination.—I would suggest to magneticians in Europe or elsewhere, before they attempt to determine this law, to observe the annual law of movement of an unmagnetized weight (or of a weight with a weak magnet attached) suspended by a silk thread like the declination-magnet. I believe they will thus find the cause of many curious differences between the results at different places, and probably why the annual law seems to depend on temperature. Cassini's observations, which have been so frequently cited, are I think worthless for this determination, as it seemed to me was proved in a paper, before referred to, read to the Royal Society of Edinburgh in 1847. I have found here that a simple weight has an annual and diurnal law of movement of its own, whether suspended by a silk thread or metallic wire, and that in a climate where the meteorological conditions are greatly more constant than in Europe.

Annual law of Force.—You are aware that I have deduced from the Makerstoun Observations, as early as 1845, that the horizontal force was a maximum near the solstices, and a minimum near the equinoxes. This law was obtained from each of several successive years' observations, and I confirmed it by an
examination of the observations at Munich and Toronto. Another period obtained by General Sabine I showed was due to error in the temperature coefficient (Trans. Roy. Soc. Edinb. vol. xvi. p. 102).

To this result I have now to add, that the relative changes of mean horizontal force are the same over the globe, and the changes from day to day of the mean horizontal force at different places on the earth's surface are nearly equal, the unit in each case being the whole value of the horizontal force at the place. In other words, the changes of mean horizontal force, from day to day, are in the same direction over the globe, and are proportional to the horizontal forces at the places; the different effect of disturbance, due to its diurnal period, and the different directions of the secular change, being allowed for.

The preceding and other facts, which I shall shortly endeavour to prove in detailed memoirs, have led me to consider anew the different hypotheses offered to explain the magnetic variations. I had already satisfied myself, from the Makerstoun Observations, that the theories depending on temperature were wholly insufficient. I showed that the areas of the declination-curves did not follow the law of temperature, as supposed by some physicists. I also showed that such areas were not the proper elements in comparison for such a purpose. If any areas were employed, it should be the areas of the curve described by the needle freely suspended in the direction of the magnetic force.

The results I have now given prove, it appears to me, the insufficiency of any theory that attempts to relate the amount of the diurnal movement to the temperature of the place. March is with us here nearly the hottest month of the year, whereas August is about the coolest; yet the former is the month of the minimum, and the latter the month of maximum diurnal movement of the declination-magnet.

None of the facts previously known seem to have been sufficiently conclusive on this point, since Dr. Faraday has proposed a new theory which depends on the temperature of the sun, acting on our magnetic atmosphere. There can be no difference of opinion as to the great ingenuity displayed by Dr. Faraday in his attempts to make his theory explain the results of observations. No doubt many of his applications are not very clear to me; thus he explains the different motions of the dipping-needle at St. Helena and Hobarton by the statement, "that as the region is located above in the air, it is above the angle which the dip makes with the horizon at St. Helena, and therefore ought to depress the line of force and lessen the dip. At Hobarton, the region being in the tropical parts, is within the angle formed by the line of dip with the horizon, and therefore deflects the
lines of force upwards and increases the dip" (Phil. Trans. p. 112, 1851).

According to Dr. Faraday's hypothesis, then, the variation of dip should be zero where the dip is zero. The law of diurnal variation ought to be opposite on opposite sides of the magnetic equator, and different on the north side of the magnetic equator, within the tropics, from that where the dip equals that at Hobarton (at Makerstoun, for example) and "the region is within the angle formed by the line of dip with the horizon." Now none of these conclusions are true.

There ought also to be some test in this place. Let us take the month of March for instance. The sun is nearly vertical; there has scarcely been a cloud seen since December; the temperature is near its maximum; the soil is red, and heated gusts of air pass as if escaped from a furnace. In October, on the other hand, the temperature is a minimum; the sky has been overcast for months, one monsoon has passed and shed its rain in deluges, and another is in full force. The lower stratum of air, a mile deep, is sheltered from the direct rays of the sun, and there is an atmospheric shell in the condition of para-magnetic polarity: ought there not to be a marked difference in the variations during these two months? and yet there is little or none.

I have now, in correspondence with his Highness the Rajah of Travancore's observatory here, another observatory on the highest and sharpest peak of our Ghats, supported also by His Highness the Rajah. You will see from the report published, of which I have sent you a copy, an account of the erection of this observatory. It is exposed for some months to the sun that is burning up the eastern side of the Peninsula, while the western side, Travancore, is covered by a sea of vapour. I have a series of observations, magnetic and meteorological, made on this peak hourly during upwards of two years. From these observations I believe I shall be able to add further evidence of the complete insufficiency of any temperature theory in accounting for the magnetic variations.

The following I shall offer partly in the form of questions:—

Does not the sun act as a magnet, perhaps as an electro-magnet, the currents forming it being within its atmosphere? Are not the solar spots disruptions of the current due to the positions of the planets with reference to the plane of its equator? After a magnetic disturbance there is a diminution of force shown on the earth, which remains for some days as if there had been a violent action with the result of a loss of energy. The connexion of the solar spots with the frequency of the aurora borealis (and therefore, as we now know, with the magnetic disturbance) was remarked by Mairan (Traité, &c. p. 264, 2nd edit.),
but it appeared to him that the coincidence then remarked had not always existed.

Having found that the magnetic disturbance was greatest not only when the sun was on the equator, but also when the moon was on the equator, I was induced to compare the times when the lines of the moon’s nodes and of the apsides would nearly agree with each other, and the position of the sun at the equinoxes. I found that the longitude of the moon’s ascending node was 0° in 1838 and 1857, while it was 180° in 1848; and that the moon was in perigee when its longitude was 0° in the same years, which were those of maximum disturbance. The times of revolution of the nodes and perigee are well known to be about 19 and 9 years respectively.

If the coincidence of the decennial laws of frequency of the solar spots and of magnetic disturbance be not accidental, we should have a correspondence between the laws of the aurora and of the solar spots. Do M. Schwabe’s observations show maxima of frequency at the equinoxes? If so, we must conclude that the earth has something to do in the formation of the solar spots, and therefore probably other planets, which would introduce other periods. Jupiter, whose revolution occupies about twelve years, was in longitudes of about 180° in the years 1826, 1838, and 1850.

It is not the case that the magnetic disturbance coexists always with the spots; but it is not improbable that during the formation of the spots the disturbance is produced, that is to say, at the period when the supposed discharge of the sun’s electrical atmosphere occurs.

If the sun acts as a magnet, how does it produce the magnetic variations? It is probable that it acts directly on the suspended magnet, but that this action is quite secondary. The diurnal variations would be produced by a shifting of all the lines of equal declination, inclination, and force usually represented as due to the action of the terrestrial magnet. Are not the magnetic variations, then, due to the shifting of these lines, by the inducing action of the solar magnet, the direction of the shift and of the lines determining the epochs of maximum and minimum?

That the sun might act as a magnet is of course not a new idea. It had occurred to myself whilst investigating the lunar magnetic influence, twelve years ago, that some of the results obtained might be due to the sun’s rotation on his axis; a similar idea occurred to M. Kreil, but an examination of the results seemed, in both cases, to prove the inaccuracy of the supposition. The results at which I have arrived lately, however, have induced me to examine the question more carefully. If the sun acts as a magnet, it is probable, from the analogy of our earth,
that its magnetic poles will not coincide with the poles of rotation; perhaps even the poles may have unequal forces. In such a case, it might be expected that the fact could be determined by our magnetic observations. It happens, however, that the period of the synodical rotation of the sun (employing the results obtained by Bianchi and Laugier, and given by Sir John Herschel as the best determination, namely $25^d 7^h 48^m*$) is exactly that of a nodal revolution of the moon. It might therefore be difficult to determine whether any result obtained was not due to the moon’s varying position relatively to the plane of the equator.

The result which I have now obtained from three years’ observations near the magnetic equator, it appears to me, is wholly independent of the moon, and is due to the sun’s rotation on its axis.

If we could suppose that the solar magnetic poles are fixed, it might then be possible to determine accurately the time of the sun’s rotation by means of the movement of our magnets. If, on the other hand, the poles are in motion, as I conceive they are, we shall have to employ another period than $25^d 325$, as obtained from the solar spots. The period to be employed will of course be found by careful examination of the observations, and by trial. Such a movement of the solar poles might explain the secular magnetic variations.

It is not improbable, after all, that some of the results obtained previously, as due to the moon, are really due to the sun’s rotation.

Supposing that I have proved the sun’s magnetic action, it has occurred to me that the fact might be applied to give some ideas for a theory of comets. Sir John Herschel, I believe, has somewhere suggested electricity as the cause which directs the tails of comets. I have looked over the different hypotheses given by M. Arago in the recent edition of his works, where he professes to omit none; but no polar hypothesis is to be found there.

Are not comets formed of magnetic gases? Is not the tail of the comet due to the directive action of the solar magnet, the curvature of the tail, sometimes seen, being due to the position of the solar magnetic poles relatively to the path of the comet? Is not the condensation of the comet, when approaching the sun, a phænomenon similar to those observed by Dr. Faraday and M. Plücker in their recent researches on the action of the poles of a magnet on certain gases or liquids? might not a like illustration be given of the varying form of the tufts in the nucleus?**

---

* Outlines of Astronomy, p. 232.
+ Since this was written I find that Bessel considered that the vibrations of the tail of Halley’s comet, observed by him, indicated the action of a polar force. Arago has made no mention of this hypothesis, apparently because he doubted the results on which it was founded (Astronomie Populaire, vol. ii. p. 396).
Is not the zodiacal light the magnetic æther in a luminous state, repelled by the solar magnetic poles? Does not the zodiacal light revolve round the sun? If so, what is its period of revolution? Are not the extent and intensity of the zodiacal light related to the periods of the spots, as Cassini and Mairan supposed*?

From the known action of the sun on the gases of comets, may we not infer some action of the sun on the gases forming our own atmosphere?

I shall not enter here into the reasons that I can produce, to show that the diurnal oscillations of the barometer do not depend on the diurnal variations of temperature as their chief cause; neither can it be explained by M. Dove's ingenious addition of the variation of the pressure of aqueous vapour, which is purely a local phenomenon. I think I have shown the insufficiency of the theory founded on the combination of these causes, in the Makerstoun Observations: it is insufficient both in Europe and India. We require a cause like that of the solar and lunar attraction on the mass of the ocean to produce a double tide daily. May not the facts known exhibit such a cause†? Should not the sun acting as a magnet on the magnetic gases forming our atmosphere, and by induction on the terrestrial magnet, cause the atmosphere to assume an ellipsoidal form, having the greater axis in or near the plane of the equator; thus determining the greater diurnal oscillation in the equatorial regions. If the form which the atmosphere assumed, under the influence of the terrestrial and solar magnets, were somewhat irregular (as in some of the figures assumed by magnetic liquids between the poles of a magnet, as in M. Plücker's experiments), we might explain in this way the diminished mean atmospheric pressure near the equator, and the maximum pressure 20° north and south of it. I am not aware that Dr. Faraday or M. Becquerel has determined the specific magnetism of air containing vapour in the form of cloud. Is not the specific magnetism of air diminished by containing aqueous vapour? and is not the diminution proportional to the relative humidity of the air, rather than to the absolute amount of watery vapour? If so, the diurnal oscillation of the barometer should diminish ceteris paribus with the relative humidity of the air‡.

* It seems to me that the zodiacal light is much brighter and longer this year than I have remarked it during the last five years. This impression, however, is not founded on real measures, as I had made none in the preceding years.

† Mr. Joule has found that in magnetizing an iron bar it is lengthened (without change of volume) in the direction of the magnetic axis.

‡ I have remarked, in several of the results of the magnetic observations made in Scotland, the coincidences with results for the atmospheric press-

The preceding facts and queries were addressed to you two months before this date; the letter was then shown to different persons, and among others to Lieut.-General Cullen the British resident here, with whom you are no doubt acquainted as a zealous inquirer in geology and terrestrial physics for the last half century in India. I had also communicated my results and the greater part of my guesses to that gentleman before.

I am, my dear Sir,

Yours very truly,

John Allan Broun.

P.S.—Since this letter was written I have seen in the Bibliothèque Universelle de Genève, July 1857, a translation of a memoir by M. Secchi on the periodic variations of terrestrial magnetism. This is a continuation of a former paper translated in the Philosophical Magazine for November 1854 and June 1855, and it draws from me a correction and a few remarks.

In his second paper, M. Secchi offers some laws of the movements of a needle freely suspended in the direction of dip. These movements were obtained by me first, I believe, from all the three magnetic instruments, and exhibited to the British Association in 1846; the curves produced by the movement of the north end of the needle were given for each month of the year, in the volume of Makerstoun Observations which M. Secchi has consulted; yet he says, "Mr. Broun has given analogous curves for Makerstoun, and a glance at these will show the same law, though somewhat more complicated from having grouped too many months together, and from the higher latitude and frequent disturbances." M. Secchi has examined plate 8 instead of plate 7, which contains the curves for each month (Trans. Royal Soc. Edinb. vol. xix. p. ii.). In plate 8 the figures are given for periods of 60 and 90 instead of 30 days; but this combination can scarcely be said to complicate the curves, as an examination of plate 7 will show. In plate 8, however, I have also projected the curves obtained from undisturbed movements, and have thus shown the effect of disturbance in displacing the curves. Various conclusions deduced from these figures are given in pages lxx–lxxiv; they are the only ones of the kind that have been projected, as far as I know, till M. Secchi has now constructed one for the whole year (Bibliothèque de Genève, p. 164). This curve, of course, may be considered complicated, and I have noted a resemblance between the annual law of difference of atmospheric pressure at Makerstoun and Greenwich, and the annual law of horizontal magnetic force. Do not the differences of the atmospheric pressures at the same two places, from year to year, show some resemblance to the decennial law, 1843–44 being an epoch of minimum? (Trans. Roy. Soc. Edinb. vol. xix. part 2. p. xei.)
since the figure deduced from the mean of 365 days cannot be said to resemble that for any day of the year.

M. Secchi deduces from his curve the following conclusions:

"Second Law.—The disturbed curve is the ordinary curve entirely displaced by a certain quantity." A glance at the Makerstoun curves (plate 8) will show that this conclusion is inexact. Doubtless there is a displacement, and, as the effect of disturbance is small compared with the whole movement, the curve cannot be greatly changed in form; but the change of figure is quite as great as the amount of perturbation admits.

M. Secchi also finds—

"Third Law.—By the effect of the disturbances, the curve tends always to become more symmetrical and equal in its two lobes."

Again, it appears to me that a glance at plate 8 will show that this conclusion is not warranted. It seems to me, on the contrary, that the effect of the disturbance is to render the figures less symmetrical, the curves for March and April perhaps excepted. The tendency of the disturbance is to introduce a loop in the easterly or westerly movement; and this is so evident that it seems probable, if we could obtain curves perfectly free from disturbance, the loop to the east in the night would not exist.

M. Secchi adds, "These conclusions, deduced from the observations of the three magnetic elements at Toronto, will be verified without doubt in other countries for which the discussion has not yet been performed." I refer to the Makerstoun Observations, where the discussion was published eight years ago, and where all the details will be found. With reference to his fourth law, given in a former part of this letter, namely, that "the disturbances are a maximum at the equinoxes, and a minimum at the solstices," M. Secchi remarks, "2nd. At the solstices, the poles of the sun are turned towards the earth, so that its magnetic action should be more powerful." Solstices must have been written by mistake for equinoxes, since it is near the latter periods that the poles of the sun are turned towards the earth; but the conclusion is a statement of the result obtained by me, and referred to previously under the head of "Annual law of Force," on the supposition that the north pole of the sun contains the same magnetism as the north pole of our earth.—J. A. B.

2nd P.S. I should have mentioned a fact in connexion with the history of the secular movements that I have not yet published. The magnetic declination is nearly half a degree east at Trenvandrum. Before the year 1854, the movement was towards the north, that is, westerly. I have found that it began to turn easterly in the year 1854, and that the rate of motion yearly towards the east is becoming greater and greater.—J. A. B.
X. Note on the Sun's Spots. By Daniel Vaughan, Esq.*

In my article published in the May Number of the Philosophical Magazine, I endeavoured to show that the heating and illuminating action of suns, if maintained by the ætherial contents of space, can fluctuate only through the influence of surrounding worlds. But, though I offered an explanation of the periodicity of the solar spots, I deem it advisable to point out the precise manner in which they may arise from the causes to which I have ascribed them, and thus to furnish a more satisfactory basis for the tests of observation.

It appears, as a legitimate deduction from my theory on solar light, that the dark spots must be deficient in number, or entirely absent, when the sun is advancing into the realms of more dense æther; and that they must be most numerous and extensive when he is departing from these localities and entering a more rarefied ætherial fluid. In the first case, his permanent atmosphere, which takes no part in the emission of light, must be heated most intensely in its uppermost strata, and thus brought into a state of the greatest security from disturbances. But when the supply of ætherial fuel from space gradually diminishes, and heat declines near the surface of the vast ocean of incombustible gases, the elevated temperature previously transmitted to great depths would produce the most violent commotions of its æeriform matter. To such storms or swellings in the sun's non-luminous envelope we may ascribe his spots. The transparency of this gaseous appendage, together with the effects of flame in intercepting the range of vision, will account for the fact that the spots appear as hollows in the sun's disc.

If an astronomer, situated at the moon, were engaged in a telescopic survey of the earth, he could distinguish volcanic eruptions from many of our most violent tornadoes only by observing how the latter changed their position with respect to other terrestrial objects. It is only by a similar test that we can decide whether the sun's spots are to be regarded as indications of great storms in his atmosphere, or of the emission of gaseous fluids from his internal regions. The evidence which observation has hitherto afforded seems favourable to the first hypothesis. But it must be extremely difficult to form any correct ideas of the changes which occur beneath the sun's surface, from the effects of a violent heat in volatilizing his materials, while the influence of an enormous pressure is exerted in maintaining them in a solid or a liquid condition.

Cincinnati, Ohio, June 25, 1858.

* Communicated by the Author.

Although it occurred to Schweigger, soon after the discovery of his multiplier, to bring salts in the fused state, instead of their aqueous solutions, between two dissimilar metals in the voltaic circuit, yet Andrews in Belfast was the first (in 1837) to apply Faraday's discovery of the high conductive power of certain salts in the melted state to the solution of the question whether electricity is developed when two metals of the same chemical nature, but of dissimilar temperatures, are brought into contact with the same fused salt, in a manner similar to that in which the thermo-electrical currents of Seebeck are formed. Andrews found that, under the circumstances described, a current was formed, proceeding from the hotter to the colder metal, passing through the melted mass of the salt. He was, however, unable to determine the law for the direction of the stream when the salt was only heated so far as to enable it to conduct the electricity, but not sufficiently to bring it into a state of fusion.

The interest attached to these electrical phænomena appeared to me to render a re-examination of the matter necessary. The following paper, which I have the honour to lay before the Königliche Gesellschaft der Wissenschaften as the third in the series of my electrical investigations, contains the results of my experiments with numerous salts concerning the development of electrical currents under the conditions described.

The paper is divided into two parts, the first of which contains the general results derived from the facts observed; the second contains these facts in a condensed form, and thus serves in a manner as an appendix to the first part.

I shall in the first place describe, for the sake of perspicuity, the process which I have made use of in my investigations, and then consider shortly the general results which have been derived from them.

The salt under examination was brought into a moderately sized platinum crucible supported on platinum wire, and fused by means of a spirit-lamp with double draught, the draught being, when necessary, increased by the use of the bellows. An isolated platinum, gold, or silver wire was introduced from above, penetrating to the required depth into the saline mass. When electrical currents had to be observed, the crucible was brought, by means of its wire support, into connexion with one end-wire of a multiplier, the platinum wire in the salt being in connexion with the other wire of the multiplier. When, on the other hand,

* Translated by Dr. F. Guthrie from Poggendorff's Annalen for April 1858.
electrical tension had to be measured, either the crucible (its support being isolated) or the internal wire was connected either with the gold-leaf of the electrometer (see part 1 of my electrical investigations, vol. v. of the Abhandlungen der Königlichen Gesellschaft Sachsens, where the electrometer is denoted by $A$), or with one plate of a condenser, the wire or the crucible being respectively in connexion with the earth. For the sake of brevity, I shall call a current proceeding from the crucible to the wire an ascending current, one from the wire to the crucible a descending one.

If the melted mass of the salt be allowed to cool after the wire has been introduced, and be then heated again, at the commencement of the heating an ascending current is formed in the solid salt, if it conduct at all: the current passes from the hotter to the cooler metal.

As soon as the salt in contact with the sides of the crucible begins to melt, cause is given for a descending current; at the same time, however, the temperature of the wire is generally superior to that of the wire in the still solid mass of the salt. The direction in which the needle of the galvanometer is deflected in consequence of the two electrical forces, which act in exactly opposite directions, that is, whether it be deflected in accordance with an ascending or descending current, depends upon the relative strengths of these two forces. In many cases, not only the strength, but even also the direction of the deflection may be changed at will by altering the relative magnitude of these forces.

After the fusion is complete, an ascending current is formed (except in the case of the sulphate of potash and copper), as the difference of temperature would lead us to expect.

On extinguishing the lamp before the salt begins to solidify, the relation of the two metallic poles as to temperature is the inverse of what it was during the application of the heat, and consequently an ascending current will be formed. [Sulphate of potash and copper, nitrate of potash and nitrate of soda are exceptions to this rule; in these salts a descending current occurs.]

The subsequent solidification, whereby the side of the crucible is in contact with solid and the wire with liquid matter, gives rise to an ascending current, that is, to one in an opposite direction to that caused by the previous melting of the mass. Whether, however, this cause is sufficiently powerful to determine the deflection of the needle in the corresponding direction, and how great such deflection may be, depends, as before, upon the relative magnitude of the electrical forces called into action on the one hand by the difference of temperature (of the platinum-
between Metals and Heated Salts.

103

poles), on the other by the contact of bodies in different states of aggregation.

After solidification, in those cases where the salt still continues to conduct, there is, generally speaking, an ascending current which is due to the difference of temperature. It is probable that the anomalous appearance of a feeble ascending current at the termination of the cooling of nitrate of potash was a consequence of the difference of temperatures of the platinum of the crucible and of the wire.

A mixture of equivalent quantities of carbonate of soda and carbonate of potash forms at first, on cooling, a greyish opal mass, which after some time is changed into a white crystalline salt. This change from one condition into the other appears also to be accompanied by a peculiar development of electricity, the current passing in an ascending direction. In the same manner, on applying heat, the inverse transformation gives rise to a current in the opposite direction, that is, a descending one.

In the cases of carbonate and of phosphate of soda, quite at the end of the cooling, after a descending current has been observed for a long time, a weak ascending current makes its appearance, the formation of which is probably due to a polarization caused by the previous passage of the descending current. I have satisfied myself, by special experiments, of the possibility of such a polarization.

When salts, such for instance as borate of soda, solidify after fusion, and after solidification separate of themselves from the sides of the crucible at a time when the salt still enjoys the power of electrical conduction, a descending current is brought about. If the separation be sudden this current is strong, but of only momentary duration; if the separation be gradual, the current continues during the separation, but is of less strength.

If borax which has separated from the crucible in the above-described manner be immediately heated again, the re-establishment of contact between the salt and the crucible is accompanied by the development of a current, which passes also in a downward direction. The cause of this current is most probably the sudden cooling which the strongly heated platinum undergoes on coming in contact with the cooler salt, inasmuch as similar currents occur when an air-bubble of some size, formed on the side of the crucible, rises through the pasty mass of melted borax, and bursts.
The end of chemistry is its theory. The guide in chemical research is a theory. It is therefore of the greatest importance to ascertain whether the theories at present adopted by chemists are adequate to the explanation of chemical phenomena, or are, at least, based upon the true principles which ought to regulate scientific research.

Among those which have lately been developed, there is one, on account of its apparently numerous merits, which particularly claims investigation, and respecting which we deem that it would not be unprofitable were either new proofs of its scientific value furnished, or, on the contrary, should considerations be adduced establishing not only its inadequacy to the explanation, but its ultimate detriment to the progress of science. I allude to the system of types as advocated by Gerhardt.

This system, striking alike for the breadth of its conception, and the logical and consequent manner in which it has been developed, has been controverted from the point of view afforded by theories less far-reaching than the one under consideration, and even based upon a one-sided and restricted appreciation of certain chemical reactions. The consequence is that this opposition has not impaired the favour with which the unitary system has been received, but has rather tended to display it in a more advantageous light.

Imposing as this theory is, it is nevertheless all the more necessary to submit it to a strict investigation; for there is nothing so prejudicial in the search for truth as the blind spirit of conservation. A rational belief demands the test of a preliminary doubt.

There are two conditions which every sound theory must fulfil:

1. It must be proved to be empirically true.
2. It must no less be philosophically true.

I admit that this theory is for the most part empirically true, that is to say, it is not contradicted by many of the facts of the science. Evidence that this condition is only partially fulfilled, is to be found—

1. In the circumstance that the peroxides, for instance, do not fit very satisfactorily into the types.
2. The principle of double decomposition cannot well be applied to the conversion of the anhydrous sulphuric acid into the hydrate of that acid by the action of one equivalent of water.

* Communicated by the Author.
the formulæ of these bodies being, according to Gerhardt, in their free state O,SO and H,O. Combined, they become simply SH² O².

The same remark applies in like manner to carbonic acid. In these instances the wanted consequence of Gerhardt is missed. The fact of the density of the vapour of these bodies being the same in the free as in the combined states, may have prevented him from doubling the formulæ of these anhydrous acids. The types of this theory being essentially types of double decomposition, this instance of simple combination diminishes somewhat the value of the otherwise great logical merit of this system.

Having taken notice of such exceptions, the empirical truth of the theory may be otherwise admitted.

The philosophical test demands that a theory be competent to explain the greatest number of facts in the simplest possible manner.

In applying this test, three aspects of it require to be taken into consideration:—

1. As to the extension of the theory.

2. The explanation it affords of the facts.

3. The manner of this explanation.

As to the first: this theory indeed brings every chemical combine under a certain comparative point of view with every other. Herein apparently is its merit. Nevertheless, should our test be applied to its full extent, it will be found that it is fatal to this system, in other respects so imposing. The comparative point of view which it adopts is fundamentally false.

As to the second: it does not explain the facts at all; consequently the most essential point of the test is unfulfilled.

3. This condition of the test is in like manner unfulfilled, from the fact of the second not being complied with.

Why is it that Gerhardt’s theory so signally fails in these two essential requisites? Because it is based upon an old but vicious principle, which has already retarded science for centuries. It begins with a generalization, and from this generalization deduces all the particular instances. But it does not come within the limits of a chemical paper to enter upon a discussion which is purely metaphysical. Nevertheless the theory of Gerhardt can only be combated upon metaphysical grounds, because it is only in overturning a general principle of research that the theory can be proposed. Gerhardt’s generalization lacks, moreover, the merit of being represented by a type having a known existence. nO II, from which he derives every chemical combine, being in itself indefinite, cannot of course be contained or be produced in any definite body. That, however, which may be demanded
of the type is, that in itself it should afford at least an instance of that which it is meant to represent. Now the part \( n \) of the type represents the notion of indefinite multiples of \( \frac{O}{H} \). But not a single instance of a multiple of \( \frac{O}{H} \) has been proved to exist; much less has it been proved that there exists, or can exist, multiples of this body in an indefinite series. The perfection or imperfection of the type meant to represent the generalized notion, is, however, a matter of comparatively inferior moment. It is the principle involved in this generalization which is essentially pernicious.

Should the principle which is therein adopted be applied to the common events of life, it will be found that it is simply absurd. Suppose that some one were to systematize the formation of letters into words that formed the contents of a book. Were he to begin by saying that he had discovered a certain word which would serve as a type, and from which by substitution and double decomposition all the others are to be derived,—that he by this means not only could form new words, but new books, and books almost \textit{ad infinitum},—that this word also formed an admirable point of comparison with all the others,—that in all this there were only a few difficulties, but that these might be ingeniously overcome,—he would state certainly an empirical truth. At the same time, however, his method would, judged by the light of common sense, be an absurdity. But a principle which common sense brands with absurdity, is philosophically false and a scientific blunder.

Suppose the book that had formed the basis of this system were a German one, where all the words were found to be composed at least of two letters, still even in this language the viewing and systematizing of words as a series of double decompositions would be no less ridiculous.

The sure and invincible method of arriving at every truth which the mind is capable of discovering is always one and the same. It is that, namely, of throwing away all generalization, of going back to first principles, and of letting the mind be guided by these alone. It is the same in common matters. It is the same in science. To reach the structure of words we must go back, seek out the undecomposable elements, viz. the letters, and study carefully their powers and bearing. Having ascertained these, the composition and structure of every possible word is revealed. It would be well to call to recollection the parallelism of chemical research with that of every other search after truth; for it has been in overlooking this that in chemistry false and vacillating theories have been advocated and a wrong route so
often pursued. In mathematics the starting-point is not generalizations, but axioms, ultimate principles. In metaphysics Descartes led the way of progress by analysing till he thought he could reach some ultimate elements beyond which it was impossible for him to go, then studying their force and power, and proceeding synthetically. The recognition of this method wrought the regeneration of science and philosophy.

On the other hand, look where Gerhardt's generalization of Williamson's generalization leads him, and legitimately too,—a fact which his logical spirit clearly discerned. He is led not to explain bodies according to their composition and inherent properties, but to think it necessary to restrict chemical science to the arrangement of bodies according to their decomposition, and to deny the possibility of our comprehending their molecular constitution. Can such a view tend to the advancement of science? Would it not be only rational, in accepting this veto, to renounce chemical research altogether?

These reflections naturally lead to the inquiry after another theory more adequate to satisfy the just demands which can be made upon it. There is one which, as it is still supported by many distinguished chemists, cannot be passed over altogether unnoticed. It is that of the theory of certain combines in organic chemistry which are to be viewed as analogous to, "playing the part of," inorganic elements. These are denominated radicals, and are supposed to be contained in all organic chemical products.

In addition to this, and also in connexion with it, there is a doctrine describing many combines to be copulated, conjugated, by addition.

It is impossible here to enter upon any extensive criticism of this theory. I can only remark that it is not merely an unprofitable figure of language, but is injurious to science, inasmuch as it tends to arrest scientific inquiry by adopting the notion that these quasi elements contain some unknown and ultimate power which it is impossible to explain. It stifles inquiry at the very point where an explanation is demanded, by putting the seal of elements, of ultimate powers, on bodies which are known to be anything but this.

Science demands the strict adherence to a principle in direct contradiction to this view. That first principle, without which research cannot advance a step, dare not be ignored; namely, that a whole is simply a derivative of its parts. As a consequence of this, it follows that it is absolutely necessary to scientific unity and research to consider these bodies as entirely derivative, and as containing no secret ultimate power whatever, and that the properties which these so-called quasi elements possess are a
direct consequence of the properties of the individual elements of which they are made up.

Nor is the doctrine of bodies being "conjugated by addition" a whit in advance of that which I have just been considering. This doctrine adopts the simple expedient of dividing certain combinates, if possible, into two imaginary parts, of which one or both are bodies already known. Then it tells us that these two parts are found united in this body. But how they are united, or what force binds them together, it does not inquire. Is this explication arbitrary? Is it instructive? Is it science?

I may now be permitted to submit a few considerations relative to a more rational theory of chemical combination.

As everything depends upon the method of research employed, it will in the first place be necessary to find one that may be relied upon. If the method is good and conscientiously carried out, stable and satisfactory results may be expected. If, on the contrary, it is vicious, we can only expect a corresponding issue. A satisfactory method is, however, not difficult to find, nor is it difficult in its application.

The principle which ought to guide all research is in every case the same. It is that of analysing till it is impossible to reach more simple elements, and of studying these elements in all their properties and powers. When all the properties and powers of the individual elements are known, then it will be possible to know the constitution of the combinates which their synthesis produces. It is necessary therefore in chemical research, in order to ascertain the various qualities and functions of the different elements,—

1. To consider the whole of chemistry as one.
2. To take into consideration every known combine, and to study the character, functions, and properties displayed by each element for itself, in each of these combinates in all their different conditions and aspects. It is by a comparison of the different bodies among themselves that we are able to trace the part that is performed by each element separately.
3. To trace the general principles common to all the elements, noting the special properties of each.

This method is essentially different from that where one class of bodies is chosen as a point for the restriction of our views of the properties of the others—where only the qualities found in the first are to be measured out to the rest.

I shall now proceed to inquire how its more thorough application tends to the development of a rational chemical theory.

It has been found that there is one leading feature, one in-
herent property, common to all the elements. It has been denominated chemical affinity. It is discovered under two aspects:—
(1) affinity of kind; (2) affinity of degree.

Affinity of kind is the special affinities manifested among the elements, the one for the other, &c., as carbon for oxygen, for chlorine, for hydrogen, &c.

Affinity of degree is the grades, or also limits of combination, which the elements display. For instance, \( \text{C}^2\text{O}^2 \) and \( \text{C}^2\text{O}^4 \) are the degrees of affinity of carbon for oxygen. \( \text{C}^2\text{O}^2 \) may be called the first degree, and \( \text{C}^2\text{O}^4 \) may be termed the second degree, and, as a higher degree than this is not known for carbon, its ultimate affinity or combining limit. Affinity of degree in an element may have only one grade. It may have, however, and generally has more than one. Here then is an inherent property common to all elements, by the removal of which the chemical character of an element will be destroyed, and by virtue of which an element finds its place marked out in a complex body.

It is such a property that is required to form the base of a system. Nor would its suitableness for this purpose be affected by the discovery that the elements are themselves composite bodies, which view the chemist is perhaps not unwarranted to adopt. For in such a case the necessity would doubtless still be found to exist of adopting the principle of affinity, or something at least equivalent to it, as the basis of the explanation of chemical combinates. In applying this method, I propose at present to consider the single element carbon. This body is found to have two highly distinguishing characteristics:—

1. It combines with equal numbers of hydrogen, chlorine, oxygen, sulphur, &c.

2. It enters into chemical union with itself.

These two properties, in my opinion, explain all that is characteristic of organic chemistry. This will be rendered apparent as I advance.

This second property is, so far as I am aware, here signalized for the first time. Evidence as to its being a property of carbon may therefore be required.

It will be found in the following:—What is the link which binds together bodies composed of 4, 6, 8, 10, 12, &c. equivalents of carbon, and as many equivalents of hydrogen, oxygen, &c.? In these you may remove perhaps all the hydrogen or oxygen, and substitute so many equivalents of chlorine, &c. It is then the carbon that is united to carbon. Further, that it is not the hydrogen that is the binding element in these combinates is evident; thus—
Here the whole four of hydrogen are not bound by a mutual affinity; for each element of hydrogen can be substituted for one of chlorine in regular series, beginning with the first and ending with the last. The atoms of oxygen are, on the contrary, united in pairs (which will be more fully developed hereafter), and only for two atoms of oxygen two of chlorine can be substituted; thus—

\[
\begin{array}{ccc}
H & O & Cl \\
C^2H & C^2O & Cl \\
H & O & Cl
\end{array}
\]

In the same manner with bodies that contain multiples of C\(^2\) united to hydrogen, &c.

Take the inverse of this. If the four atoms of hydrogen were bound together, we could evidently expect to form such bodies as

\[
\begin{array}{cccc}
Cl & Br & Br & Br \\
H^4Cl & H^4Cl & H^4Br & H^4Br \\
Cl & H^4Cl & Cl & Cl
\end{array}
\]

or for bodies like C\(^4\)H\(^4\), C\(^6\)H\(^6\), C\(^8\)H\(^8\), one would naturally expect to find the carbon substituted for chlorine, and find bodies like H\(^4\)Cl\(^2\), H\(^6\)Cl\(^6\), H\(^8\)Cl\(^8\) &c.

These bodies are not only unknown, but the whole history of hydrogen might be investigated and not a single instance be found to favour the opinion that it has any affinity for itself when in union with another element.

Now, on the other hand, carbon remains chemically united to carbon, while perhaps 8 equivalents of hydrogen are exchanged for 8 equivalents of chlorine, as in naphthaline. Analogous to this is the conversion of alcohol, C\(^4\)O...OH\(^5\), and the hydrocarbon C\(^4\)H\(^6\) into C\(^4\)Cl\(^6\). All the countless instances of substitution of chlorine, &c. tend in the same direction. They prove beyond doubt that carbon enters into chemical union with carbon, and that in the most stable manner. This affinity, one of the strongest that carbon displays, is perhaps only inferior to that which it possesses for oxygen.

Another feature in the affinity of carbon is, that it combines by degrees of two; thus, C\(^2\)O\(^2\) and C\(^2\)O\(^4\), C\(^4\)H\(^4\) and C\(^4\)H\(^6\),
C\(^6\)H\(^6\) and C\(^6\)H\(^8\), C\(^8\)H\(^8\) and C\(^8\)H\(^{10}\), &c.: from these last it is especially evident that two is the combining grade of carbon. It becomes still more apparent when we compare the bodies

\[
\begin{align*}
&\text{C}^4\text{H}^4 \\
&\text{and C}^4\text{H}^5\text{Cl, that is, C}^4\text{H}^4 & \text{\&c.}
\end{align*}
\]

be added, while on the other hand there are no instances contradictory of this point. Hence the circumstance that it must ever remain impossible to isolate a combine of the form C\(^2\)H\(^3\) or C\(^4\)H\(^3\), &c.

Carbon having only two grades of combination of two atoms each, a fact which is easily traced throughout all organic chemistry, this inherent property of the element may legitimately furnish two grand types for all its combines.

The first type will be nC\(^2\)M\(^4\).

The second type will be nC\(^2\)M\(^4\)—mM.

As examples belonging to the first type, may be mentioned the alcohols of the æthyllic form, their æthers, the fatty acids, &c.

Thus methyllic alcohol has the formula C\(^2\)...O...OH, and æthyllic

\[
\begin{align*}
&C^2...\text{O}...\text{OH} \\
&C^2...\text{H}^3
\end{align*}
\]

In these instances it will be observed, that for each double atom of carbon the combining power is (4) four, which is the ultimate limit of combination for carbon in all bodies yet produced.

In the latter instance it is apparent, inasmuch as if the combining limit of two C\(^2\)s be each reduced by 3 in hydrogen or oxygen, there still remains a combining power of one to each of the two C\(^2\)s which each expends in uniting with the other;

\[
\begin{align*}
&C^2...\text{H}^3 \\
&\text{therefore } \text{C}^2...\text{H}^3, \text{or what is the same thing, } \text{C}^2...\text{H}^3 \text{ belongs to the type } n\text{C}^2\text{M}^4.
\end{align*}
\]

Again, the inherent properties of the elements may be viewed as dividing bodies into primary, secondary, tertiary, and so on combinates. These may be termed so many orders of complicity. Thus C\(^4\)H\(^6\) is a primary combine, or it belongs to the first order of complicity; but C\(^4\)...O...OH is a secondary combine, or belongs to the second order of complicity. C\(^2\)O\(^2\) and C\(^2\)O\(^4\) are primary, while C\(^2\)O\(^4\), 2OH and C\(^2\)O\(^4\), 2OKa are secondary.

A primary combine is then nC\(^2\) united to nM\(^4\) or to nM\(^4\)—mM\(^2\).
in such a manner that the combining energy of the complement (nM⁴, &c.) either potentially or actually does not extend beyond nC².

A secondary combine is one in which the combining energy of the complement is not all expended upon nC², but is extended further to one or more elements.

On the same principle there are tertiary combines, &c.

These orders of complicity ought in reality to be subdivided. This, however, I do not think it necessary for the present to enter upon. It will now be understood why an alcohol belongs to the type nC²M⁴, and on the same principle why a free aether belongs to the same type, thus

C²...O...O...C²

at the same time secondary combines.

A secondary combine, that is to say, a body belonging to the second order of complicity, is, as will be understood from the principle which forms the ground of the rational theory, a direct consequence of an inherent property of one or more of the elements which form the complement to the carbon.

In the instance before us, it is a certain property of the oxygen which is the cause of the secondary combine. This property is the affinity which one atom of oxygen in combination always exerts towards another atom of oxygen likewise in combination.

This affinity is modified by the electric position of the element to which the respective atoms of oxygen are bound. From this property results the fact, that in organic combines the atoms of oxygen are always found double.

For instance, the combining limit of oxygen being two, when

two molecules of

C²...O...

the oxygen instantly produce the union of these molecules. The cause of the union of two molecules of C²H³ has been already remarked. In the two cases, the causes of the union of the respective molecules are in so far different, that the one is the result of a property of the carbon, while the other is the result of a property of the oxygen.

The view here adopted of the nature of oxygen is, I am convinced, alone in conformity with the reactions where the properties of this body develope themselves.

The vapor of anhydrous sulphuric acid, for instance, is conducted into anhydrous aether. The following will then be the reaction:—
the two atoms of the oxygen of the sulphuric acid and the two atoms of the oxygen of the æther (now in presence of each other) being in different (perhaps different electric) conditions, mutually loosen their former affinities and reunite themselves to the (electrically ?) different atoms of oxygen of these respective combinates.

The same principle may naturally be expected to display itself with regard to acids and bases. The oxygen of an acid unites itself to the (electrically ?) different oxygen of water. The oxygen of a base on the same principle has an affinity for the electrically different oxygen of water.

It will be observed—

1. That the oxygen of the water of an acid can only be expelled by that of a base, and vice versa.

2. It is to be remarked that it is not the metal of a base which exchanges places with the hydrogen of the hydrate of an acid; for if that were the case, the affinity of the oxygen of the metal, and also of the acid, would be greater for the oxygen of the water than the affinity of the hydrogen for that same oxygen. But this is not so. The very opposite is the truth. If one atom of hydrogen be withdrawn from the hydrate of an acid or from the hydrate of an oxide, it is universally accompanied by an atom of oxygen. It is evident, then, that the affinity between the positive and negative atoms of oxygen is less than that which attaches these atoms to the element with which they form a primary com- binate.

A consequence of this truth is, that it is impossible to double the equivalent of oxygen, if the chemical equivalents are to be understood as not being in direct contradiction to any chemical truth or essential feature in the properties of an element. Carbon differs entirely in this respect from oxygen.

There is no reaction found where it is known that C² is divided into two parts. It is only consequent therefore to write, with Gerhardt, C² simply as C, it being then understood that the equivalent of carbon is (12) twelve.

This value of the atom will be adopted in the following part of this paper.

Sulphur, selenium, &c. being bodies displaying properties similar, not to carbon, but to oxygen, it will be necessary to retain the equivalent value that has generally been assigned to them.

I have now shown how ordinary alcohol, C²H⁶O², common æther, and the hydrocarbide, C²H⁶, belong to the type nCM¹.

The phenomena which necessitate this view of the constitution of these bodies have a like consequence in regard to the other alcohols, glycols, acids, and ethers of this series.

\[ \text{Propyle alcohol is } \begin{array}{c}
\text{C}...\text{H}^2 \\
\text{C}...\text{H}^2 \\
\text{C}...\text{H}^3
\end{array} \]

where it will be seen that the atom of carbon situated between the two others, on account of being chemically united to these, is reduced to the combining power of two for hydrogen, oxygen, &c. One combining power is given up to the carbon upon the one side, and a second to the carbon upon the other.

It will be observed also, that the primary combinates ought in rigour to be themselves enumerated in an inverse order. The type nCM\(^4\) becomes then in reality the type CM\(^4\). This enumeration, however, does not appear to possess any great practical utility, and it is perhaps preferable simply to denote it in an indefinite manner by adding "n" to the true type CM\(^4\).

\[ \begin{array}{c}
\text{C}...\text{O}...\text{OH} \\
\text{C}...\text{H}^2 \\
\text{C}...\text{H}^3
\end{array} \]

In like manner the butyle alcohol is to be viewed as

\[ \begin{array}{c}
\text{C}...\text{H}^2 \\
\text{C}...\text{H}^2 \\
\text{C}...\text{H}^3
\end{array} \]

and so on throughout all the series of these alcohols. The constitution of the ethers will be evident:

\[ \begin{array}{c}
\text{C}...\text{H}^2 \text{H}^2 \text{C} \text{H}^3 \text{C}
\end{array} \]

represents the mixed butyl-ethylene ether.

Formic acid is represented by the form \[ \begin{array}{c}
\text{C}...\text{O}^2 \\
\text{H}
\end{array} \] in like manner, \[ \begin{array}{c}
\text{C}...\text{O}^2 \\
\text{C}...\text{H}^3
\end{array} \]. Propionic acid is \[ \begin{array}{c}
\text{C}...\text{O}^2 \\
\text{C}...\text{H}^2 \\
\text{C}...\text{H}^3
\end{array} \]. The constitution of glycol may be represented as follows:

\[ \begin{array}{c}
\text{C}...\text{O}...\text{OH} \\
\text{C}...\text{H}^2 \\
\text{C}...\text{H}^2 \\
\text{C}...\text{O}...\text{OH}
\end{array} \]
In like manner as to the acids of these glycols; oxalic acid, for instance, may be represented as

\[
\begin{align*}
C & \quad \text{O}_2 \\
\text{H} & \\
\text{C} & \quad \text{O}_2 \\
\text{H} &
\end{align*}
\]

Respecting these acids, it may perhaps be allowable to suggest the possibility of the molecule having two poles, and that especially the atom of oxygen situated at one or perhaps both, and near to two atoms of oxygen bound together, and forming no secondary combine, may be in a state presenting great affinity for basic oxygen. Analogy with the electric poles may perhaps demand the opinion that all the negative oxygen be situated upon one side of the molecule. It will in that case be preferable to represent the oxalic acid as

\[
\begin{align*}
\text{O}_2 \\
\text{O}_2 \\
\text{O} \quad \text{O} \\
\text{O} \\
\text{O}
\end{align*}
\]

as it may, however, the rational method of investigation proves it to be a law, that in acids of the type nCM^4 the presence of two atoms of oxygen bound together so as to form only a primary part of the same molecule, and situated close to the negative oxygen, is necessary to the calling forth or production of this negative state.

This is a particular instance, but it moreover shows generally how the electro-positive or the electro-negative value of the elements mutually modify and condition the electro-positive or electro-negative value of each other when in combination.

This law is different from the electric hypothesis which chemists have formerly defended, but which never could be traced throughout a thoroughgoing application of their views to organic chemistry.

The law here distinctly enounced coincides exactly with, and is rendered apparent by the application of the theory of chemical combination which I support.

But to return. Glycerine is

\[
\begin{align*}
\text{O} & \quad \text{OH} \\
\text{C} & \quad \text{O} \\
\text{H} & \\
\text{C} & \quad \text{O} \\
\text{H} & \\
\text{C} & \quad \text{O} \\
\text{H}
\end{align*}
\]

and glyceric acid

\[
\begin{align*}
\text{H} & \\
\text{C} & \quad \text{O} \\
\text{H} & \\
\text{C} & \quad \text{O} \\
\text{H} &
\end{align*}
\]

Glucose has been perhaps too little investigated to afford data sufficient to determine definitely its formula. Taking, however, mucic and saccharic acids as starting-points, these bodies may
be meanwhile represented as—

\[
\begin{align*}
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{H} \\
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{O} \cdot \text{OH}
\end{align*}
\]

the acids. Glucose

\[
\begin{align*}
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{H} \\
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{H} \\
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{O} \cdot \text{OH}
\end{align*}
\]

It will thus be seen that these combinates all belong to the type nC\text{M}^4.

Many others might be added. For instance, tartaric acid:

\[
\begin{align*}
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{H} \\
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{H} \\
\text{C} & \cdot \text{O} \cdot \text{OH} \\
\text{C} & \cdot \text{O} \cdot \text{OH}
\end{align*}
\]

And the dibasic acid produced from it by the action of heat will be perhaps tartarelic acid.

It is my intention to consider, in a future communication, the second type, and to apply my views to the cyanogen combinates, &c.

---

XIII. Note on the Algebraical Theory of Derivative Points of Curves of the Third Degree. By J. J. Sylvester, Professor of Mathematics in the Royal Military Academy*.

Two years and upwards have elapsed since I discovered the extraordinary theorem in the doctrine of cubic forms which I am about to state, but which has never yet been published by me, although communicated in confidence to a few friends, including Mr. Cayley. It arose out of purely arithmetical speculations relating to such forms, to some of which I may make a brief allusion in the course of this note.

If we suppose the general homogeneous equation of the third

* Communicated by the Author.
Derivative Points of Curves of the Third Degree. 117

degree in \(x, y, z\) reduced to the canonical form

\[x^3 + y^3 + z^3 + mxyz = 0,\]

any solution \(x = a, y = b, z = c\) of this equation is of course one of a group of six obtained by the permutations of the three letters \(a, b, c\), and having an obvious relation to one another through the medium of the points of inflexion. So, too, it is manifest if we take the equation to the curve in its most general form, from any given solution, a group of six, including the given one, may be formed, the characteristics of each of which will be linear functions of one another. For the purpose of the theorem about to be enunciated, such a group of solutions will be treated as a single solution; and then we can affirm the proposition following, in which a solvent system means a system of values of the variables \(x, y, z\) satisfying the equation \(f(x, y, z) = 0\), and free from any common factor.

Let \(a, b, c\) be any solvent system to a cubic homogeneous equation in \(x, y, z\); then from \(a, b, c\) we may derive a new solvent system, \(a', b', c'\), where \(a', b', c'\) are each of them functions of the fourth degree of \(a, b, c\), and another system \(a'', b'', c''\) of the ninth degree in \(a, b, c\), and another \(a''', b''', c'''\) of the sixteenth degree, and so in general a new solvent system of the degree \(n^2\) in \(a, b, c\). One such derivative system, and only one, of the degree \(n^2\) can be formed, and none of any intermediate degree.

Thus, for instance, the coordinates of the tangential (the name adopted from me by Mr. Cayley to express the point of intersection of a tangent to a cubic curve at any point with the curve) being called \(a', b', c'\), these last letters are biquadratic functions of \(a, b, c\).

So again, as I also suggested to Mr. Cayley, the point in which the conic of closest contact with a cubic curve cuts the curve will necessarily have a derivative system of coordinates of a square-numbered degree in respect of the original ones, which by actual trial Mr. Cayley has found to be the 25th. Mr. Salmon, I believe, has obtained in certain geometrical investigations derivatives of the 49th degree.

I am in possession of the equations by means of which the successive systems of the fourth, fifth, &c. degrees, which I incline to call the first or primary, the second, third, &c. derivative systems, may be formed explicitly by successive derivation from one another; so that, for instance, as soon as I am informed that the system investigated by Mr. Cayley is of the twenty-fifth degree or fifth order, I can find them without any reference to

* This derivative solution (though not as corresponding to the tangential) was known also to rule for a particular ease, as will be seen by reference to his 'Algebra.'
the geometry of the question, the quantities belonging to the \( n \)th
derivative being in fact a known algebraical function of \( n \)!
I
was led to the discovery of this surprising and unique law by a
statement of a friend, not since verified, and which, for aught that
has yet been shown, may or may not be true, that the number 5
could be divided into two rational cubes: assuming this to be
the fact, it necessitated (by virtue of my investigations) the coin-
cidence to a factor \( \text{près} \) of two functions obtained by apparently
independent algebraical processes, which coincidence by actual
comparison of the functions I found to obtain.

With reference to the connexion of this theory of derivation
with the arithmetic of equations of the third degree between
three variables with integer coefficients, it is after this kind.
Fermat has taught us that a certain class of such equations, viz.
the equation \( x^3 + y^3 + z^3 = 0 \), is absolutely insoluble in integers
(abstraction made of the trivial solutions of the type \( x = 0, \)
\( y + z = 0 \)).
I
have greatly multiplied the classes of such known
insoluble equations, as may be seen by a communication from
me to Tortolini's \textit{Annali} in 1856. But over and above such
equations I have ascertained the existence of a large class of
equations, soluble, or possibly so, it is true, but enjoying the
property that all their solutions in integers, when they exist,
are \textit{monobasic}; that is to say, all their solutions are known func-
tions of one of them, which I term the \textit{base}, and which is charac-
terized by this property,—that of all the solutions possible it is
the one for which the greatest of the three variables is the smallest
number possible. If this solution be laid down as a point in the
curve corresponding to the given cubic, all the other solutions
possible in integers will be represented by points in this curve,
which are derivatives (in the sense previously employed in this
note) to the given point, having coordinates respectively of the
4th, 9th, 16th, &c. degrees, in respect of the coordinates of the
\textit{basic point}.*

If my memory serves me truly, I have found (as a particular
case) that all cubic equations in numbers of the form

\[ x^3 + y^3 + z^3 = i m x y z, \]

where \( i \) is 1 or 3 or 6 (I cannot at the moment remember which),
arc either \textit{insoluble} or \textit{monobasic}. The case of \( i m = 3 \) must of
course be exceptional, being satisfied by \( x + y + z = 0 \). This

* This theorem is analogous to that relating to the integer solutions of
\( x^2 - A y^2 = 1 \), in so far as there is a \textit{basic} solution to this equation in in-
tegers of which all the other solutions are derivatives, and not more than
one such derivative exists of any given degree, but with the difference that
there does exist one of every degree, and not merely (as in my theorem for
cubic forms) of every square degree.
doctrine of derivation evidently conducts to a new branch of the
grand doctrine of invariance. I hope to have tranquillity of mind
er long to give to the world my memoir, or a fragment of it,
"On an Arithmetical Theory of homogeneous and the Cubic
Forms," the germ of which, now, alas! many weary years ago,
first dawned upon my mind on the summit of the Right,
during a vacation ramble.

XIV. On the Action of the Magnet upon the Electrical Dis-
charge in Rarefied Gases. By M. Plücker*.

[With a Plate.]

1. THE idea of employing tubes with platinum electrodes
fused into them for observing the electrical discharge
through rarefied gases, instead of the electrical egg, as originally
employed by Ruhmkorff and Quet, may be considered in many
respects a happy one. Such tubes, containing various gases and
vapours, are prepared in this city, of the most different forms, by
M. Geissler, and present sometimes an appearance of incom-
parable beauty. Geissler's tubes (I give them, and with justice,
this name, although the first such tubes were not prepared by
him) were tried at the beginning of this year in the Physical
Cabinet: and what more natural than the thought of approxi-
mating such tubes in various ways to the poles of a magnet
during the discharge? Davy had already noticed that the arch
of light which he formed between carbon-points by means of a
powerful battery was diverted by the magnet. Arago had pre-
dicted such diversion. In the same way it was possible to pre-
dict generally the nature of the diversion of the electric current in
Geissler's tubes. But on the actual performance of the experi-
ment, in addition to the phenomena which were looked for,
certain unexpected ones presented themselves; namely, the divi-
sion of the light-stream, its decomposition at the negative elec-
trode into an undulating flickering light, and the extension of
the stream from the positive electrode into a brilliantly illu-
minating fine point. I communicated these results to the
Rheinische Gesellschaft der Naturforscher und Aerzte (Kölnische
Zeitung, July 22, 1857). I showed the phenomena in question to
the members of this Society, as also afterwards to a section of the
Thirty-third Versammlung deutscher Naturforscher und Aerzte.
Soon afterwards, however, I was convinced that phenomena of
the most beautiful kind, and such as were hitherto without
analogous ones, had remained unnoticed. This circumstance

* Translated by Dr. F. Guthrie, from Poggendorff's Annalen, vol. ciii.
No. 1, 1858, p. 88.
determines me to complete my former communications; in doing so I shall trespass as little as possible upon investigations to be published subsequently.

2. The first two tubes, which M. Geissler prepared according to direction, contained mercury and atmospheric air in the highest possible state of rarefaction. At my suggestion, resulting from former trials, instead of air, the vapour of an aetherial oil, hydrogen, and phosphorus were employed, and the tubes were widened at one part. The mercury also was removed from the tubes, but their evacuation was still performed by its means. The two tubes containing the last-named substances, hydrogen and phosphorus, still continue to give a beautiful effect with the greatest certainty. M. Geissler, however, afterwards tried many other gases, both simple and compound, altering at the same time the shape of the tubes in various ways. In most cases it has hitherto been difficult to say with certainty what gases are contained in the Geissler tubes. Even on testing one of the first of these tubes which contained the vapour of an aetherial oil, an immediate alteration in the phenomenon appeared, owing to the decomposition of the vapour. This electrolysis of dilute compound gases received complete verification in subsequent cases. In tubes containing hydriodic acid, the iodine is gradually deposited. In highly rarefied gases this electrolysis by the electric stream, as it becomes finely divided, often manifests itself suddenly by a remarkable alteration of colour. Examples of this were furnished by tubes containing phosphoretted hydrogen and sulphurous acid. The laws of the electrolysis brought about by the spark of Ruhmkorff's apparatus may, however, be traced in gases and vapours of ordinary density.

3. The metal composing the electrodes, especially the negative one, is transferred to the glass of the tube. This is the case even with platinum electrodes; and the blackening of the tube in the neighbourhood of the warmth-pole (the negative pole) is principally attributable to the combination of the platinum with traces of other substances present in the tube.

4. In the different Geissler's tubes the light appears of all kinds of colours, often of a very intense nature, and on analysis with the prism yields variously modified spectra. The description of these colours, especially the more delicate ones, is the more difficult, inasmuch as the impression they produce upon the eye depends upon the external illumination, and the same colour may, for instance, appear bright violet or rosy red, according as it is viewed by weak daylight or by the light of a candle.

5. The dark bands first observed by Ruhmkorff and Quet in the electrical egg appear in Geissler's tubes of the most varied
shape, and in some of them with the greatest distinctness. I might say that an entirely new notion is formed of the character of this stratification of the light on seeing, in a tube about 2 millims. wide and 400 millims. long, containing traces of hydrogen gas, about 400 clear spaces, alternating with dark intervals, with perfect regularity from one end of the tube to the other, and the dark space which has been before observed occurring only at the warmth-pole (the negative electrode). In wider tubes the dark intervals may attain a breadth of 5 millims.; they become narrower if the electric light passes from a wider tube into a narrower one. They often appear only after the discharge has passed for a long time through the tube, and then become gradually better and better defined. In most cases the illuminating portions perform small oscillations; and when this is the case, they give the impression of a spiral motion. They are often, especially towards the negative pole, completely stationary, and appear then as solid immoveable discs. In many cases, especially in wide tubes, these discs assume a strongly conoidal form, the convexity of which is invariably turned to the warmth-pole, thus corresponding to the direction of the electrical discharge. On inverting the poles, the conoidal points, in accordance with the above, assume an opposite direction.

6. It may seem at present premature to attempt to explain fully the stratification of the light produced by the electric discharge, for such might perhaps embrace the explanation of the physical process which takes place on such discharge. So much, however, we may already consider as decided, that it is impossible for the discharge through Geissler's tubes to consist of the passage of a luminous appearance, or illuminating substance, from one end of the tube to the other; nor does such an explanation derive support from the fact that the discharges of light take place at intervals, which, if Ruhmkorff's apparatus be employed, depend upon the rapidity with which the breakings of the current follow one another. The phenomena can only consist in an aggregation of matter at definite parts of the tube which become luminous through the discharge, while the passage of the electricity from one luminous place to the other is dark. The aggregation of the matter which becomes luminous at these definite places in the tube is a consequence of the discharge itself, and often requires a long-continued series of discharges.

7. In the neighbourhood of the warmth-pole, towards which the electricity passes, no aggregation of matter rendered luminous by the discharge occurs; but at this place there is invariably a dark space, which in the wider kinds of tubes often
attains a length of 80 millims. This dark space separates sharply the stratified light we have been hitherto considering, from the light which appears about the negative electrode (the warmth-pole) itself, and which forms a kind of atmosphere around it. This remarkable light will occupy our special attention at the end of this paper.

8. Finally, the negative electrode is, in its immediate neighbourhood, surrounded by an envelope of variously coloured, finely stratified light, which corresponds with the form of the electrode. The fine layers are spherical when the positive electrode itself has a spherical form; they have a cylindrical form in the case where the electrode projects into the tube as a wire.

9. After these preliminary observations, I proceed at once to the proper subject of this paper, namely, the action of the magnet upon the electrical discharge through spaces containing rarefied gases. In the experiments immediately to be described, I employed a great upright horseshoe magnet, to the two limbs of which two heavy armatures were applied, 4 centims. thick, 13 centims. wide, and 20 centims. long. Each of these armatures was rounded circularly at one end, and the rounded extremities were directed towards one another, being kept, by an interposed brass disc, at a distance of about 4 millims. The Geissler tubes were placed upon these armatures, first, in an equatorial direction; secondly, in a position perpendicular to it, that is, in an axial direction.

10. If, instead of the tube, a freely moveable copper wire, through which a galvanic current passes, be brought in the (horizontally) equatorial position, it will be either drawn vertically downwards or repelled upwards, according as the direction of the galvanic current in the wire corresponds, or the reverse, with the superior directions of the Ampèreian currents, which are the same in direction in the two approximated armatures, and may be supposed to move in vertical planes perpendicular to the axial direction. (If we represent the direction of the Ampèreian currents by the path of the hand of a watch, we must of course regard the watch itself as a magnet whose south pole is the lettered watch-plate.) The attraction or repulsion of the conducting wire may also be regarded as the resultant of two couples which tend to turn the wire in opposite directions.

Further, if instead of the tube we imagine the conducting wire to be placed in an axial direction, in such a manner as to extend equally far on both sides beyond the point of maximum action, then the two halves of the conducting wire are subject to opposite actions of the magnet. Supposing the two extreme ends to be rigidly fixed, the two halves would strive to revolve in a
horizontal plane in opposite directions around the two ends, as
gthough endeavouring to break the wire in the middle (Pl. I. fig. 2).

11. I placed a tube about 270 millims. long, widened in the
middle to an ellipsoid (width of tube about 12 millims., greatest
width in the middle 52 millims.), equatorially upon the ap-
proximated armatures. This tube contained a trace of phos-
phorus, and gave a beautiful red light when the discharge was
led through it by means of the two platinum wires fused into its
extremities. In the tube itself the sharply-defined stratification
of the light appeared; on entering the ellipsoid the strata spread
out in a spherical form, becoming thereby more strongly bent;
and having become plane in the middle of the ellipsoid, they
changed the direction of their curvature.

According to the direction of the current and to the manner
of exciting the magnet, both of which may be reversed, four
distinct cases present themselves; these, however, require no
special attention, inasmuch as they are the same as in the case of
the conducting wire through which a galvanic current passes.
In two cases the electrical light-currents were attracted in the
ellipsoid; in the other two they were repelled. In the case of
attraction (fig. 3), the light-current declined from the side of the
positive electrode (the light pole) into the ellipsoid, and termi-
nated immediately above the approximated armatures in a gra-
dually-narrowing sharply-defined point of continually increasing
brightness. From the other side, beautiful red undulatory
flames proceeded, which sunk down in the ellipsoid, and ex-
tended beyond the middle point between the armatures, without
manifesting any tendency to unite with the steady light-stream
from the opposite side. On changing the direction of the mag-
netic current (fig. 4), the phænomenon underwent a gradual
alteration. On entering the ellipsoid the light became concen-
trated into a luminous arch, which traversed the upper part of
the bulb in the equatorial plane. Immediately on entering the
ellipsoid the stratification of the light became finer; at the upper
part of the arch, where the concentration of the light was great-
est, the dark intervals became more and more numerous and
distinct. A simultaneous change in the direction both of the
electric and magnetic currents, effected, as was of course to be
expected, a mere inversion of the original phænomenon, the
light-stream sinking to a point from the opposite side. In the
second phænomenon (that of repulsion) a similar double inversion
of the direction of the currents effected no appreciable alteration.
The repelled arch of light did not undergo any apparent change
on changing the direction in which the positive electric current
entered the ellipsoid, which was clearly owing to the greater
distance of the arch from the poles.
12. When the tube was slipped along parallel to itself, so that the ellipsoid moved in an axial direction from the point of maximum action above the approximated armatures towards either one side or the other, the plane in which the attraction or repulsion of the electrical light-stream occurred no longer remained vertical, but became more and more inclined, its inclination being always determined by the relative positions of the axis of the tube and the place of greatest action.

13. A perfectly similarly-shaped tube, containing a small quantity of hydrogen instead of the trace of phosphorus, showed exactly the same appearances, with the single exception that the light, instead of being red, was bright violet.

14. When these same two tubes were so placed upon the armatures that the electrical current passed through them in an axial direction, the light-streams which entered the ellipsoid from both sides underwent horizontal aberrations in opposite directions. The stream proceeding from the light-pole terminated as before in a brightly luminous point, which coincided in position with one of the two extremities of the horizontal equatorial diameters of the ellipsoid. The light-stream entering the ellipsoid from the warmth-pole was driven towards and concentrated at the opposite side of the bulb, whence continually-undulating flames were directed towards the side of the luminous point. This appearance was especially beautiful when, instead of the bulb-tubes described, simple cylindrical tubes of greater width were taken (270 millims. long and 18 millims. wide). The undulating flames extended then for a distance greater than 10 centims.

Both on reversing the electric current and on inverting the magnetic polarity, the phænomenon underwent simple inversion more quickly in the first case than in the second, the luminous point and the undulating light changing places.

15. In the phænomena just described, the direction of the horizontal aberration of the light-stream is determined as in the case of the moveable conducting wire. From the consideration of the same case, the breaking of the light-stream above the point of greatest action might have been predicted. In the previously described phænomenon, however, where the light-stream throughout its whole length is only attracted, such a rupture of the stream by the magnet was in no way to be anticipated.

16. I may as well mention here an isolated phænomenon or two which were observed in connexion with the action of the magnet upon the electrical light-discharge through Geissler’s tubes.

When the light in such tubes, especially at their widened portions, is diffused, showing no trace of stratification, the dark
intervals are very often brought into existence by the magnet, whose attractive or repulsive power concentrates the diffused light. I imagine that this concentration of the light-stream must be considered as the cause why the beautiful blue light in a tube containing sulphurous acid, which had remained quite unchanged during a long-continued succession of discharges from Ruhmkorff’s apparatus, on placing the tube upon the magnet, was in a very short time converted into a washed-out green, a colour corresponding to that produced by oxygen gas. It seems to me as yet improbable that the magnet should exert a direct influence upon the decomposition which takes place.

17. A tube containing bromine, 200 millims. long and 10 millims. wide, terminated at both ends with bulbs, gave rise to reddish and violet flashes, which, forming a pencil, continually traversed the axis of the tube from one end to the other, being surrounded by a diffuse green light. On placing the tube equatorially upon the magnet, the flashes were, when the current passed in one direction, drawn down so that they traversed the lower part of the tube without rupture. They underwent, however, thereby a change of colour, becoming bright bluish green, while a red mist appeared in the upper part of the tube.

18. In another case, a short tube, terminating in a wider bulb, was laid upon the approximated armatures. Before the magnet was excited, the light on entering the bulb, which rested sideways on the two armatures, was dispersed in all directions: after the magnet was excited the bulb was traversed by green and reddish flashes.

19. It being my intention to study the action of magnetism upon the light which appears at the warmth-pole and which is separated by the dark space from the true light-stream, I brought appropriate tubes with their warmth-electrodes into the neighbourhood of the pole; and I shall now consider the new class of phenomena which I thereby observed. In order to see clearly the phenomena to be described, it is necessary that the electrode, where the warmth-pole has to be situated, should penetrate far enough into the tube and that the tube be sufficiently wide at this end.

20. I chose, in the first place, a tube (fig. 5) 10 millims. wide, upon whose two extremities glass bulbs of 35 millims. diameter are fused. The length of the entire tube is 250 millims. The platinum electrodes extend into the bulbs as far as their centres. Upon the electro-magnet two armatures were placed, whose section was a square having a side of 70 millims. The edges of the two armatures which were turned towards one another were rounded off, and in the middle of each an obtuse
conoidal pole-point was screwed in. That bulb of the tube which contained the warmth-pole was then so disposed upon the conoidal poles, that the tube passing between these points was situated in a horizontally equatorial position. Previous to the excitation of the magnet, diffuse violet light was spread through the bulb, surrounded by a pale green light, which appeared to form a thin coating immediately upon the surface of the glass bulb. After the magnet was excited, the diffuse violet light collected to a horizontal, semilune-shaped, bright and uniformly luminous disc, bounded towards the tube by an almost circular well-defined concave arch, whose middle passed through the point of the platinum electrode. This disc was upon the opposite side enveloped by a narrow strip of beautiful bright green light, which followed the curvature of the glass; Moreover, the light (according to what has been already described), which was red at a distance from the pole, but became bright violet towards the pole, and showed the dark intervals beautifully, descended towards the bottom of the bulb on entering it from the tube, and terminated in a point. On inverting the polarity of the magnet, the phenomenon remained entirely unaltered, the tongue in which the light-stream terminated merely passing towards the top instead of the bottom of the tube—an alteration which we need no longer consider.

21. On so changing the position of the tube (fig. 6) that, while still remaining in a horizontally equatorial position, it no longer passed between the polar points which continued in contact with the bulb, the phenomenon remained unaltered, with the single exception that the horizontal violet disc, separated from the glass by a green strip, was bounded towards the tube by an arch whose convexity was now turned towards the tube, and whose middle point still coincided with the point of the platinum electrode.

22. The two armatures were then moved apart from one another, just sufficiently to allow the bulb to fit in between them (fig. 7). The tube having the same direction as before, the line joining the pole-points passed through the central point of the bulb, where also the electrode terminated. On exciting the magnet, the disc, which was formed as in the former cases, was bounded by the axial diameter: it was, as before, so far as the glass sides extended, enveloped by a beautiful green strip. Upon the disc, in the neighbourhood of the axial diameter, a strong accumulation of light took place, a phenomenon which was not observed in the previous cases.

In the two last cases, as in the first, on inverting the magnetic polarity, exactly the same appearance was seen after some time, as far at least as the disc was concerned.
23. After the excitation of the magnet, the light diffused in
the bulb of the warmth-pole, as it gradually accumulates to a
disc, rotates around the axial line. The direction of this rota-
tion is the then direction of the Ampèreian currents in the
excited magnetic poles. When the magnetism ceases, the light
rotates back again in the opposite direction. This rotatory motion
is stronger on inversion of the poles. The direction of the
Ampèreian currents in the newly-caused magnetic excitation
determines the direction of the rotation: on inversion this
direction is reversed. In fig. 7 the arrow shows this direction,
N and S marking the north and south magnetism of the
armatures.

24. Finally, the tube and bulbs remaining in the position
last described, the armature was removed from one of the two
limbs of the magnet (fig. 8). The horizontal disc of light
remained, as before, bounded by the axial diameter of the bulb;
but the violet light, in the neighbourhood of the remaining pole-
point, was more intense, and the bright green strip enveloping
the disc became somewhat broader towards the opposite ex-
tremity of the diameter, and diminished in the intensity of its
light in a corresponding degree.

25. It follows as a matter of course, that in all the cases
hitherto described, if the tube be turned about the axial line, the
plane of the disc moves with the tube also about the same axial line.

26. The tube still remaining in its horizontally equatorial
position, on bringing the bulb down upon the two pole-points,
that which was before a disc, assumed the form of a dome-
shaped surface which descended from the platinum wire on both
sides. By a greater approximation of the pole-points, the cur-
vature of the surface is increased.

27. If we consider the contents of paragraphs 20 to 26 as a
whole, the idea presents itself forcibly to us, that the several
planes or curved surfaces, in which the diffused light spread
around the warmth-pole becomes concentrated, are formed of
lines of light which, proceeding from the separate points of the
positive electrode, coincide with magnetic curves.

28. This view is supported also by the fact that the arched
concave or convex border of the disc of light becomes more
strongly curved in the two cases first described, if, instead of
the armatures with conoidal points, parallelopipedal armatures
(190 millims. long, 68 millims. broad, 20 millims. thick) be
laid upon the magnet, and then the tube so brought between the
approximated armatures that in one case the bulb with the
warmth-electrode rests upon them from the outside, in the other
the surfaces of the armatures meet the tube with the bulb
exteriorally.
29. The same view is also supported, as far as we can judge of the complex path of the magnetic lines of force, if in the first series of experiments we make use of the heavy armatures.

If, still keeping the tube equatorial, we bring the bulb containing the warmth-pole above the place of greatest action, the domelike-light surface appears with unusual distinctness. On moving the tube, then, so as to keep it parallel with itself, towards one or the other side, in an axial direction, the surface alters its form in a manner which may be previously determined from the path of the magnetic curves existing at the time.

30. We have now to describe those phenomena which appear when the tube through which the discharge takes place is in an axial position (fig. 9). We shall, in the first place, again employ the tube having bulbs at both ends, and place an armature with conoidal point upon only one limb of the electromagnet. If, then, we so touch the tube, lying in an axial position, that the warmth-electrode comes in contact with this pole-point, the violet light streams into the tube, assuming the form of a cylinder. The green light collects with especial brilliancy at the entrance of the tube, from thence spreading out over the inner surface of the bulb, and being sharply bordered towards the centre of the bulb by a circle, the plane of which is perpendicular to the axial direction, and which is distant from the entrance of the tube about one-fourth of the diameter of the bulb.

On inversion, it appears as if both the violet and green light first returned to the platinum electrode and then moved forwards again, occupying finally exactly the same position as before. During such motion the green light remains in the neighbourhood of the glass.

31. The following series of experiments were performed with a tube tapering conically towards one end. Its greatest diameter at the wider end was 40 millims. At this end the platinum wire intended for the warmth-electrode entered, penetrating to a distance of 23 millims. When the discharge passed through the tube towards the platinum wire, the latter was surrounded by the diffused light sharply bordered by the dark space, which extended almost 60 millims. into the interior of the tube. Beyond this dark space the dark bands appeared extending through the whole tube, at first about 3 millims. apart, but gradually approaching one another towards the end of the tube. In the neighbourhood of the warmth-pole these bands assumed the form of spherical surfaces, whose convex sides were turned towards that pole. On inverting the current, the direction of curvature of the stratified light was changed, the layers reaching now to the platinum wire itself, which had
become the light-pole, in the neighbourhood of which they did not extend as far as the sides of the tube (fig. 11).

32. On both limbs of the electro-magnet the two heavy armatures were again placed (fig. 12) with their rounded ends turned to one another. In the first instance, the tube was so placed upon these, in an axial position, that the point of the platinum wire which still served as the warmth-pole was situated nearly above the point of maximum action. The diffused light surrounding the platinum wire became, in consequence of the magnetic action, concentrated to an arched disc of violet light, which stood in an axial position upon the two armatures and was bordered approximately by two concentric circular arcs whose centre coincided with the middle point between the higher surfaces of the two armatures. In the inner arc, which touched the extremity of the platinum wire and formed almost a semicircle, light was concentrated in the form of a bright narrow strip, while the remainder of the space, 8 to 9 millims. broad, between the two concentric arcs was more feebly; though for the most part uniformly, illuminated. Towards the end of the tube alone, beneath the platinum wire, the light increased in brightness. The wire itself, as is always the case, remained constantly surrounded by a finely stratified envelope of light. In this case the feebler illumination is also bordered with perfect sharpness by the higher arc, which terminates upon the platinum wire just where it is fused into the bulb. A narrow dark interval separated the light around the warmth-pole from the remaining light of the tube, which had retained its stratification. The form of the layer only followed to some extent that of the border in the neighbourhood of the dark interval. Moreover, the stratified light no longer filled the whole interior space of the tube: it had concentrated itself in a horizontal direction on one or other side, in the manner of the previously described horizontal aberration (14), in consequence of the armature placed beneath it. On inverting the magnetic polarity the phænomenon gradually disappeared, but reappeared again slowly in precisely the same form, with the exception, which we need not further consider, that the stratified light was driven into the opposite side of the tube in a horizontal position, the side view of the tube remaining the same on turning it round.

33. If the tube (fig. 13), still remaining in an axial position, was so moved endways that the place where it rested upon the armatures approached the light-pole, the appearance was quite changed. The two arcs of light, which were before circular and which bordered the ring in which the atmosphere of light had become concentrated around the warmth-pole, assumed the form of magnetic curves.

The interior one, which continued to pass through the platinum point and remained a magnetic curve, on gradually slipping the tube along, became more and more elongated towards the narrower part of the tube, until, finally, on reaching the upper portion of the tube, it bounded the light around the warmth-pole. By this change a remarkable commixture of the light occurs, and new colours appear when the ring opens and its light approaches that of the other part of the tube. The former of these was violet, the latter reddish; a beautiful blue light was formed on their approximation.

34. If, finally (fig. 14), the conical tube be so moved lengthways from its position as to come entirely above one of the two armatures, the ring of light remains quite sharply defined. In this case that arc which was formerly the inside one, has become the exterior one, still passing through the point of the platinum wire, and intercepts the violet light towards the narrower side of the tube. The other arc, which was formerly the outside one, still continues to pass through that point of the platinum electrode where it enters the tube through the glass. This arc has now become the brightly luminous one, and in its neighbourhood the otherwise uniform light of the ring is brighter. Both limiting arcs follow here also the path of magnetic curves.

35. The tube employed in the last experiment showed also the beautiful green light, whose appearance is so enigmatical. At first I considered this light to be a subjective “contrast-colour;” subsequently, however, when this generally-accepted view was shown to be untenable, the idea forcibly presented itself that it was a fluorescence in the glass itself. Nevertheless the light in question is in the inside of the tube; but it is situated so closely to its sides as to follow exactly its inequalities, and thus to give the impression of belonging to the glass itself. This explanation was first given on using the magnet. On inverting the magnetic poles, the light was moved backwards and forwards in the neighbourhood of the warmth-pole, and collected at the place where the surface formed by the magnetic curves touched the glass. If, in consequence of a movement of the tube, this surface touches the glass in a fresh place, the green light immediately appears at that point.

The conditions under which this light appears at all in Geissler’s tubes, can only be determined by solving the question as to the nature of the dependence of the phenomena in these tubes upon the chemical nature of the traces of ponderable matter which they contain.

36. We can only say with certainty that the concentration of the luminous atmosphere around the warmth-pole to a luminous surface formed of magnetic curves, is a general physical fact.
which remains the same in all cases, with the exception that the
diverse nature of the gases in the interior of the tubes affects in
many ways the phænomena of colour. Tubes containing carbonic
acid, to adduce one more example, give a beautifully stratified
green light. The luminous atmosphere around the warmth-pole is of a beautiful blue, and the envelope of light around it
again green. The surfaces of light into which the blue light
collects, under the influence of the magnet, are in nowise differ-
ent in form from those before described.

37. The colour of the luminous atmosphere around the
warmth-pole, and of the surfaces in which it collects under the
influence of magnetism, is totally independent of the nature of
the electrodes. Three precisely similar tubes were taken, pro-
vided at their extremities with similar bulbs, into the middle of
which the platinum wires extended. The platinum wires were
previously covered with fused glass at the parts where they were
melted into their respective glass bulbs; one of them was then
coated (galvanically) with gold, the second with silver, and the
third with copper. The phenomenon presented under the in-
fluence of magnetism was perfectly the same on making each of
the ends of the three tubes, one after the other, the positive
electrode.

38. Finally, a small sphere was melted on to the end of a
platinum wire*; and after covering all the rest of the wire with
melted glass for the sake of isolation, it was fused into a wider
tube, with the sphere as warmth-electrode. From this sphere as
a centre the whole luminous atmosphere spread out, which, by
the action of magnetism, was concentrated, not, as before, to a
surface of light, but to a line of light, a brightly luminous mag-
netic curve.

If the whole of the platinum wire serving as warmth-electrode
be isolated in the manner described, with the exception of several
single points, every such non-isolated place becomes a centre
of light, spreading out in all directions, which light is drawn
together by the magnet into the magnetic curve passing through
this point. Such a luminous magnetic curve corresponds to
every one of the points. If the platinum wire be not isolated,
every one of its points gives rise to radiant light, and we obtain

* As it may be desirable for many purposes to form platinum wires
terminating in a sphere, I shall here mention the following sure and easy
method which I have for many years employed for this purpose. I take
three or more Grove’s elements, according to the thickness of the wire,
and complete the circuit by means of mercury with the platinum wire as
warmth-pole. The point of the wire melts on coming into contact with the
surface of the mercury. The contact is preserved by gradually advancing
the wire, from 1 to 2 inches of which are melted in a few seconds, to a perfect
sphere.

K 2
a luminous magnetic surface as the geometrical locus of the magnetic curves passing through the separate points of the platinum wire.

Bonn, December 27, 1857.

*Addition to the above. By M. Plücker.*

39. In accordance with the phenomena described in the latter part of the preceding paper, we may say that electric light under the circumstances in point is magnetic*. Inasmuch as such light, which proceeds from one point of the negative electrode in all directions, is drawn together by the magnet to a luminous magnetic curve passing through the same point, the original rays behave as iron-filings would do if we imagine them infinitely fine, perfectly flexible, and attached to the point of the electrode in opposition to the force of gravitation. This magnetic behaviour is entirely distinct from the effect of the magnet upon the luminous magnetic currents. The phenomenon in question reappears in precisely the same manner, as I have already proved, after inversion of the magnetic poles, whence we may conclude it to be altogether independent of the direction of the discharge of the electric current.

40. This last hypothesis was most distinctly supported by the following experiment:—A tube was taken which, as before, had a bulb fused on to one end. The negative platinum electrode was introduced into the bulb, so as to form one of its diameters perpendicular to the axis of the tube. The two ends of this electrode, which was thus fused into the glass in two places, were connected together outside the bulb. The bulb was placed upon the approximated armatures in such a manner that the tube was in a vertical, and the horizontal electrode in an equatorial position. On discharging, as before, Ruhmkorff's apparatus through the tube, light emanated from every point of the electrode through the bulb, and this light collected to a curved surface, which, forming a bridge from one polar surface to the other, consisted of the illuminated magnetic curves passing through the separate points of the electrode. This surface remained perfectly firm and immoveable when the bulb with the tube was turned to any extent around the platinum wire, which formed a horizontal diameter of the bulb. It also remained unaltered even when the tube (and therewith the path of electrical discharge) was turned obliquely to the horizon, vertically or horizontally, and in the latter position the tube might reach above the one or other armature, away from the place of maximum magnetic action, without altering this surface.

* These phenomena appear most beautifully in the wide cylindrical tubes lately made by M. Geissler.
In the same manner, the bulb, when placed upon the point of greatest magnetic action in such a manner that the platinum electrode was horizontal and axial, might be turned round in the equatorial plane without the disc of light thus formed undergoing any alteration at all.

41. On making use of the tube before employed, provided with a bulb in the centre of which the negative electrode terminated, whose whole length, with the exception of the extremity, was isolated with glass, the corresponding luminous magnetic curve passing through the end of the wire remained always the same, in whichever direction the bulb with its tube was turned around its centre.

42. In order to discover magnetic light, in the above-described meaning of the term, experiments were in the first place made with ordinary frictional electricity. The positively charged conductor of an electrical machine, terminating in a sphere, was placed above the great electro-magnet in such a manner that the sphere was at a distance of from 20 to 22 centimetres from it. "Brush" discharges were effected from the sphere, which passed to the edges of the armatures, whose rounded ends were approximated to one another and covered with caoutchouc. On exciting the electro-magnet, the brush-discharges remained unaltered. In the same manner I have not been successful in discovering any action of the magnet, in the open air, upon the glow, as M. Riess calls it (Glimmlicht), which appears at the negative pole.

43. On the other hand, the effect was complete on discharging the positively charged conductor of the machine as it was being turned, through one of Geissler's tubes terminating in bulbs. In this case the platinum electrode of one bulb was brought into contact with the conductor, while the other bulb rested upon the approximated armature. In the latter bulb, that is, in the negative electrode, the magnetic light appeared, which was drawn by the magnetic action into magnetic curves. The phenomenon appeared continuous to the eye, and the same, only somewhat more feeble, as when Ruhmkorff's apparatus was employed. Even when the conductor discharged itself in powerful sparks, which passed to a metallic sphere in contact with the positive electrode, the same appearance presented itself in the neighbourhood of the magnet at the negative electrode, at every separate discharge.

When the conductor was charged with negative electricity, no magnetic light appeared above the magnet at the positive electrode.

44. In these experiments also the dark bands were observed in the tube, and the proper electrical light-current was on every occasion aberrated in the same manner as the stream of induction electricity as before described.
45. On discharging Ruhmkorff’s apparatus through Geissler’s tubes, the two electrodes are most certainly distinguished from one another by their simple appearance. The platinum electrode at one end of the tube glows; at the other, brightly-luminous points appear upon the wire.

The glowing platinum wire is covered with a finely stratified luminous envelope, around which is formed the magnetic atmosphere of light bordered by a dark space.

If we attempt to bring the above experiments in accordance with statical electricity, we must assume that the electrode at which warmth is chiefly manifested is the negative electrode, in direct contradiction to the hypothesis universally received since the experiments of Neeff, that warmth is most abundantly manifested at the positive and primary (primaires), light at the negative electrode: in contradiction also to many experiments with the battery; for instance, to the observation given in a note at the end of the first part of this paper, concerning the fusion to a sphere of a platinum wire serving as positive electrode in contact with mercury.

46. Misled for a moment by such contradictions (M. De la Rive also adduces such contradictions), I yet soon completely satisfied myself that in our experiments that electrode which glows, and at which the magnetic atmosphere of light appears, is really the negative one. For, first, on repeating Neeff’s experiments under the microscope, with the weakened current of Ruhmkorff’s apparatus, light and warmth appeared exactly as on the discharge through Geissler’s tubes, that is just in the reverse way to that given by Neeff. Still the phænomenon is very variable with different degrees of weakness of the current, so that the phænomenon of Neeff appears to be a complex one. Secondly, on referring to the construction of Ruhmkorff’s apparatus, in which the induced current in the thin wire which is alone employed in our experiments agrees with the direction of the inducing current, it follows that the discharge-current passes through the tube towards the warmth-electrode. Thirdly, the electrometer proves the presence of free electricity during the discharge as well as at other times, at the end of the induced-current wire (most strongly at the outer end of the wire). This free electricity is negative at the warmth-pole where the luminous atmosphere is situated, in agreement with the experiments which were performed with frictional electricity. In the fourth place, the aberrations of the discharge-current in Geissler’s tubes, described in the first part of this paper, all correspond with the hypothesis that this current passes towards the warmth-pole, where is formed the atmosphere of light. In order to prove this directly, instead of the discharge-current passing through a Geissler tube
laid horizontally upon one of the free polar surfaces of the great electro-magnet, we may bring a narrow column of a conducting liquid upon the same surface. Such a column breaks, by being deflected at its middle point in opposite directions, if we plunge the electrodes of a galvanic battery of about two Grove's elements into its ends. Such breaking occurs in exactly the same direction as the former discharge-current, when the warmth-pole, with the magnetic light-atmosphere, corresponds to the negative electrode of the battery. We may also render the forces visible which act upon the electric light-current, by placing a shallow vessel of mercury upon the pole-surface of the excited magnet, and observing the motions which ensue in the liquid on the introduction into it of the two electrodes of the same battery. Or, finally, we may fasten two pivots upon two opposite points of the edge of the vessel, and balance upon them two strips of copper (which may have considerable weight), each provided with a platinum point at one end, in such a manner that the platinum points dip into the mercury above the centre of the polar surface, and in the line joining the two opposite pivots. If then the two electrodes of the battery be connected with the pivots, the current is completed through the mercury, and on exciting the magnet the two copper strips are forcibly driven asunder; and the direction of this repulsion agrees with the direction of the stream of light, if the latter be viewed as above described. Finally, if with the two hands we direct the wire which completes the circuit between the two armatures laid upon the polar surfaces, or equatorially above or below the place of greatest magnetic action, we feel the wire either strongly pulled downwards, or driven upwards according to the direction of the current and the kind of polarity which we call into play. And these two motions occur as with the luminous induced current, if we suppose its direction to be such that the warmth-electrode with the magnetic luminous atmosphere is the negative electrode.

Bonn, January 25, 1858.

XV. Chemical Notices from Foreign Journals.

By E. Atkinson, Ph.D.

[Continued from vol. xv. p. 459.]

Kündig* found that acetate of ammonia, simply distilled, loses water, and is converted in great part into acetamide. When heated, ammonia first escapes at 160° C., the thermometer remains constant for some time, and an acid distillate passes over which is probably acid acetate of ammonia. As soon as the thermometer rises above 160° C., the distillate contains a large quantity of acetamide, which at a little higher tem-

* Liebig's Annalen, March 1858.
perature solidifies in the condenser: above 190° C., the distillate consists almost entirely of pure acetamide. Kündig found the boiling-point to be 222° C., which agrees with that found by Dumas, Malaguti, and Leblanc.

The largest yield of acetamide is obtained when dry ammoniacal gas is passed for some hours through glacial acetic acid, which is at first kept cool, but can afterwards be heated to incipient ebullition. On subsequent distillation, above one-fourth of the glacial acid is obtained as acetamide.

Strecker has investigated some of the compounds and decompositions of acetamide*. He shows that this body, which is universally considered to be a neutral amide, has the property, like glycocol, aniline and lencine, and like benzamide, the type of a neutral amide, of forming definite combinations with acids. Their preparation is very difficult from their ready solubility, and from the case with which acetamide decomposes into ammonia and acetic acid.

Hydrochlorate of Acetamide was first obtained by treating acetamide with oxychloride of phosphorus, but is best prepared by treating a solution of acetamide in aether-alcohol with hydrochloric acid gas. If care be taken to prevent a rise of temperature, by cooling the liquid and merely passing the gas to the surface, it solidifies to a mass of crystals. These are washed with aether, in which they are insoluble, and are then dissolved in warm alcohol, from which they separate on cooling in long lance-shaped crystals. They are soluble in alcohol and in water, but not in aether, are strongly acid, and their alcoholic solution gives no precipitate with bichloride of platinum. The crystals gave on analysis numbers agreeing with the formula

$$2C_4H_5NO_2 + HCl.$$  

Nitrate of Acetamide.—When acetamide is dissolved in cold strong nitric acid and the solution exposed to the air in shallow vessels, colourless crystals are formed which are nitrate of acetamide. It has the formula

$$C_4H_5NO_2 + HNO_3.$$  

Mercury Acetamide, $C_4H_4HgNO_2$, is formed by adding yellow oxide of mercury to a solution of acetamide. The oxide disappears at first very easily, but afterwards it is necessary to warm the solution with excess of oxide. On evaporating the filtered solution in vacuo, colourless crystalline crusts are obtained, which are readily soluble in water, but sparingly soluble in alcohol.

Silver Acetamide is formed by adding freshly precipitated oxide of silver to aqueous solution of acetamide. On filtration and evaporation, crystalline plates of the compound in question are obtained.

* Liebig's Annalen, September 1857.
When dry acetamide is heated in a retort in a current of dry hydrochloric acid gas, there is obtained in the receiver a mixture of a colourless liquid and a crystallized distillate, while the retort contains a solid, difficulty-volatile residue. The liquid distillate consists of a mixture of chloride of acetylene and of concentrated acetic acid, together with traces of acetonitrile or cyanide of methylene. The crystalline mass was mostly soluble in æther. The insoluble part consisted of hydrochlorate of acetamide. The ætherial solution, on evaporation in vacuo over sulphuric acid, deposited hard granules; they gave on analysis the composition \( \text{C}^{12}\text{H}^{12}\text{N}^{2}\text{O}^{6} \) and consisted of a mixture in equal equivalents of acetamide and diacetamide,

\[
\text{C}^{12}\text{H}^{12}\text{N}^{2}\text{O}^{6} = \text{C}^{4}\text{H}^{5}\text{NO}^{2} + \text{C}^{6}\text{H}^{7}\text{NO}^{4}.
\]

The residue in the retort consisted of sal-ammoniac, and of a substance soluble in alcohol, and separating from its alcoholic solution in colourless columnar crystals. These were separated from adherent sal-ammoniac by re-solution in absolute alcohol. They were the hydrochlorate of a new organic base which neutralizes the strongest acids, and which Strecker names acediamine. The aqueous solution of the hydrochlorate gives with chloride of platinum no precipitate, but on evaporating the solution, large, hard, yellow crystals of a platinum double salt are obtained. This is soluble in water, but insoluble in a mixture of alcohol and æther.

It has the formula \( \text{C}^{4}\text{H}^{6}\text{N}^{2} \), HCl, PtCl\(^2\). The sulphate obtained by digesting the hydrochlorate with sulphate of silver, crystallizes in colourless pearly plates. It is easily soluble in water, and is quite neutral.

It was found impossible to prepare the base by treating the sulphate with excess of baryta, as in the free state it decomposes under assimilation of the elements of water into acetic acid and ammonia, according to the equation

\[
\text{C}^{4}\text{H}^{6}\text{N}^{2} + 4\text{HO} = \text{C}^{4}\text{H}^{4}\text{O}^{4} + 2\text{NH}^{3}.
\]

The decomposition of acetamide by dry hydrochloric acid yields diacetamide, acediamine, sal-ammoniac, and acetic acid, and small quantities of chloride of acetylene and of acetonitrile. These bodies result from various decompositions, which may be thus expressed:

\[
\begin{align*}
2(\text{C}^{4}\text{H}^{5}\text{NO}^{2}) + \text{HCl} & = \text{C}^{8}\text{H}^{7}\text{NO}^{4} + \text{NH}^{4}\text{Cl}. \\
\text{Acetamide.} & \quad \text{Diacetamide.} \\
2(\text{C}^{4}\text{H}^{5}\text{NO}^{2}) + \text{HCl} & = \text{C}^{4}\text{H}^{6}\text{N}^{2}, \text{HCl} + \text{C}^{4}\text{H}^{4}\text{O}^{4} \\
\text{Acetamide.} & \quad \text{Hydrochlorate of Acetic acid. acediamine.}
\end{align*}
\]

\[
\begin{align*}
\text{C}^{4}\text{H}^{5}\text{NO}^{2} + 2\text{HCl} & = \text{C}^{4}\text{H}^{3}\text{O}^{2}, \text{Cl} + \text{NII}^{4}\text{Cl}. \\
\text{Acetamide.} & \quad \text{Chloride of acetylene.}
\end{align*}
\]

\[
\begin{align*}
\text{C}^{4}\text{H}^{5}\text{NO}^{2} & = \text{C}^{4}\text{H}^{3}\text{N} + 2\text{HO}. \\
\text{Acetamide.} & \quad \text{Acetonitrile.}
\end{align*}
\]
The amides were divided by Gerhardt and Laurent into six classes, all of which are derivable from neutral or acid ammonia salts of monobasic or polybasic acids by the loss of water; and acediamine would be an amide derivable from a basic ammonia salt, inasmuch as by its decomposition it yields one equivalent of acetic acid and two of ammonia. Strecker considers it probable that many of the natural alkaloids, which contain in one equivalent two equivalents of nitrogen, might be derived like acediamine, from two equivalents of ammonia and one equivalent of a neutral or acid body.

In an investigation on monochloracetic acid, Hofmann* observed a peculiar decomposition of its salts, in which, by simply heating the air-dried salt, a metallic chloride was formed, and the previously neutral liquid assumed an acid reaction. He suggested that this might take place by the assimilation of water, and an acid be formed which would have the composition of glycolic acid,

$$C_4H^2Cl \cdot MO^4 + 2HO = C_4H^4O^6 + MCl.$$

Kekulé† has examined this reaction, and has found Hofmann’s supposition confirmed, and that glycolic acid is in fact formed from acetic acid. This is interesting, as being the first example of the formation of a biatomic acid from a monoatomic acid of the acetic acid group. It is in fact, in the series of acids, exactly parallel to the formation of glycol from alcohol in the series of alcohols. Glycolic acid bears the same relation to acetic acid that glycol does to alcohol,—

$$\text{Alcohol, } \frac{C_4H^3O^2}{H} \text{. Acetic acid, } \frac{C_4H^3O^2}{H} \text{. Glycol, } \frac{C_4H^4O^2}{H^2} \text{. Glycolic acid, } \frac{C_4H^4O^2}{H^2} \text{.}$$

Monochloracetate of potash, heated for some time to 120° C., becomes yellow and assumes an acid reaction. On treatment with cold water, glycolic acid and chloride of potassium are dissolved out, and there is left a small quantity of a white powder which is glycolide, the formation of which might be thus expressed:—

$$C_4H^2KClO^4 = KCl + C_4H^2O^4.$$

From the aqueous solution which still contains chloride of potassium, the acid is easily obtained by evaporating the solution to dryness, and extracting with a mixture of alcohol and aether. After evaporating the alcohol the acid remains as a yellow syrup. Kekulé confirmed the identity of the acid by preparing and analyzing the silver, lime, baryta, and lead salts.

Kekulé made an experiment to ascertain if this reaction ex-

* Phil. Mag. vol. xiv. p. 56. † Liebig’s Annalen, March 1858.
tended to the aromatic series of acids. According to analogy, monochlorobenzoate of potash ought to yield oxybenzoic acid,

$$C_{14}H_4KClO_4 + H^2O^2 = C_{14}H_6O_6 + KCl.$$  

Accordingly chlorobenzoate of potash was heated, both dry and in concentrated aqueous solution, from 180° to 200°. In both cases decomposition ensued, but not as expected; the contents of the tube, instead of an acid, had an alkaline reaction. Probably chlorobenzoic and carbonate of potash were formed,

$$C_{14}H_4ClKO_4 + H^2O^2 = C_{12}H_5Cl + C_2O_6KH.$$  

Chlorobenzoate  Chlorobenzole  Carbonate  of potash.

The direct production of urea from albumen by oxidation by means of permanganate of potash, which was stated to have been effected by Béchamp*, has been subjected by Städele** to a careful examination, and he has arrived at results entirely differing from those of Béchamp. About 50 grms. white of egg (or 6 grms. albumen), diluted with water to 180 grms., were heated to a temperature of 50° to 55° C. with 1 grm. permanganate of potash. The action took place very slowly; and when decolorization was complete, the solution no longer gave the reactions for albumen: an additional quantity of permanganate was added, and the whole maintained for some time at the previous temperature. The liquid was no longer decolorized; and hence the oxidation for this temperature was complete. To decompose the excess of permanganate, some alcohol was added, the liquid filtered, neutralized with sulphuric acid, and evaporated to dryness, the residue extracted with alcohol, the solution poured off from the deposit (which consisted of sulphate of potash and a yellowish syrup), and evaporated to a small volume. This residue contained no crystals of urea; but when nitric acid was added, crystals were formed which had indeed some resemblance to that substance, but could not be taken for it without further investigation.

If these crystals were nitrate of urea, urea could readily be obtained therefrom: they were dried between blotting-paper and treated with moist carbonate of baryta, filtered and evaporated, and the residue extracted with alcohol. In this solution no urea was found; on the contrary, all its reactions exhibited in the most characteristic manner the presence of benzoic acid in the form of benzoate of baryta. And the presence of benzoic acid is the less surprising, as it was found by Guckelberger as a product of the action of chronic acid on proteine substances. It is re-

* Phil. Mag. vol. xii. p. 537.

markable that, by the action of permanganate of potash, a comparatively considerable quantity of benzoic acid was obtained.

Béchamp mentions nothing of the occurrence of benzoic acid in the oxidation products of albumen; and it must hence be assumed that he has mistaken it for nitrate of urea. And the fact that the alcoholic solution of the product of oxidation gave a precipitate not only with nitric acid, but also with oxalic acid and with protonitrate of mercury, would quite agree with this assumption.

XVI. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 69.]

Dec. 17, 1857.—Major-General Sabine, R.A., Treas. and V.P., in the Chair.

The following communication was read:—

"Observations on the Poison of the Upas Antiar." By Prof. Albert Köllicker, of Wurzburg.

During my stay in England, in the autumn of 1857, I was so fortunate as to acquire the rare poison of the famous Antiaris toxicaria (Lesch.), with which no experiments have been tried since the time of Magendie, Brodie, Horsfield, and Schnell and Emmert (1809–1815). I owe my specimens of the Antiar poison to my friend Prof. Christison, of Edinburgh, who had it from Borneo, and to Dr. Horsfield, of London, who collected it himself during his stay at Java in the beginning of this century; and as both specimens were fully active—as some preliminary experiments made in company with my friends Dr. Sharpey and Dr. Allen Thomson showed—I thought it well worth while to devote some time to the study of the poison, and to try to elucidate its manner of action on the animal organism. The following are the principal results which I obtained in my experiments with frogs, and I hope that they will not be deemed unworthy of notice by those who take an interest in the physiological action of poisons in general.

The Antiar, like most other poisons, acts from the intestinal canal, and from wounds; but it must be remarked, that it is much more energetic and rapid when introduced into a wound. The symptoms which are observed in frogs, in the latter case, are the following:—First of all, the voluntary movements become less energetic, and at length cease totally, 30 to 40 minutes after the introduction of the poison (after 21 min. and 1¹⁄₂ 21 max.). Then follows a time in which reflex movements may be caused by stimulating the skin; but this faculty also is lost very soon, viz. at from 50 to 60 minutes (at 33 min. and 85 max.); and the animals die without the slightest trace of convulsions or tetanic spasm. If now the frogs are opened, we find that, without any exception, the heart has ceased to beat. The auricles are dilated, the ventricle corrugated, rather small, and generally red, as if blood had been extravasated into its muscular parietes; but very soon the exposure of the heart to the air causes
the ventricle to shrink a little more, and to become pale and stiff, as if in the state of *rigor mortis*. All interior organs, especially the lungs, liver, stomach, intestine, and kidneys, are gorged with blood, and in a state of great, especially venous, hyperemia. The blood is fluid and rather dark, but soon coagulates when exposed to the air, and assumes a brighter colour. The lymphatic hearts cease to beat as soon as the reflex movements are lost. At the same time the nerves are yet found excitable, but their power is very low, and generally vanishes in the second hour after the application of the poison. The same must be said of the muscles, which contract very feebly when directly stimulated by galvanism, and in most cases lose their power totally in the second or third hour, and generally a little after their nerves. The *rigor mortis* begins early, sometimes in the sixth hour, and is generally well established at the eighteenth hour.

Amongst all these symptoms, to which we may add some signs of vomiting occurring now and then, there was none which attracted my attention more than the cessation of the movements of the heart, considering the great energy which this organ possesses in frogs; and I tried, therefore, before all, to elucidate the action of the Antiar upon the heart. For this purpose I instituted a new series of experiments, in which I exposed the heart by the section of the sternum, before the poison was introduced into a wound of the back; and in this way I easily got the result, that the heart ceases to beat as soon as from the fifth to the tenth minute after the introduction of the Antiar; and so, that first the ventricle stops, and half a minute or one minute later, also the auricles. Now, as the frogs at this time are not at all deprived of their faculty to move, we may have the rather astonishing view of an animal, with artificially-paralysed heart, which moves and leaps as freely as if nothing had happened.

The experiments just mentioned prove, that the first action of the Upas Antiar is to paralyse the heart; and I am therefore quite in accordance with Sir Benjamin Brodie, who, by his experiments on mammals, came to the same result in 1812; whilst I cannot otherwise than disagree with Schnell (Diss. de *Upas Antiar*, Tubinge, 1815), who assumes that this poison acts in the first place on the spinal marrow. Now this point fixed, the further question arises, whether the other symptoms mentioned, viz. the paralysis of the voluntary and reflex movements, and the loss of the irritability of the muscles and nerves, are only the results of the paralysis of the heart, or must be attributed to a specific action of the Antiar. For the elucidation of this question, I found it necessary to study the consequences of the suppression of the heart's action on the organism of frogs, which I did in the same way as it had been done by others, especially by Kunde (Müller's Archiv, 1847); viz. by cutting out the heart, or by putting a ligature around the base of it, so as to stop the circulation totally. The results of these experiments were in both cases the same, that is to say, the voluntary movements ceased in from 30 to 60 minutes, and the reflex movements after one or two hours. Hence it follows that these two symptoms of the poisoning
with Antiar are simply dependent on the paralysis of the heart caused by it. With reference to the irritability of the muscles and nerves, on the contrary, it is easy to show that the ligature or excision of the heart has not the same influence as the Antiar; inasmuch as in the first case the muscles and nerves are found irritable six or seven hours, and more, after the experiment has been made. Therefore it may be said that the Antiar has a direct action on these organs.

These points once demonstrated, there remained one more question to elucidate, namely, whether the Antiar acts only upon the muscles, or also upon the nerves. If we consider that the Antiar undoubtedly paralyses the muscles, we may easily see that the loss of the excitability of the nerves possibly depends merely upon the impairment of the muscular contractility, and is therefore not real, but only apparent. With a view to determine the real state of things, I tried a third series of experiments—poisoning frogs in such a manner that the muscles of one limb were kept free from the influence of the poison. This was done in two ways: first, by putting a ligature round the crural artery and vein of one leg; and secondly, by cutting through a leg entirely, after the ligature of its vessels, with the exception only of the ischiadic nerve. In poisoning frogs treated in one of these ways, through a wound of the back, I found that, with the exception of the heart, the Antiar acts in the first instance upon the muscles. This is shown by the fact, that in the second hour, at the time when the muscles of the poisoned parts have lost their irritability, the nerves of the sacral plexus in the abdomen still possess their full influence upon the muscles of the leg which has been kept free from the action of the poison. One might be inclined from this to conclude, that the nerves are not at all acted upon by the Antiar; but this inference would be erroneous. In fact, the experiments just mentioned, if followed a little longer, show that in the third or fourth hour the sacral plexus also becomes inactive, at a time when the muscles of the non-poisoned leg are fully contractile. The Antiar, therefore, paralyses also the nervous trunks, but later than the muscles.

From all these experiments, it seems to follow that the Antiar is a poison which acts principally upon the muscular system (the heart and the voluntary muscles), a conclusion, in favour of which I may further add, that the muscles and the heart of frogs poisoned by Urari (Woorara, Curare) lose their irritability totally, and in a short time, if Antiar is introduced into a wound some time after the Urari. If we consider that, as I have shown (see Proceedings of the Royal Society, 1856, p. 201), the Urari only acts upon the terminations of the nerves in the muscles, and does not affect the irritability of the heart and muscles at all, we may conclude, that a poison, which, as the Antiar, is capable of paralysing the muscles after the Urari, has really a direct action upon the muscular fibre.

The results of my investigation into the effects of the Antiar upon frogs, are therefore the following:—

1. The Antiar is a paralysing poison.
2. It acts in the first instance and with great rapidity (in 5 to 10 minutes) upon the heart, and stops its action.
3. The consequences of this paralysis of the heart are the cessation of the voluntary and reflex movements in the first and second hour after the introduction of the poison.

4. The Antiar paralyses in the second place the voluntary muscles.

5. In the third place it causes the loss of excitability of the great nervous trunks.

6. The heart and muscles of frogs poisoned with Urari may be paralysed by Antiar.

7. From all this it may be deduced, that the Antiar principally acts upon the muscular fibre and causes paralysis of it.

So much for this time. My experiments with the Antiar upon warm-blooded animals have only begun, and I am not yet able to draw any conclusion from them. As soon as this will be possible, I shall take the liberty to submit them to the Royal Society, together with the results of my experiments with the *Upas tienté*, which poison I had also the good fortune to obtain through the kindness of Sir Benjamin Brodie and Dr. Horsfield. With regard to the Antiar I may further add, that experiments made independently, and at the same time, by my friend Dr. Sharpey with this poison, have conducted to the same results as my own.

January 7, 1858.—J. P. Gassiot, Esq., Vice-President, in the Chair.

The following communication was read:


In the spring of 1857, Her Majesty's Government, designing to send an expedition to examine and survey the yet unsettled country north of the boundary-line between the British territory and that of the United States, and comprised between Canada on the east and the Rocky Mountains on the west, notified their intention to the Royal Society, and invited suggestions regarding any objects of physical research, for which the Royal Society might deem this to be a fitting occasion.

Amongst the subjects to which attention was called in the reply, the expediency of confirming and extending the Magnetic Survey of British North America, which, at the instigation of the Royal Society, was made in the years 1843 and 1844, and of which the results are contained in the 'Philosophical Transactions' for 1846, Art. XVII., was not forgotten; and Lieut. Blakiston, of the Royal Artillery, personally known to Mr. Palliser, the conductor of the proposed Expedition, having been appointed to the special charge of the Magnetic Observations, and to assist generally in Geographical Determinations, the Royal Society undertook to provide the instruments suitable for the purpose, and with the sanction of the Committee of the Kew Observatory of the British Association, placed their preparation under the superintendence of Mr. Welsh, Director of that Observatory, where also Lieut. Blakiston received instructions for their use, and acquired practical experience in their manipulation. About the middle of June, Lieut. Blakiston sailed in the Hudson's Bay Com-
pany's ship the 'Prince of Wales' for York Fort, where he arrived on the 16th of August, and after completing the Magnetic Observations which he had been charged to make at that station, proceeded on the 30th of the same month, by the canoe route, to join Mr. Palliser, who had quitted England some days before him, and had taken the route by the United States to Canada and the Red River Settlement, and thence to Carlton House, where the whole party would be assembled in the fall.

The care which Lieut. Blakiston bestowed upon his determinations at York Fort appears to have been commensurate with the theoretical importance which, before he quitted England, he was aware would attach to the results. In submitting these to the Society, I must solicit a continuance of the patience and indulgence so kindly given to me on a recent occasion; for the subject of Terrestrial Magnetism is far less generally understood than I believe it deserves to be; and there is often an apparent complexity in the details, especially to those who are not familiar with the subject, which requires time to be occupied in their elucidation. I shall commence with showing the confirmation which Lieut. Blakiston's results give to the approximate accuracy of the value assigned in the 'Philosophical Transactions' for 1846, for the absolute magnetic force at its principal point of maximum in the northern hemisphere.

Those who are conversant, either from personal recollection or as a matter of history, with the opinions regarding the phenomena of terrestrial magnetism entertained in the first quarter of the present century, will scarcely need to be reminded how generally the belief then prevailed, that the magnetic dip and the intensity of the magnetic force at different points of the earth's surface might be represented with at least a sufficient approximation by mathematical formulae, obtained by supposing the magnetism of the earth to be concentrated into two magnetic poles, very near to each other and to the earth's centre; the supposition being also equivalent to that of an infinite number of small magnets parallel to each other, distributed equally throughout the earth's surface. According to this supposition, the greatest intensity of the magnetic force in each of the two hemispheres should be found at the points where the dip should be $90^\circ$, and the intensity should vary in the proportion of $2:1$ between places where the dip should be respectively $90^\circ$ and $0^\circ$.

In the Arctic Expeditions of 1818, 1819, and 1820, I had an opportunity of measuring the intensity of the magnetic force at several stations in the immediate vicinity of the dip of $90^\circ$; and in the years 1821 and 1822, of comparing these measures with others made at several points of the coasts of Europe, Africa, and America, and at islands in the Atlantic Ocean (which I visited for the purpose of making observations with the pendulum), in dips which, including the Arctic stations, varied from $0^\circ$ to $88^\circ 47'$. The result of this comparison was to place beyond a question the irreconcilability of the phenomena with the supposition of a coincidence between the points of $90^\circ$ of dip and of the maximum of force. For example, the magnetic force was found to be considerably greater at New York, where the dip was not more than $73^\circ$, than at the stations in the Polar Sea.
On Magnetic Observations transmitted from York Fort. 145

where it was nearly 90°; and by graphical delineations, according to well-known methods, in which all the observations were taken into the account, it was shown that whilst the dip of 90° could not be in a more southerly latitude than 70°, the greatest intensity of the force would be found somewhere about the 53rd parallel in the vicinity of Hudson’s Bay, not less than 1000 geographical miles distant from the point of 90° of dip with which it had been supposed to coincide.

The hypothesis, so generally put forward in the elementary treatises on Magnetism of that period, was therefore shown to be no longer tenable. It was in fact specially one of that class of speculations designated by Bacon as "anticipations of nature," of which it is so commonly the fate to be swept away, as knowledge advances by that more slow and gradual, but more philosophical and certain "interpretation of nature," which results from a strictly inductive process.

Steadily pursuing this last-named process, the Royal Society—after provision had been made by the establishment of Colonial Magnetic Observatories for a systematic examination of the phenomena of the variations of comparatively small amount, which are produced at the surface of our planet by the influence of other bodies of our system; and by the Antarctic Expedition of Sir James Ross, for the magnetic survey of such portions of the higher latitudes of the southern hemisphere as are accessible to navigation,—recommended to Her Majesty’s Government, that in the northern hemisphere a magnetic survey should be made of those parts of the British possessions which were adjacent to the position which observation had indicated as that of the principal maximum of the magnetic force in that hemisphere. This recommendation was carried out in 1843 and 1844, and the particulars of the survey, together with the conclusions derived from it, form No. VII. of the magnetic contributions in the 'Philosophical Transactions' for 1846, Art. XVII. The geographical position of the maximum of magnetic force derived from the combination of the 78 stations of that survey was 52° 19' N. and 91° 59' W. of Greenwich, and the absolute value of the force at its point of maximum was found to be 14.21 in British units (i.e. of mass, a grain; of time, a second; and of space, a foot). As both the geographical position of the point of maximum and the absolute value of the force prevailing there, are subject to a secular variation, of which the nature, the period, and the epochs are desiderata of the highest theoretical importance,—and as the determinations which are now made may therefore probably be referred to as data by remote posterity,—their confirmation, by the observations of a second observer visiting the same localities within a few years of the same date, furnished with different instruments, and pursuing in some respects different methods, was viewed as a circumstance much to be desired by the Committee of the Royal Society appointed, at the request of Her Majesty’s Government, to suggest scientific desiderata, to be accomplished by Mr. Palliser’s North American Expedition.

York Fort had been one of the stations visited by Lieut. (now Lieut.-Col.) Lefroy, in the Survey of 1843–44. It is situated Phil. Mag. S. 4, Vol. 16, No. 105. Aug. 1858.
nearly due north of the point of maximum deduced from that survey, and less than 300 miles distant from it. The intensity of the force at York Fort in July 1843, derived from the combined observations of the inclination and of the horizontal force observed by Gauss's well-known absolute method, was 14°07; and by Mr. Fox's statical apparatus, taking Toronto as a base, 14°03. We have now to compare with these Lieut. Blakiston's results in August 1857, viz. 14°024 by the combination of the inclination and the absolute horizontal force, and 14°017 by a recent improvement of Dr. Lloyd's statical method, which renders the result independent of changes which may take place in the magnetic moment of the needle employed in the determination. The first of these two last-named results has been computed by Mr. Welsh, of the Kew Observatory, from the observations received from Lieut. Blakiston, who was too much pressed for time by his approaching departure from York Fort to compute them himself. The second is the mean of five determinations on three different days, which were computed by himself on the spot; they are severally as follows:—

<table>
<thead>
<tr>
<th>Date</th>
<th>Time</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>August 20th</td>
<td>noon</td>
<td>14°03</td>
</tr>
<tr>
<td></td>
<td>20th</td>
<td>14°01</td>
</tr>
<tr>
<td></td>
<td>22nd</td>
<td>14°024</td>
</tr>
<tr>
<td></td>
<td>24th, noon</td>
<td>14°00</td>
</tr>
<tr>
<td></td>
<td>24th, 3 P.M.</td>
<td>14°02</td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td>14°017</td>
</tr>
</tbody>
</table>

We have therefore by the mean of the two methods in 1843, 14°05, and by the mean of the two methods in 1857, 14°02, differing only about 1/40 th from each other. As far, therefore, as agreement at a single station may be regarded as confirming the conclusions of the survey of 1843-44, Lieut. Blakiston's results furnish that confirmation; and judging from the result at the first station at which the comparison has been made, we may anticipate, from the opportunities which he is likely to have of repeating observations at other stations of the former survey, as well as of adding stations previously unvisited, that the ultimate conclusion in respect to the absolute value of the magnetic force at its point of maximum at this particular magnetic epoch, will be as perfect as could be desired. With respect to its present geographical position, we may also hope that Lieut. Blakiston may have an opportunity, before his employment is terminated, of removing any doubts that may exist as to the precision of the longitude assigned to it by the survey of 1843-44. It cannot have escaped notice that the 78 stations of that survey, which by their combination assigned the latitude and longitude of the point of maximum, did not perfectly fulfil one important condition regarding their distribution, viz. that of symmetrical arrangement on all sides of the point in question. There was a considerable preponderance of stations situated on the west of the meridian of the point itself, and a deficiency on the eastern side, which might have been remedied, had circumstances permitted, by a line of stations as originally contemplated on the canoe route from Canada to Moose Fort at the south-western end of Hudson's Bay, and possibly by some additional stations between Moose Fort and York Fort.
experience which Lieut. Blakiston has had in canoe-travelling will have prepared him to profit by the opportunities it may afford for observation, and the route referred to is one of the ordinary canoe routes of the Hudson's Bay Company; with this addition, the determinations of geographical position and of the value of the magnetic force at its point of maximum, may be expected to be amongst the most perfect, as they will undoubtedly be amongst the most important data, in this great branch of Physical Geography.

I proceed to notice Lieut. Blakiston's observations upon the magnetic declination at York Fort, which, taken in conjunction with those of the survey in 1843-44, tend to substantiate conclusions of no less theoretical importance than those with which we have been occupied regarding the magnetic force. It is well known to those who are conversant with the phenomena of the secular change of the declination, that during the whole of the last century, and for some time after the commencement of the present century, the secular change which took place in the position of the isogonic lines in the northern parts of the North American continent, consisted in the progressive translation of the lines from west to east. The line of no declination, for example, to which, when Halley collected and coordinated the most trustworthy observations previous to the publication of his Magnetical Map in 1702, he assigned a position "about the meridian of the middle of California" (Phil. Trans., No. 148), appears in Hansteen's 'Mappa hydrographica sistens Declinationes magneticas Anni 1787' (Erdmagnetismus, Atlas), at the latter epoch, as crossing Lake Superior, and proceeding from thence in a direction west of north, so as to pass altogether to the west of any part of Hudson's Bay; whilst from well-assured observations of a still later date we know that soon after the beginning of the present century, places situated on the western shores of Hudson's Bay had east declination, showing that the line of no declination had passed over and was now to the east of them. Consistently with this general movement of the isogonic lines from west to east, the declination at York Fort, which, according to the observations of Capt. Middleton (Phil. Trans. 1726; No. 393, and 1731, No. 418) was at least 19° West in 1725, had diminished to about 5° West in 1787 (Hansteen, l. c.). In September 1819 it was found by Sir John Franklin to be 6° East (Journey to the Shores of the Polar Sea, 1819-22, p. 26), and by Lieut.-Colonel Lefroy in 1843, 9° 25' East. Thus we perceive that in little more than a century (from 1725 to 1843) the declination at York Fort had changed progressively, by the operation of secular change, not less than 28°, always in the direction of westerly decreasing or easterly increasing; (which is in effect the same as a movement of translation of the isogonic lines from west to cast).

In 1841 the Toronto Observatory commenced its observations, and although (from defective instrumental organization) the conclusions in regard to the secular change of the declination were not at first as precise as could be desired, they were sufficiently so to justify a strong persuasion that some very notable change had recently taken place in the order of the phenomena, and to lead to the commencement, in January 1845, of a special series of monthly determi-
nations in a detached building, appropriated chiefly to a close investigation into the direction and amount of the secular change. The result is stated in the 3rd volume of the Toronto Observations, p. cxxvi, and is as follows:—"The secular change of the declination from 1845 to 1851 inclusive was an annual increase of 1°95 of west declination. From July 1851 to April 1854 (two years and nine months) an annual increase of 2°54: and assuming the circumstances of a new series commenced in 1855 with the same instrument placed in a new building to be strictly comparable with those of the old series, the increase from April 1854 to October 1855 is at the mean annual rate of 3°54." The progressively increasing amount of the rate of secular change is a circumstance which, for obvious reasons, may be expected to follow for a time after the reversal of the direction of the change.

Attention being thus alive, particular care was taken that the azimuth compass with which Lieut. Blakiston was supplied should be free from instrumental error, and the practice was recommended to him of repeating observations at different hours and on different days. The following is a transcript of the report received from him from York Fort, showing how thoroughly these directions were kept in view:—

"Declination at York Fort, 1857.

<table>
<thead>
<tr>
<th>Date</th>
<th>Time</th>
<th>Declination</th>
</tr>
</thead>
<tbody>
<tr>
<td>17th Aug</td>
<td>5 30 P.M.</td>
<td>7 01 E.</td>
</tr>
<tr>
<td></td>
<td>5 43 P.M.</td>
<td>7 21 E.</td>
</tr>
<tr>
<td></td>
<td>6 14 P.M.</td>
<td>7 43 E.</td>
</tr>
<tr>
<td>20th Aug</td>
<td>5 16 P.M.</td>
<td>7 41 E.</td>
</tr>
<tr>
<td></td>
<td>5 53 P.M.</td>
<td>7 24 E.</td>
</tr>
<tr>
<td>26th Aug</td>
<td>5 54 A.M.</td>
<td>8 01 E.</td>
</tr>
<tr>
<td></td>
<td>6 40 A.M.</td>
<td>7 57 E.</td>
</tr>
<tr>
<td></td>
<td>7 20 A.M.</td>
<td>7 50 E.</td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td>7 37 E.</td>
</tr>
</tbody>
</table>

"Ten to twelve observations in each set, the compass being lifted and shaken between each observation."

The observations of Franklin in September 1819 gave 8 00 E.
Those of Lefroy in July 1843 gave 9 25 E.
Those of Blakiston in August 1857 gave 7 37 E.

It appears therefore that the secular variation which between 1819 and 1843 caused an increase of east declination, caused on the contrary between 1843 and 1857 a decrease of east declination. This is a reversal in the same sense as that which has been seen to have taken place at Toronto. It seems probable from an inspection of the intervals, and of the differences of declination-value, in the three determinations above noticed, that the epoch of reversal must have coincided very nearly with that of the survey of 1843-44; and consequently that Lieut.-Col. Lefroy's result may show approximately the maximum which the easterly declination attained at York Fort before the change took place. If we might assume 1843 to be the precise epoch, it is deserving of remark that it is the same year in which the observations of the inclination at Toronto show that the annual secular variation of that element changed from a decreasing
On Magnetic Observations transmitted from York Fort. 149

to an increasing rate. The dip observed by Lieut. Blakiston at York Fort was $83^\circ 53'$ in 1857, and by Lieut.-Col. Lefroy $83^\circ 47'2$ in 1843, showing, as at Toronto, a slight increase to have taken place in that element in the interval.

I am indebted to Dr. Norton Shaw, Secretary of the Royal Geographical Society, for a copy of declinations observed by Mr. Palliser in his passage between Fort William and the Red River Settlement. It happens that four of the stations in this route, at which Mr. Palliser observed the declination in the summer of 1857, had been stations of Lieut.-Col. Lefroy in 1843-44. They are as follows:

<table>
<thead>
<tr>
<th></th>
<th>Lat.</th>
<th>Long.</th>
<th>1843-44</th>
<th>1857.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Savannah Portage</td>
<td>48 53 N.</td>
<td>90 05 W.</td>
<td>7 46 E.</td>
<td>6 53 E.</td>
</tr>
<tr>
<td>Fort Francis</td>
<td>48 36</td>
<td>93 30</td>
<td>9 36</td>
<td>9 31</td>
</tr>
<tr>
<td>Lake of the Woods</td>
<td>49 27</td>
<td>94 44</td>
<td>12 16</td>
<td>10 17</td>
</tr>
<tr>
<td>Lake Winnipeg</td>
<td>50 28</td>
<td>96 35</td>
<td>15 30</td>
<td>14 25</td>
</tr>
</tbody>
</table>

Means. . . . . 11 17 E. 10 14 E.

At all the stations the easterly declination is less in 1857 than in 1843-44; and on the average of the four stations it would appear to have decreased about $1^\circ$ in the fourteen years.

It would be unjust to the memory of the profound and sagacious philosopher, by whom, more than 150 years ago, the facts both of the magnetic declination in different parts of the globe and of its changes were first collected and framed into an hypothesis (Halley in Phil. Trans. 1692, No. 193), if we were to fail to recognize that this reversal in the direction of the motion of the isogonic lines, in the vicinity of the principal magnetic pole in the northern hemisphere (using the term 'pole' in the physical sense in which Halley employed it), is conformable to the hypothesis which he propounded at that early date,—"to explain," according to his own words, "the change in the variation (declination) of the magnetic needle." By the supposition of a double system of the terrestrial magnetic forces, occasioning two poles or principal points of attraction in each hemisphere producing resultant phenomena in all parts of the surface of the globe according to their relative strength and proximity, Halley showed that all the apparently complex phenomena of the magnetic direction might be systematically represented; and by the further supposition that one of the two systems (the stronger one) was fixed, and the other (the weaker one) possessed a gradual and slow motion, that a reasonable explanation could be given of the phenomena of the secular change in different parts of the globe, as far as they were known in his time. At the period when this hypothesis was originated, viz. in 1692, the two poles in the northern hemisphere were considered to be situated as follows: that of the stronger and fixed system in North America, about the meridian of the middle of California, and that of the weaker and moving system, about the meridian of the British Islands, having a progressive motion towards the east. Now as the resultant phenomena in the north of America,
though influenced principally by the nearer and stronger system, would still exhibit in a slighter degree the influence of the weaker and moving system, the isogonic lines in that part of the globe should have, according to the hypothesis, a movement of translation from west to east conformably to the motion of the weaker system, until the difference in longitude between the poles of the respective systems should amount to 180°, an event which would constitute an epoch in the secular magnetic variations, characterized (amongst other circumstances) by the reversal of the motion of the isogonic lines in America, which would thenceforward take place from east to west, as the distance between the poles should diminish on the Siberian side of what Halley termed the American Pole. Now it is well known that the expedition of MM. Hansteen, Erman, and Due, across the continents of Europe and Asia in 1828 and 1829, had, for its principal object, the determination of the magnetic phenomena around the point of maximum attractive force of the weaker or moving system; and that the position those gentlemen assigned to it in longitude at the time of this expedition was about 115° East of Greenwich, to which meridian it had progressively moved in the interval which had elapsed since Halley assigned its position near the meridian of our Islands. Fully recognizing that in the present, as in the earlier state of magnetical science we can only regard such assignments as approximate, we have still full reason to believe that about the time of the memorable expedition of MM. Hansteen, Erman, and Due, i. e. a few years earlier or a few years later than 1828–29, the epoch must have occurred when the points of greatest attraction of the two systems in the northern hemisphere must have passed through their greatest longitudinal distance from each other, and when, according to Halley's hypothesis, the direction of the movement of translation of the isogonic lines in the northern parts of America should be reversed, which we find to have now taken place.

I have ventured to think that these few remarks, recalling to recollection an hypothesis which was not framed without a most laborious coordination and sagacious grouping of the phenomena which it professed to represent, and which has its place in the earlier volumes of our Transactions, would not be unacceptable to the Members of the Royal Society, of which Society Halley has ever been regarded as one of the brightest ornaments.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 72.]

May 25, 1857.—Mr. Bashforth made a communication "On some Calculations and Experiments undertaken for the purpose of testing the Theories of Capillary Action."

Also Mr. Candy exhibited a Physiological Alphabet.

The principle of this alphabet is to make the form of the letter indicate the manner in which the sound is produced, by showing the position of the organs of speech concerned.
The open mouth, affording an uninterrupted, unimpeded passage to the breath, is represented by two horizontal straight lines (—).

The sudden, complete, closing of the lips, which gives the sound of the letter P, is represented by drawing a line joining these two on the left (—); the mouth looking in the same direction as the Queen's head on the coins and stamps.

All the letters in the first column of the Table have a line on the left, and are therefore labials.

The second column, having a line inclining to the left, are dentals.

The third column have a vertical line in the centre of the mouth, and are the linguals.

The fourth column incline to the right, and are the palato-linguals or weakened linguals.

The fifth column, with a line curving over to the right, are the Sanscrit cerebrals, in which the tongue is curled up, so that the lower side of it comes against the roof of the mouth.

The sixth column, in which the line leans from the right to the centre, are the palatals.

The seventh column, with a vertical line on the right, are the gutturals, at the back of the mouth.

The eighth column, with a line on the right, leaning to the right, are the faucals, still further back than the gutturals.

The first row of letters in the Table, with a thin line from top to bottom, are the mute explosents, in which there is a sudden and complete stoppage of the breath.

The second row, with a thick line, are the corresponding sonant explosents.

The third row, in which there is an interval between the top line and the connecting line, are the whispered continuants, in which there is a passage of breath, producing a hissing.

The fourth row, with a short thick line, are the corresponding voiced continuants, or buzzes.

The fifth row contains additional hissing letters, and the sixth row corresponding additional buzzing ones.

In the letters of the seventh row, there is represented an opening upwards at the back of the mouth, leading into the nose. These, therefore, are the nasal letters.

The double curl in the eighth and ninth rows indicates a vibration of both sides of the tongue, while the tip is fixed. This is the case in L, which is represented in Sanscrit by a similar form. The eighth row, with thin lines, are the whispered Ls.

The tenth and eleventh rows, with a single curl, are the Rs or trills. The curl indicates vibration.

The twelfth row are the Caffre clicks. The short line darting out from an angle, shows that the tongue is placed in a certain position, and then suddenly jerked away.

The thirteenth row are the breathings.

The dots and accents (· †) in the last row are the vowel-points.

The last figure shows the applicability of this alphabet to monograms, being a combination of F, DZh=J, and K=C, the initials of the inventor, F. J. Candy.
The analysis of sound exhibited in this alphabet is that in ‘Universal Writing and Printing,’ by Mr. A. J. Ellis, a Fellow of this Society.

The following is Mr. Ellis’s Universal Digraphic Alphabet arranged in the order of this alphabet, to facilitate comparison.

| p  | tt | t | tj | t' | k | q |
| b  | dd | d | dj | d' | g | g |
| ph | ss | s | sj | s' | ch | kh |
| bh | zz | z | zj | z' | jh | gh |
| wh | f  | th| sh | yh | x  |
| w  | v  | dh| zh | y  |
| m  | nn | n | nj | n' | ngj | ng |
| lkh| lh |
| l  | l | lj | l' |
| rh | r |
| brh| r  | nj | zrh | t  | grh |
| cc | c  | ck | cj | c' |
| l  | n | nh | a |

Key to the Consonants and Breathings.
[See the Table for the forms of the Physiological consonants.]

1. \[
\begin{align*}
 p & \quad P, \pi, \daleth \text{ with dagesh.} \\
 tt & \quad \text{Arabic dental } t, \text{ Hebrew } \daleth \\
 t & \quad T, \tau, \nun \text{ with dagesh.} \\
 t' & \quad \text{Sanskrit cerebral } t. \\
 kj & \quad \text{French } qu, \text{ Italian } chi. \\
 k & \quad K, \kappa, \daleth \text{ with dagesh.} \\
 q & \quad \text{Hebrew } \daleth \\
\end{align*}
\]

2. \[
\begin{align*}
 b & \quad B, \daleth \text{ with dagesh.} \\
 dd & \quad \text{Arabic dental } d. \\
 d & \quad D, \tau, \nun \text{ with dagesh.} \\
 dj & \quad \text{Hungarian } gy. \\
 d' & \quad \text{Sanskrit cerebral } d. \\
 gj & \quad \text{Old Engl. guard, French } gueux. \\
 g & \quad G, \daleth \text{ with dagesh.} \\
\end{align*}
\]

3. \[
\begin{align*}
 ph & \quad \phi, \daleth \text{ without dagesh.} \\
 ss & \quad \text{Arabic dental } s, \text{ Hebrew } \aleph. \\
 s & \quad S, \sigma, \daleth. \\
 sj & \quad \text{Polish } \acute{s}, \text{ Hebrew } \aleph. \\
 sh & \quad \text{Sanskrit cerebral } sh. \\
 ch & \quad \text{German palatal } ch \text{ in “ich.”} \\
 kh & \quad \text{German guttural } ch \text{ in “ach!”} \\
\end{align*}
\]
<table>
<thead>
<tr>
<th>Alphabet</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>&quot;bh&quot;</td>
<td>German &quot;w, z&quot; without dagesh.</td>
</tr>
<tr>
<td>&quot;zz&quot;</td>
<td>Arabic dental &quot;z&quot;.</td>
</tr>
<tr>
<td>&quot;z&quot;</td>
<td>&quot;Z, ژ, ژ&quot;.</td>
</tr>
<tr>
<td>&quot;zj&quot;</td>
<td>Polish &quot;ژ&quot;.</td>
</tr>
<tr>
<td>&quot;zh&quot;</td>
<td>Buzz of the hiss &quot;sh&quot;.</td>
</tr>
<tr>
<td>&quot;jh&quot;</td>
<td>German palatal &quot;g&quot; in &quot;teig.&quot;</td>
</tr>
<tr>
<td>&quot;gh&quot;</td>
<td>German guttural &quot;g&quot; in &quot;tag.&quot;</td>
</tr>
<tr>
<td>&quot;f&quot;</td>
<td>&quot;θ, θ&quot; without dagesh.</td>
</tr>
<tr>
<td>&quot;wh&quot;</td>
<td>Whispered &quot;w, English wh, Saxon hw.&quot;</td>
</tr>
<tr>
<td>&quot;l&quot;</td>
<td>I.e. in which the lower lip touches the upper teeth.</td>
</tr>
<tr>
<td>&quot;nh&quot;</td>
<td>&quot;θ, θ&quot; without dagesh.</td>
</tr>
<tr>
<td>&quot;v&quot;</td>
<td>&quot;English v&quot;.</td>
</tr>
<tr>
<td>&quot;dh&quot;</td>
<td>&quot;th&quot; in &quot;the,&quot; &quot;θ&quot; without dagesh.</td>
</tr>
<tr>
<td>&quot;zh&quot;</td>
<td>&quot;French j&quot;.</td>
</tr>
<tr>
<td>&quot;y&quot;</td>
<td>&quot;English y, German j, Hebrew י&quot;.</td>
</tr>
<tr>
<td>&quot;m&quot;</td>
<td>&quot;M, μ, מ&quot;.</td>
</tr>
<tr>
<td>&quot;n&quot;</td>
<td>Arabic dental &quot;n&quot;.</td>
</tr>
<tr>
<td>&quot;nj&quot;</td>
<td>&quot;Fr. and Ital. gn, Span. ɲ, Port. nh&quot;.</td>
</tr>
<tr>
<td>&quot;ng&quot;</td>
<td>Sanscrit cerebral &quot;n&quot;.</td>
</tr>
<tr>
<td>&quot;ngj&quot;</td>
<td>Back palatal &quot;n&quot;.</td>
</tr>
<tr>
<td>&quot;ng&quot;</td>
<td>&quot;ng&quot; in &quot;sing,&quot; &quot;γ&quot; before gutturals.</td>
</tr>
<tr>
<td>&quot;llh&quot;</td>
<td>Whispered Polish &quot;l&quot;.</td>
</tr>
<tr>
<td>&quot;lh&quot;</td>
<td>Whispered &quot;l, Welsh ll&quot;.</td>
</tr>
<tr>
<td>&quot;l&quot;</td>
<td>&quot;l, چ, چ&quot;.</td>
</tr>
<tr>
<td>&quot;lj&quot;</td>
<td>&quot;Ital. gli, Span. ʝ, Port. lh&quot;.</td>
</tr>
<tr>
<td>&quot;l&quot;</td>
<td>Sanscrit cerebral &quot;l&quot;.</td>
</tr>
<tr>
<td>&quot;rh&quot;</td>
<td>Whispered &quot;r, Welsh rh&quot;.</td>
</tr>
<tr>
<td>&quot;r&quot;</td>
<td>Sanscrit cerebral &quot;r&quot;.</td>
</tr>
<tr>
<td>&quot;brh&quot;</td>
<td>German lip-trill.</td>
</tr>
<tr>
<td>&quot;r&quot;</td>
<td>Initial &quot;R, r, ɾ&quot;.</td>
</tr>
<tr>
<td>&quot;v&quot;</td>
<td>Palatal &quot;r&quot;.</td>
</tr>
<tr>
<td>&quot;zr&quot;</td>
<td>Polish &quot;rz&quot;.</td>
</tr>
<tr>
<td>&quot;a&quot;</td>
<td>&quot;English r in &quot;fIr,&quot; &quot; her.&quot;</td>
</tr>
<tr>
<td>&quot;r&quot;</td>
<td>&quot;English r in &quot;fur,&quot; &quot;poor.&quot;</td>
</tr>
<tr>
<td>&quot;grh&quot;</td>
<td>Arabic &quot;گ&quot;.</td>
</tr>
</tbody>
</table>
Vowels.

There are three series of vowels; the upper, middle, and lower; the points representing which are placed respectively above, between, and below the two horizontal lines of the letter.

There are also three positions of vowels; labial and dental (which may be called front), palatal, and guttural. The points are placed respectively at the left hand, middle, and right of the letter.

In some cases there are three vowels of the same series in the same position. These are expressed respectively by in the appropriate place.

The number of vowels which may be expressed is thus twenty-seven; but there are only twenty distinct vowels recognized, of which ten occur in English. There are no front upper vowels.

Each of these twenty vowels may be long or short. The long vowels may be expressed by doubling, or thickening, the sign for the short vowel; or perhaps better by combining with it the mark ( ), giving or or or for the long sounds of ( ) ( ) ( ) respectively.

Scale of Vowels.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Candy's</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Physiol.</td>
<td>+</td>
<td>+</td>
<td></td>
</tr>
<tr>
<td>Alphabet.</td>
<td>+</td>
<td>+</td>
<td></td>
</tr>
<tr>
<td>Ellis's</td>
<td>i e</td>
<td>e e a e a</td>
<td>Upper.</td>
</tr>
<tr>
<td>Digraphic</td>
<td>uh ih</td>
<td>ok eo</td>
<td>Middle.</td>
</tr>
<tr>
<td>Alphabet.</td>
<td>u o</td>
<td>o o</td>
<td>Lower.</td>
</tr>
<tr>
<td>Candy's</td>
<td>&gt; &lt;</td>
<td>&gt; &lt;</td>
<td>Upper.</td>
</tr>
<tr>
<td>Physiol.</td>
<td></td>
<td></td>
<td>Middle.</td>
</tr>
<tr>
<td>Alphabet.</td>
<td>&gt; &lt;</td>
<td></td>
<td>Lower.</td>
</tr>
<tr>
<td>Ellis's</td>
<td>ii ee</td>
<td>e e a e a</td>
<td>Upper.</td>
</tr>
<tr>
<td>Digraphic</td>
<td>u uh iih eeh ooh eeo</td>
<td>oee  e e</td>
<td>Middle.</td>
</tr>
<tr>
<td>Alphabet.</td>
<td>u u o</td>
<td>e e</td>
<td>Lower.</td>
</tr>
</tbody>
</table>
On Dislocations of Rocks between Leven and Duddon Sands.

### Key to the Vowels

<table>
<thead>
<tr>
<th>Short</th>
<th>Phys. Digr.</th>
<th>Long</th>
</tr>
</thead>
<tbody>
<tr>
<td>a, Ital. and Germ. short a</td>
<td>aa, a in &quot;father.&quot;</td>
<td></td>
</tr>
<tr>
<td>ae, English short a</td>
<td>aae, Provincial English</td>
<td></td>
</tr>
<tr>
<td>eo, English short e</td>
<td>eeo, ee in &quot;bear.&quot;</td>
<td></td>
</tr>
<tr>
<td>e, French é</td>
<td>ee, English ay, French ée</td>
<td></td>
</tr>
<tr>
<td>io, English short i</td>
<td>iio, the long sound of i</td>
<td></td>
</tr>
<tr>
<td>i, French short i</td>
<td>ii, Engl. ee, Fr. i, Germ. ie</td>
<td></td>
</tr>
<tr>
<td>o, Engl. &amp; Dutch, Sanscr. a</td>
<td>oo, the long sound of o</td>
<td></td>
</tr>
<tr>
<td>oe, Fr. eu, Ger. oe or o short</td>
<td>ooe, Fr. eh, Germ. oe or o long</td>
<td></td>
</tr>
<tr>
<td>eo, French mute e</td>
<td>eeo, the long sound of e</td>
<td></td>
</tr>
<tr>
<td>oh, Gaelic ao in &quot;laogh.&quot;</td>
<td>ooh, the long sound of o</td>
<td></td>
</tr>
<tr>
<td>eh, Polish y short</td>
<td>eeh, the long sound of e</td>
<td></td>
</tr>
<tr>
<td>ih, Welsh y (tongue between teeth)</td>
<td>iih, Welsh y long</td>
<td></td>
</tr>
<tr>
<td>uh, Swedish u short</td>
<td>uuh, Swedish u long</td>
<td></td>
</tr>
<tr>
<td>ue, Sc. ui, Ger. ue or ü, Sw. y</td>
<td>uue, German ue or ü long</td>
<td></td>
</tr>
<tr>
<td>ao, French a short</td>
<td>ao, French d</td>
<td></td>
</tr>
<tr>
<td>ao, English o short</td>
<td>ao, English a in &quot;water.&quot;</td>
<td></td>
</tr>
<tr>
<td>o, Italian o aperto, short</td>
<td>oo, Ital. o aperto, long</td>
<td></td>
</tr>
<tr>
<td>o, English o in &quot;omit.&quot;</td>
<td>oo, Engl. long o, Gr. ω</td>
<td></td>
</tr>
<tr>
<td>uo, Ital. o chiuso, Sw. o short</td>
<td>uo, Ital. o chiuso, Sw. o long</td>
<td></td>
</tr>
<tr>
<td>u, Engl. oo short; Germ. &amp;c.</td>
<td>uu, Engl. oo long, Germ uh</td>
<td></td>
</tr>
</tbody>
</table>

Nov. 9.—Professor Sedgwick gave a description of a series of dislocations which have moved the Cambrian and Silurian rocks between Leven Sands and Duddon Sands, several miles out of their normal position in the Geological Map of the Lake Mountains.

To make the subsequent descriptions clear, the author first gave a normal or typical section of the older Palaeozoic rocks, by enumerating (in an ascending order) the great groups which have been well established, as follows:—1. Skiddaw Slate. 2. Chloritic slate, porphyry, trappean shales, &c. 3. Coniston Limestone, calcareous slate, Coniston Flagstone, &c. (All the above three groups are called Cambrian.) 4. Coniston Grit,—a good physical group, which however, from the paucity of its fossils, may be thought of ambiguous relations: it appears to represent the May Hill Sandstone, and there-
fore to be the base of those rocks which, in the north of England, represent the Wenlock and Ludlow groups of Silurian. 5. A coarse slate, often much contorted. 6. A bed of impure limestone, which may be traced from Tottlebank Fell towards the north-east for about two miles, after which it thins out and does not again appear further northward. 7. A coarse and often contorted state. When No. 6 is wanting, No. 5 and No. 7 may be considered as one group (now called Banisdale Slate), which is widely spread, and gradually passes into more coarse and gritty masses, that link themselves to the next superior group. 8. Grit, slate, and tilestone, expanded between Kendal and Kirkby Lonsdale. This is followed by unconformable masses of Old Red Sandstone and carboniferous limestone. Of the above groups, Nos. 5, 6, and 7 approximately represent the Wenlock series, and No. 8 abounds in characteristic Ludlow fossils.

In the range of these groups from Shap Fell to Tottlebank Fell (which is about two miles south-west of Coniston Waterfoot) there seems to be no ambiguity; but to the south of Tottlebank Fell the groups have, through the intervention of great faults, been thrown into such abnormal positions that their relations have often been misunderstood. Thus, (in 1822) when the author first attempted to map this part of Lancashire, he was led, by the line of strike as well as by the whole physical characters of the country, to identify the Tottlebank Limestone with a calcareous band a few miles further south, which ranges (on the east side of the Duddon estuary) from the hills above Bunk House, through Meer Beck, towards the village of Ireleth, and which from thence by an enormous fault (upcast towards the south) is thrown into the ridge of High Haume, near Dalton, from which it is continued nearly in the same strike till it is covered by the Old Red Sandstone and Mountain Limestone. This identification was however erroneous; for the limestone-beds above mentioned, ranging on the east side of Duddon Sands, are altogether unconnected with the Tottlebank Limestone, and are in mineral type and fossils absolutely identical with that portion of the Coniston Limestone which appears, as is well known, on the other side of Duddon Sands in the south-western extremity of Cumberland.

The above mistake (made in like manner by several subsequent observers) was partially corrected by the author in 1845; when, on good physical and fossil evidence, he placed the High Haume Limestone on the same parallel with that of Coniston. Not having any fossils from the limestone quarries north of the village of Ireleth, and not having found a single characteristic fossil from the slate-rocks between Ulverston Sands and Duddon Sands, he was in 1845 unable to carry his correction any farther; but even then he remarked again and again that the calcareous beds north of the village of Ireleth in structure resembled the Coniston Limestone, which is seen on the north-west side of the Duddon, much more nearly than they resembled the calcareous beds of Tottlebank.

In the autumn of 1856 the author, accompanied by his friends (Mr. Gough of Reston Hall, and Mr. John Ruthven of Kendal), saw for the first time the excellent local collection formed during the
labours of many past years by Mr. Bolton of Ulverston. It was evident almost at a glance that the fossils he had collected from the slate-rocks between Ulverston and Duddon Sands belonged to the upper part of the Coniston group (No. 3). He kindly pointed out to them some of his best localities, and they left the country convinced that nearly all the older rocks between Ulverston and the Duddon estuary belonged to the Coniston group, and consequently that these rocks were not superior, but inferior to the Coniston grits, though the prevailing dip and the geographical position of the groups might seem to indicate the very contrary.

In 1857 they again visited the district, re-examined Mr. Bolton's unrivalled local collection, and again during two days made traverses under his guidance. They then devoted a few days to the approximate determination of the vast breaks and faults, which have so much disturbed the normal position of the physical groups in a part of Furness, and made the colours of the Geological Map to appear almost incredibly anomalous.

The author then described, by help of plans and sections, the faults above alluded to. 1. Black Coomb, protruded as it is at the south-west end of Cumberland, seems to have been a kind of centre of disturbance. The chloritic slates and Coniston group which skirt the south-east side of Black Coomb have been ripped up by a north and south fault which at one cast throws the Coniston limestone about three miles to the south of its previous range.

2. Similar enormous up-casts towards the south-east cause the repetition of the Coniston limestone and flagstone on the other side of the Duddon Sands. This repetition is not produced by undulations, but by great up-cast faults.

3. A great east and west fault descends near the rivulet of Beck Side with a down-cast to the north, which brings the Ireleth slates down to the level of the sea at Sandside.

4. A complicated fault, or system of faults, with a very great up-cast to the south-east, runs from Kirkby Hall, skiriting the brow of the hill under the great Ireleth slate quarries. The slates of Ireleth cannot be separated from the Coniston flag. They do not overlie the Coniston grits (as the author and other observers had long supposed), but abut against them. This conclusion seems inevitable, though the sections are broken and difficult of interpretation.

5. By a complication of faults the Coniston grits are widely expanded in the hills immediately north of the great Ireleth slate quarries; but all the above mentioned groups are by an east and west fault (or series of faults), with an enormous up-cast towards the south, cut off from the normal groups (viz. Nos. 4, 5, 6, and 7 of the typical section), which range towards Coniston water and thence into Westmorland. This east and west fault runs down into the valley of the Crake, not far from Lowick Bridge.

6. Another great fault appears to descend from Coniston waterhead down the Crake, producing an up-cast on its south-east side. The facts given in the above abstract necessitate a partial change of nomenclature. The Ireleth slates can no longer be appealed to as groups superior to the Coniston grits and on the parallel of the
Wenlock shale. But in Banisdale there are old slate quarries in the group which, without sectional difficulty or ambiguity, does overlie the Coniston grits. For the future, therefore, the author proposes to use the term Banisdale slates for the lower part of the group which overlies the Coniston grits.

Having approximately laid down the faults above mentioned, there was still something wanting to complete the evidence; for the slate rocks between Duddon Sands and Leven Sands, in spite of their contortions, form an ascending section of great thickness. If, therefore, these slates be a repetition of the Coniston flags, the Coniston grits (typical section No. 4) might be looked for somewhere towards the south-east. To put this to the test, the author (accompanied by his two friends) made a complete traverse from Broughton to the upper part of Leven Sands; and they found, as they were finishing their traverse, that the ridges which skirt the estuary of the Leven, below Penny Bridge, were composed of the Coniston grits in their characteristic form. The evidence was then complete, and they next day left the country.

Also a paper was read by Professor De Morgan, "On the Beats of Imperfect Consonances."

This subject has been left in great obscurity by Dr. Smith, and subsequent writers have either neglected it, or misunderstood it, or obtained results by methods which miss the principal simplification of which the theory appears susceptible. Omitting historical matter, Mr. De Morgan’s method may be described as follows:—

The grave harmonic of Tartini, formed by sounding two notes of which the vibrations take \( n \) and \( m \) equal parts of time (\( m : n \) being in its lowest terms), has a vibration which lasts through \( mn \) of those times. This is called Tartini’s beat, whether it produce a sound, or whether it only produce what Dr. Smith calls a fluttering. This beat is most perfect when the consonance is in perfect tune. If the consonance be a little out of tune, Tartini’s beats are not destroyed, but do not succeed each other with perfect reiteration of circumstances, owing to the gradual advance or regression of the position in one vibration of the commencement of the other. A cycle of disturbances is the result, which cycle is repeated, or repeated quam proximé; and the ear recognizes this recurrence in Smith’s beats, which are entirely due to the imperfection of the consonance. The connexion has a close resemblance to that of the instantaneous ellipse of a planet and its disturbed orbit.

The simplest connexion of beats and vibrations is as follows:—

The smaller of the two numbers, \( n \) and \( m \), being \( n \), every vibration by which the upper note is tuned wrong gives \( n \) beats per second. Thus, the consonance being a fifth (\( 2 : 3 \)), every vibration by which the upper note is too flat or too sharp gives two beats per second.

In an appendix, Mr. De Morgan gives some tables of beats, repeats some theorems on temperament from the Penny Cyclopædia, and recommends and argues in favour of tuning being performed by a whole octave of tuning forks, adjusted by beats to the system employed.
XVII. Intelligence and Miscellaneous Articles.

ON THE FLUORESCENCE OF A SOLUTION OF FRAXINE WHICH APPEARS UNDER CERTAIN CIRCUMSTANCES OF A YELLOW COLOUR.

BY THE PRINCE SALM-HORSTMAR.

PROFESSOR STOKES has had the goodness to call my attention to the probability of the observation of a yellow fluorescence of a solution of fraxine, in a test-tube placed in a case of blue cobalt glass (see Poggendorff's Annalen, vol. xcvi. p. 638), being due to an optical illusion caused by the colour contrast. This suggestion induced me to examine the matter more closely.

If the experiment be so arranged that the test-tube containing the solution be placed in a narrow opake case, closed at the bottom, and having a single opening through which the daylight passes horizontally, the blue fluorescence appears precisely as in direct daylight. In a case of dark violet manganese glass, the solution also presents the same blue fluorescence as appears in direct daylight.

Hence it certainly follows that the yellow fluorescence mentioned above was an optical illusion, caused by the fact that the eye only recognized one part of the fluorescent light of the fraxine, that is, only the yellow rays, because the much stronger blue light of the glass case prevents the eye from recognizing distinctly the feeble blue rays of the total fluorescence. The yellow rays of the fluorescent light, although inferior in respect to quantity to the blue rays of the entire fluorescence, are yet distinctly perceived, because the eye is especially sensitive to them in the neighbourhood of the blue.

The yellow fluorescent appearance is accordingly a part of the total fluorescent light. Hence the appearance of the light in the cobalt-glass case remains yellow upon an almost black ground if a deep orange-coloured glass be held before the eye; and it appears to differ very little from the fluorescence of the uranium glass if the orange glass be changed for a pure yellow one.

The cobalt-glass case appears to be capable of application, not only for finding quickly, and without the use of a prism, the yellow constituent in a blue fluorescence, but also for judging to some extent of the relative quantities of the yellow constituent of the light in totally different solutions having blue fluorescence. This is effected by placing the several solutions side by side, ceteris paribus, in the case. It is especially advantageous to cover one of the glass sides of the case, that one namely which is turned towards the window, with a piece of black paper, the height of which is almost equal to the length of the test-tubes. The brilliancy of the yellow light is thereby increased.—Poggendorff's Annalen, for April, 1858.

NOTE ON INTERNAL CONICAL REFRACTION. BY DR. P. LECH.

A simple means of observing and measuring the internal conical refraction in even feebly double-refracting crystals is the following:—

A polished parallel plate of crystal, cut almost at right angles to an
optical axis, is covered on one side with tinfoil, and so placed under the microscope that the surface of the tinfoil is visible, seen through the crystal. If now the tinfoil be illuminated from above through the crystal by sun- or lamp-light, there are seen at a proper position of the crystal a quantity of delicate brilliant rings, which are the images of the reflecting parts of the tinfoil on the white light. The delicacy of the rings renders the measurement tolerably accurate. I found in this manner the width of the aperture of the cone,—

For Tartaric acid, $4^{°} 12'$; difference of the least and greatest observed values . . . . 34'

For Sugar, $0^{°} 55'$; " " " 4'
For Diopside, $0^{°} 54'$; " " " 3'
For Nitre, $0^{°} 45'$; " " " 2'
For Gypsum, $0^{°} 18'$; " " " 1$\frac{1}{2}$

For Arragonite I found $1^{°} 52'$ (according to Lloyd, $1^{°} 50'$; theory, $1^{°} 55'$). Ångström's coefficients of refraction give for gypsum, 17' 25"; for nitre, theory gives 46' 25".—Poggendorff's *Annalen*, vol. civ. p. 188.

---

**ON THE INFLUENCE OF PRESSURE ON CHEMICAL AFFINITY.**

**BY DR. LOTHAR MEYER.**

In the twelfth volume of Poggendorff's *Annalen*, there is a note by Babinet which contains the proposal to use, as a measure of chemical affinity, the pressure which a gas generated by chemical decomposition must attain in order that the decomposition may cease. The author states that for zinc and sulphuric acid the limit is reached when for $0^{°}$ C. the pressure of the liberated hydrogen amounts to thirteen atmospheres; at $25^{°}$ C., on the contrary, this pressure exceeds the height of thirty-three atmospheres.

Experiments which I have made in Prof. Werther's laboratory do not agree with these statements. With the most varied strengths of sulphuric acid, even in the presence of large quantities of different sulphates, and by the use of citric and acetic acids, the pressure of the hydrogen liberated by zinc far exceeds the limits given by Babinet. The reason of this appears to lie in the fact that Babinet used copper vessels, closed by a cock, while I used scaled glass tubes.

The decomposition appears, however, to attain a limit; at any rate the liquid, even with excess of zinc, has still a strong acid reaction after standing for months. But what the maximum of this pressure may be I have not been able to determine, inasmuch as the only tubes I could obtain which would stand the pressure were too narrow to allow a manometer to be introduced. The greatest pressure which I observed directly at the manometer was 66 atmospheres. The acid consisted of one volume $\text{SH}_2\text{O}_4$, and three volumes of $\text{H}_2\text{O}$; the temperature was $0^{°}$ C. The tube exploded shortly after observing this pressure.—Poggendorff's *Annalen*, vol. civ. p. 189.
XVIII. On Equally Attracting Bodies. By Dr. T. A. Hirst*.

II. On equally attracting Surfaces.

1. As in the former memoir†, we shall again find it convenient to employ polar coordinates, the attracted point being taken as pole, and to adopt the following as definitions:

Corresponding points of two or more surfaces are those which lie on the same radius vector.

Corresponding tangent planes touch their respective surfaces at corresponding points, and the normals at these points are corresponding normals.

If we allow ourselves, for the sake of brevity, to make use of the term vector-plane for any plane through the radius vector at any point, then the plane which contains both the radius vector and the normal may be called the normal vector-plane of the point under consideration, so that corresponding normal vector-planes will pass through the same radius vector and contain corresponding normals.

Lastly, any cone having its vertex at the attracted point or pole will be said to intercept upon two or more surfaces corresponding portions or elements according as the dimensions of the cone are finite or infinitesimal. Corresponding elements produced indefinitely in every direction constitute, of course, corresponding tangent planes.

2. In each point of our surfaces we shall conceive matter of a certain density to be concentrated, and this matter will be regarded as capable of acting at a distance—of attracting along the radius vector—according to the law of inverse squares. Amongst the infinite

* Communicated by the Author.
† See Phil. Mag., 4. vol. xiii. p. 305.

number of surfaces, however, capable of attracting the pole with the same intensity along the same line, we shall consider, solely, the group whose corresponding elements attract equally. The expression 'equally attracting surfaces' being interpreted in this very restricted manner, it will be seen that the problem before us reduces itself to a purely geometrical one, which, apart from the theory of attraction, is not without interest.

3. Let $d\sigma$ be the measure of the solid angle of a very small cone having its vertex at the pole, in other words, let $d\sigma$ be the element which such a cone intercepts upon a sphere, with radius unity, described about the pole as centre. If $ds$ be the element of an attracting surface intercepted by the same cone, $r$ the distance of the element from the pole, and $\psi$ the acute angle between the radius vector and the normal to this element, then

$$ds \cdot \cos \psi = r^2 d\sigma,$$

so that the attraction upon the pole, due to the matter spread over the element $ds$ with the density $\varrho$, will be proportional to

$$\frac{\varrho ds}{r^2} = \frac{\varrho d\sigma}{\cos \psi}, \quad \ldots \quad (1)$$

that is to say, the attraction of an element at any point of a given surface is dependent solely upon the ratio between the density at that point, and the cosine of the acute angle between the normal and radius vector.

From this we easily conclude, that when the densities at corresponding points are equal, the pole is attracted with the same intensity along the same line by all surfaces whose corresponding elements (or tangent planes) are equally inclined to their common radius vector, or, in other words, by all surfaces which cut at the same angle each ray of a pencil whose centre is the attracted point.

Of course the resultant attractions of two such equally attracting surfaces will have like or opposite directions according as their corresponding points are situated on the same or on opposite sides of the attracted point. Our solutions will always include both cases; but since no difficulty will be encountered in distinguishing between the two, we shall, for simplicity, keep the former—where the surfaces attract in precisely the same manner—more especially in view.

The equation (1) also shows that the pole is attracted with the same intensity along the same line by all surfaces whose densities at corresponding points are proportional to the cosines of the acute angles between the common radius vector and the normals at those points.

The ratio between the density and the cosine of the acute angle between the radius vector and normal, which according to
this theorem must be the same for all surfaces and a determinate
direction of the radius vector, will, in general, vary with this
direction; in other words, it will be a function of the two
angles $\theta, \phi$ which determine the same. The theorem leads im-
mediately to the solution of the following problem:—To deter-
dine the density on a given surface in order that any portion of
the same may attract the pole in the same manner as the corre-
sponding portion of any other surface with respect to which the
density is known. In the present memoir, however, instead of
entering into the details of this problem, we shall occupy our-
selves exclusively with the consequences of the first theorem.

4. A few of the simplest of these consequences may be at once
deduced. For example, each ray of a pencil is cut twice, at the
same angle, by a sphere; so that the centre of the pencil, if it
be without the sphere, will be attracted in the same manner by
corresponding portions of the convex and concave surfaces; and
if the centre of the pencil be within the sphere, it will be kept in
equilibrium by the attractions of corresponding portions, pro-
vided that, in both cases, the densities are the same at corre-
sponding points. The well-known theorem given by Newton in
the 70th Prop. of the first book of his 'Principia,' is a particular
case of the above.

5. Surfaces whose like-directed radii vectores are proportional
to each other, or, as we shall call them, similar surfaces, also at-
tract the pole in the same manner when the densities at corre-
sponding points are equal. This is at once evident from the
equation (1), which shows that the attraction of an element inter-
cepted by a given cone is independent of its distance from the
pole. If

$$ r = cf(\theta, \phi) $$

be the equation of any surface, those of all surfaces similar to it
will be found by giving to the constant $c$ all possible values.

6. It can easily be shown, too, that, under the same hypo-
thesis with respect to the densities at corresponding points,
inverse surfaces, or those whose like-directed radii vectores are
inversely proportional to each other, also attract the pole equally.
Although the truth of this assertion will be at once manifest
from the general equation of equally attracting surfaces, to be
hereafter given, it may be well to give at once a simple and
direct demonstration of the same. Let $m$ and $m_1$ be two conse-
cutive points on a given surface, $m'$ and $m'_1$ their corresponding
points on the inverse surface, and $O$ the attracted point or pole;
then, by hypothesis,

$$\overline{Om} \cdot \overline{Om'} = \overline{Om}_1 \cdot \overline{Om'}_1 = \text{const.},$$

so that the triangles $mOm_1$, $m'Om'_1$, which have in common an infinitesimal angle at $O$, are similar, and the angles $m,mO$ and $m'm'O$ equal. Ultimately, therefore, the angles $m,mO$ and $m',m'O$ will be supplemental, in other words, the corresponding linear elements $mm_1$, $m'm'_1$ are equally inclined to their common radius vector $O\overline{m}$. Since the same is true for all other corresponding linear elements around $m$ and $m'$, it follows at once that the plane of all the former, or the superficial element at $m$, is inclined towards the radius vector at the same angle as is the plane of all the latter, and consequently that the corresponding superficial elements attract $O$ equally.

7. Inasmuch as inverse surfaces will simultaneously present themselves in all our subsequent solutions, it will be worth while to state briefly some of their principal properties.

Around a fixed point $O$ (the centre of inversion), and with any radius $c$, conceive a sphere to be described; then any two points $m$ and $m'$, situated on the same line through $O$, and on the same side of the latter point, are said to be inverse to each other when

$$\overline{Om} \cdot \overline{Om'} = c^2.$$

This being premised, inverse figures or systems of inverse points possess, amongst others, the following properties:—

I. Any two lines whatever (right or curved) cut each other at the same angle as do their inverse lines.

For if $mm_1$, $mm_2$ be the elements of one pair of lines at their point of intersection $m$, and $m'm'_1$, $m'm'_2$ the corresponding elements of the inverse lines, it is evident from art. 6 that the pair of corresponding elements $mm_1$, $m'm'_1$, if produced, would meet in a point $\mu_1$ and form an isosceles triangle on $mm'$ as base. Similarly, $mm_2$ and $m'm'_2$, would if produced, meet in a point $\mu_2$ and form another isosceles triangle on the same base. This being the case, it is easily seen that the two triangles $\mu_1m\mu_2$, $\mu_1m'\mu_2$ have all their sides equal, whence results the equality of the angles $\mu_1m\mu_2$, $\mu_1m'\mu_2$, as asserted in the theorem.

Amongst other consequences of I. it is evident that the inverse of the normal to a surface cuts the inverse surface perpendicularly.

II. The inverse of a plane is a sphere through the centre of inversion, whose centre is on the perpendicular let fall from the centre of inversion upon the plane.

For if $p$ be the foot of this perpendicular and $p'$ its inverse, $m$ any point of the plane and $m'$ its inverse, we conclude, as in art. 6, that the triangles $mpO$ and $p'm'O$ are similar. The angle
\( \rho \rho' \theta \), therefore, is a right angle, and the locus of \( \nu \) a sphere on \( O \rho' \) as diameter.

As the plane recedes from or approaches \( O \), the diameter of its inverse sphere diminishes or increases, so that the centre of inversion itself must be regarded as the inverse of the infinitely distant plane, and a plane through the point of inversion as coinciding with its own inverse. The converse of the present theorem needs no demonstration; from the two we conclude, too, the following and its converse:

III. The inverse of a line is a circle, passing through the centre of inversion, whose plane coincides with that determined by this point and the line, and whose centre is upon the perpendicular let fall from the former upon the latter.

IV. The inverse of a sphere is also a sphere.

Let \( S \) be the centre and \( s \) the radius of the given sphere; then if \( S' \) be the inverse of \( O \) with respect to the given sphere \( (S) \), regarded as a sphere of inversion, we shall have

\[
S'O : SS' = s^2 = \text{const.,}
\]

and every circle described through \( O \) and \( S' \) will cut the given sphere perpendicularly. Now, by III., the inverses of all such circles constitute a pencil of rays through the point \( S' \), the inverse of \( S \), and from I. it follows that every such ray cuts the inverse of the given sphere perpendicularly, so that this inverse must be itself a sphere around \( S' \) as centre. The centres of the two spheres are corresponding, but not inverse points, for they are, clearly, connected by the relation

\[
\frac{OS'}{OS} = \frac{c^2}{OS^2 - s^2} \cdot OS,
\]

so that the centres \( S \) and \( S' \) are on the same side or on opposite sides of \( O \) according as the latter point is without or within either sphere; in all cases, however, \( O \) is a centre of similitude of the inverse spheres. When either cuts the sphere of inversion orthogonally it coincides with its own inverse, for then \( c^2 = OS^2 - s^2 \). When the given sphere \( (S) \) passes through \( O \), the latter point and \( S \) coincide, so that \( S' \) recedes to infinity, the rays through the same become parallel to \( OS \), and the sphere \( (S') \) cutting these rays perpendicularly degenerates into a plane perpendicular to \( OS \), in accordance with the converse of II.

V. The inverse of a circle is also a circle.

For the first circle may be regarded as the intersection of two spheres whose inverses, by IV., being also spheres, must intersect in a second circle, the inverse of the former. It is evident that the two circles lie on a sphere which cuts the sphere of inversion orthogonally, and that they are at the same time anti-parallel
sections of a cone whose vertex is the centre of inversion. When the plane of one circle passes through this centre of inversion \( O \), it coincides, of course, with the plane of the other circle, and when one circle passes through \( O \), its inverse is a line in accordance with the converse of \( \text{III} \).

VI. The inverse of a surface of the \( n \)th order is, in general, a surface of the \( 2n \)th order.

For in general every circle through the point of inversion will cut the given surface in \( 2n \) points, and hence by \( \text{III} \). every right line will cut the inverse surface in \( 2n \) points. Similarly:

VII. The inverse of any plane curve of the \( n \)th order, the centre of inversion being in the same plane, is a curve of the \( 2n \)th order.

Since the infinitely distant line cuts the original curve in \( n \) real or imaginary points, it is evident from \( \text{III} \). that the inverse curve will have a multiple point (real or imaginary) of the \( n \)th order in the centre of inversion, and besides this every multiple point and cusp of the original curve will have its corresponding one of the same order in the inverse curve, and so on.

VIII. The inverse of any surface is at the same time the locus of the foot of the perpendicular let fall from the centre of inversion upon a tangent plane to the reciprocal polar of the given surface with respect to the sphere of inversion.

For, as is well known, the polar plane of any point cuts the radius vector perpendicularly in the inverse of that point.

IX. The inverse of a line of curvature on a given surface is also a line of curvature on the inverse surface.

For the normals in two consecutive points \( m \) and \( m_1 \) of a line of curvature on the given surface meet in a point \( \mu \), so that if, in the planes \( O\mu m \), \( O\mu m_1 \), two circles be drawn so as to pass through \( O \) and touch the normals, respectively, in \( m \) and \( m_1 \), these circles will not only cut the surface perpendicularly, but they will intersect again in a point \( \mu_1 \), of the line \( O\mu \), such that
\[
\mu O \cdot \mu_1 = \mu m^2 = \mu m_1^2.
\]

But by \( \text{III} \). the right lines, inverse to these two circles, are the normals to the inverse surface in the consecutive points \( m' \), \( m'_1 \) corresponding to \( m, m_1 \), and since these normals meet in a point \( \mu' \), inverse to \( \mu_1 \), \( m' m'_1 \) must be an element of a line of curvature of the inverse surface.

From the above, we conclude, too, that the principal centres of curvature \( \mu \) and \( \mu'_1 \), at corresponding points of inverse surfaces, correspond, but not as inverse points; they are, in fact, connected by the relation
\[
O\mu'_1 = \frac{c^2}{O\mu^2 - \mu m^2} \cdot O\mu.
\]
Analogous theorems with respect to inverse curves, of single and double curvature, might easily be established; to do so, however, would be to extend too much the present digression. We return, therefore, to the subject under consideration*.

8. A sphere around the attracted point as centre is but an individual of a large class of surfaces which possess peculiar attracting properties. Any surface of this class may be defined as cutting every ray of the pencil whose centre is the attracted point at the same angle $\beta$. The densities at corresponding points being everywhere assumed to be the same, it is evident that the class referred to includes innumerable groups of equally attracting surfaces, each group corresponding to a particular value of $\beta$. The sphere belongs to the group determined by the value $\beta = \frac{\pi}{2}$. The attractions of corresponding portions of two surfaces belonging to different groups ($\beta$) and ($\beta_1$) are like-directed and have a constant ratio to each other, that is to say, a ratio which is independent of the magnitude of the attracting portions. This is at once evident from the equation (1), which shows that the attractions of corresponding elements are inversely proportional to the sines of the constant angles $\beta$ and $\beta_1$. As a particular case, therefore, it follows that the attraction of a portion of a surface of the group ($\beta$) is to that of the corresponding portion of the sphere as $1 : \sin \beta$. Hence, as far as attraction is concerned, surfaces of this class enjoy all the symmetrical properties of spheres. For example, if the density on such a surface be everywhere the same, any portion of the surface intercepted by a cone, having an axis of symmetry and its vertex at the attracted point, will attract this point along that axis, and so on.

The inverse surfaces of those here considered clearly belong to the same class and group; the general equation of the class will be given in the sequel; in the mean time, however, it is easy to find individuals; such, for instance, would be the surface generated by the rotation around any radius vector of a plane logarithmic spiral, having its pole at the attracted point. Or, more

* The subject of inverse figures appears to have been first introduced under the name of "electrical images" by Prof. W. Thomson in the Cambridge and Dublin Mathematical Journal (vols. viii. and ix.); and subsequently, under the title "principe des rayons vecteurs réciproques," it received considerable development from M. Liouville. It was not until the present memoir had been written that I discovered how closely the above art. 7 coincides with a portion of the excellent memoir of M. Liouville (Journal de Math. vol. xii.). Notwithstanding this coincidence, however, I have allowed the article in question to stand, as well in order to avoid the inconvenience of reference, as on account of the demonstrations there briefly indicated, which differ somewhat from those given by M. Liouville.
generally, if the same spiral were to move so that its plane constantly touched, without sliding over the surface of any cone whatever, having its vertex at the attracted point, it would generate a surface of the class under consideration.

9. As a last conclusion, immediately deducible from equation \((1)\), we may cite the following:

The densities at corresponding points being the same, the resultant attraction upon the centre of a sphere of any quadrant on its surface (or of any portion of such a triangle) is less than that of the corresponding portion of any other surface whatever.

The demonstration presents no difficulties.

10. In order to arrive at the analytical expression of the first theorem of art. 3, we propose next to determine, directly, the requisite formulae in polar coordinates.

Let \(\triangle ABC\), \(\triangle AB'C'\) be two great circles of a sphere whose planes are inclined to each other at the acute angle \(\hat{A}\). Further,

\[
\tan \alpha = \tan \hat{A} \cdot \sin c, \\
\tan \alpha' = \tan \hat{A} \cdot \sin c',
\]

where \(a\) and \(c\) are the sides opposite to the angles \(A\) and \(C\) in the triangle \(\triangle ABC\), and \(a', c'\) the sides respectively opposite to the angles \(A, C'\) in \(\triangle AB'C'\). But since the sum of, or difference between \(c\) and \(c'\) is a right angle, we have, on squaring and adding these equations,

\[
\tan^2 a + \tan^2 c' = \tan^2 \hat{A}. \quad \quad (2)
\]

Again, if through \(B\) and \(B'\) we draw two arcs of great circles, \(BD = a\) and \(B'D' = a'\), perpendicular to \(ACC'\), we have, by well-known formulæ in spherical trigonometry,

\[
\sin \alpha = \sin a \cdot \sin \hat{C}, \\
\cos \hat{A} = \cos a \cdot \sin \hat{C}.
\]

Eliminating \(\sin \hat{C}\) from these equations, and performing the
same operations in the triangles $AB'C'$ and $B'C'D'$, we arrive at the simple formulae,
\[
\sin \alpha = \cos \hat{A} \cdot \tan \alpha, \quad \sin \alpha' = \cos \hat{A} \cdot \tan \alpha'. \tag{3}
\]

From the centre $M$ of the sphere, let the normals $MO, MN$ to the planes of $AB'B', ACC'$ be so drawn that the angle $OMN$ may be acute, and consequently equal to $\hat{A}$. Let us further agree that the acute angles $\alpha, \alpha'$—and hence also $\alpha, \alpha'$—shall be estimated as positive or negative magnitudes according as the corresponding arcs fall on the opposite or on the same side of the plane $AB'B'$ as does its normal $MO$. Under these conditions it is evident that the direction angles of the normal $MN$, with respect to the rectangular axes $MO, MB, MB'$, will be respectively equal to

\[
\hat{A}, \quad \frac{\pi}{2} - \alpha, \quad \frac{\pi}{2} - \alpha';
\]

so that by the equations (3), the direction cosines will have the respective values,

\[
\cos \hat{A}, \quad \cos \hat{A} \cdot \tan \alpha, \quad \cos \hat{A} \cdot \tan \alpha'. \tag{4}
\]

11. Conceive $M$ to be a point on any surface, $MO$ the radius vector making an angle $\theta$ with the polar or $x$-axis, $\phi$ the angle between the plane $(xy)$ and the vector-plane through the $x$-axis. Further, let $MB$ and $MB'$ be drawn perpendicular to the radius vector, the first in the above vector-plane, the second perpendicular thereto, and both in the direction of the increasing angles $\theta$ and $\phi$. If, lastly, we identify the tangent-plane at $M$ with that of our former great circle $ACC'$, it will be at once seen that, in virtue of the agreement respecting the signs of $\alpha$ and $\alpha'$,

\[
\tan \alpha = \frac{1}{r} \frac{dr}{d\theta},
\]

and

\[
\tan \alpha' = \frac{1}{r \sin \theta} \frac{dr}{d\phi};
\]

or, if we set

\[
\log \frac{r}{c} = u, \quad \ldots \ldots \ldots \quad \tag{5}
\]

where $c$ is an arbitrary constant or line,

\[
\tan \alpha = \frac{du}{d\theta}, \quad \tan \alpha' = \frac{1}{\sin \theta} \frac{du}{d\phi}.
\]
Dr. Hirst on Equally Attracting Bodies.

Substituting these values in equation (2), we find at once the following analytical expression, in polar coordinates, for the tangent of the acute angle, \( \hat{A} = \psi \), between the radius vector and normal at the point \( M \),

\[
\left( \frac{du}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{du}{d\phi} \right)^2 = \tan^2 \psi. \quad \ldots \quad (6)
\]

From this expression we deduce, too,

\[
\cos \psi = \frac{1}{\sqrt{1 + \left( \frac{du}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{du}{d\phi} \right)^2}}.
\]

Again, by equation (4), the direction cosines of the normal with respect to the rectangular axes \( MO, MB, MB' \) are, respectively,

\[
\cos \psi, \quad \frac{du}{d\theta} \cos \psi, \quad \frac{du}{d\phi} \sec \psi.
\]

If \( M_1 \) be the corresponding point to \( M \) of any other surface \( (u_1) \), the direction cosines of the corresponding normal \( M_1N_1 \), with reference to the rectangular axes \( M_1O, M_1B_1, M_1B'_1 \) respectively parallel to \( MO, MB, MB' \), will be

\[
\cos \psi_1 \quad \frac{du_1}{d\theta} \cos \psi_1, \quad \frac{du_1}{d\phi} \cos \psi_1,
\]

where \( \psi_1 \) is the acute angle between the normal \( M_1N_1 \) and the same radius vector \( MO \). Consequently, if we represent by \( \hat{N}_1 \) the inclination of the corresponding normals to each other, or, more strictly, the angle enclosed by two intersecting lines parallel to these normals, we have

\[
\cos \hat{N}_1 = \cos \psi \cdot \cos \psi_1 \left\{ 1 + \frac{du}{d\theta} \cdot \frac{du_1}{d\theta} + \frac{1}{\sin^2 \theta} \cdot \frac{du}{d\phi} \cdot \frac{du_1}{d\phi} \right\}. \quad (7)
\]

Again, by the fundamental formula of spherical trigonometry,

\[
\frac{\cos \hat{N}_1 - \cos \psi \cdot \cos \psi_1}{\sin \psi \cdot \sin \psi_1}
\]

is the general expression for the cosine of the angle \( \nu \) between the corresponding normal vector-planes; the equivalent expression in polar coordinates therefore is, by equation (7),

\[
\cos \nu = \cot \psi \cdot \cot \psi_1 \left\{ \frac{du}{d\theta} \cdot \frac{du_1}{d\theta} + \frac{1}{\sin^2 \theta} \cdot \frac{du}{d\phi} \cdot \frac{du_1}{d\phi} \right\},
\]

or more fully by equation (6),

\[
\cos \nu = \sqrt{\left( \frac{du}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{du}{d\phi} \right)^2} \sqrt{\left( \frac{du_1}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{du_1}{d\phi} \right)^2}. \quad (8)
\]
From this expression we deduce at once the conditions of coincidence and orthogonality of the corresponding normal vector-planes. They will coincide when $v = 0$ or $=\pi$, i.e. when

$$\frac{du}{d\theta} \frac{du_1}{d\theta} - \frac{du}{d\phi} \frac{du_1}{d\phi} = 0,$$

or

$$\frac{du}{d\theta} \frac{du_1}{d\theta} = \frac{du}{d\phi} \frac{du_1}{d\phi}, \quad \ldots \ldots \quad (9)$$

and they will be perpendicular to each other when $v = \frac{\pi}{2}$, i.e. when

$$\frac{du}{d\theta} \frac{du_1}{d\theta} + \frac{1}{\sin^2 \theta} \frac{du}{d\phi} \frac{du_1}{d\phi} = 0. \quad \ldots \ldots \quad (10)$$

It will also be remarked that this equation (10), identically fulfilled, expresses the condition of orthogonality between two systems of cones having the origin for common vertex, and the equations $u = c, u_1 = c_1$; where $c$ and $c_1$ are variable parameters.

12. Having thus established the necessary analytical formulæ, we proceed next to examine the mutual properties of any two equally attracting surfaces $(u)$ and $(u_1)$. According to the theorem of art. 3 and the equation (6), the necessary and sufficient condition to be fulfilled, identically, by two such surfaces is

$$\left(\frac{du}{d\theta}\right)^2 + \frac{1}{\sin^2 \theta} \left(\frac{du}{d\phi}\right)^2 = \left(\frac{du_1}{d\theta}\right)^2 + \frac{1}{\sin^2 \theta} \left(\frac{du_1}{d\phi}\right)^2. \quad \ldots \ldots \quad (11)$$

As remarked in art. 6, a glance at this equation is sufficient to show that it is satisfied when $u = \pm u_1$, or $\log \frac{r}{c} = \pm \log \frac{r_1}{c_1}$, that is, when

$$\frac{r}{c} = \frac{r_1}{c_1}, \text{ and when } \frac{r}{c} = \frac{c_1}{r_1};$$

in the first case the surfaces are similar, in the second inverse to each other; and in both cases, as may be seen from equation (9), all corresponding normal vector-planes coincide. It is also manifest, conversely, that this coincidence only exists when the equally attracting surfaces are similar or inverse to each other, for the equations (9) and (11) can only be satisfied, simultaneously, when $u = \pm u_1$.

13. The inquiry here naturally presents itself—under what conditions will two equally attracting surfaces have all their corresponding normal vector-planes perpendicular to each other? In this case it is the equations (10) and (11) which, by hypothesis, are simultaneously and identically fulfilled. By means of the substitution

$$\log (\tan \frac{1}{2} \theta) = \omega,$$
which we shall often find convenient on account of the greater symmetry thereby introduced, these equations (10) and (11) assume, respectively, the forms

$$\frac{du}{d\omega} \cdot \frac{du_1}{d\omega} + \frac{du}{d\phi} \cdot \frac{du_1}{d\phi} = 0, \quad \ldots \ldots \quad (10a)$$

$$\left(\frac{du}{d\omega}\right)^2 + \left(\frac{du}{d\phi}\right)^2 = \left(\frac{du_1}{d\omega}\right)^2 + \left(\frac{du_1}{d\phi}\right)^2 \quad \ldots \ldots \quad (11a)$$

from which we easily deduce

$$\left\{ \begin{array}{c}
\frac{du_1}{d\omega} = \pm \frac{du}{d\phi} \\
\frac{du_1}{d\phi} = \mp \frac{du}{d\omega}
\end{array} \right\} \quad \ldots \ldots \quad (12)$$

where the upper and lower signs respectively correspond. On differentiating the first of these expressions according to $\phi$, and the second according to $\omega$, we ought of course to obtain identical results; whence we conclude that, in order that the two surfaces may possess the properties required, each must fulfi!

the equation

$$\frac{d^2u}{d\omega^2} + \frac{d^2u}{d\phi^2} = 0. \quad \ldots \ldots \quad (13)$$

It is scarcely necessary to remark, that, in virtue of the last article, if $u$ and $u_1$ possess the properties under consideration, so also will their inverse surfaces $u' = -u$ and $u'_1 = -u_1$; so that if two of the four equally attracting surfaces $u$, $u'$, $u_1$, $u'_1$ have their corresponding normal vector-planes perpendicular to each other, there will always be four distinct pairs which possess the same property. On this account we need only consider one pair of such surfaces—say that which corresponds to the lower signs in (12).

14. Conversely, if any surface ($u$) fulfils the equation (13), a second, equally attracting surface ($u_1$) may always be found whose corresponding normal vector-planes are perpendicular to those of the former. For the hypotheses (12) are admissible when (13) is fulfilled; and $u_1$ being determined accordingly, the equations (10a) and (11a), which are perfectly represented by the system (12), will of course be satisfied.

15. The equation (13) plays an important part in physics; its general integral is well known to be

$$2u = F(\omega + i\phi) + F_1(\omega - i\phi),$$

where $F$ and $F_1$ are symbols of arbitrary, real or imaginary functions, and, as usual, $i = \sqrt{-1}$. By means of this and the equations (12), we find without difficulty

$$2iu_1 = F'(\omega + i\phi) - F_1'(\omega - i\phi),$$
and hence

\[ u + iu_1 = F(\omega + i\phi) \]
\[ u - iu_1 = F_1(\omega - i\phi). \]

(14)

In our case, therefore, where \( u \) and \( u_1 \) necessarily represent real functions, \( F \) and \( F_1 \) instead of being perfectly arbitrary are so related that one becomes identical with the other on changing \( i \) into \(-i\); in other words, in order that the surfaces represented by the functions \( u \) and \( u_1 \) of \( \omega \) and \( \phi \) may be equally attracting, and at the same time have their corresponding normal vector-planes perpendicular to each other, it is necessary and sufficient that one should be the real part of, and the other the coefficient of \( i \) in some function \( F(\omega + i\phi) \).

16. The simplest example of such a pair of surfaces is

\[ u = m\omega = \log \left( \tan^m \frac{\theta}{2} \right), \]
\[ u_1 = m\phi, \]

or

\[ r = e \cdot \tan^m \frac{\theta}{2}, \]
\[ r_1 = c_1 e^{m\phi}. \]

The former of these is the only surface of revolution which is compatible with the conditions under consideration; for in the case of \( u \) being a function of \( \theta \) alone, the equation (13) reduces itself to

\[ \frac{d^2u}{d\omega^2} = 0, \]

whose integral is, clearly,

\[ u = m\omega + A, \]

where \( m \) and \( A \) are arbitrary constants. The surface \( (r_1) \) would be generated by a circle of variable magnitude, moving so that its plane always passes through the \( x \)-axis, its centre always coincides with the origin, and its radius with a radius vector of a logarithmic spiral traced in the plane \((yz)\) with the origin for pole.

17. We may always decide at once whether, amongst the group of surfaces which attract in the same manner as any given one, there exist conjugate pairs whose corresponding normal vector-planes are perpendicular to each other, and in the case of their existence we can easily detect them. In fact, by the fundamental theorem of art. 3, the acute angle \( \psi \) between the normal and radius vector, regarded as a given function of \( \theta \) (or \( \omega \)) and \( \phi \), determines the group in question, inasmuch as it is identically the same for every surface which this group includes. Let us suppose

\[ \tan \psi = \frac{t}{\sin \theta}. \]
where \( t \) is a known function of \( \omega \) and \( \phi \). The equation (6), art. 11, will then resolve itself into

\[
\left(\frac{du}{d\omega}\right)^2 + \left(\frac{du}{d\phi}\right)^2 = t^2;
\]

and assuming the existence, in the group determined by \( t \), of pairs of surfaces of the required kind, the function \( u \) which represents one of such a pair will have the form

\[
2u = F(\omega + i\phi) + F_1(\omega - i\phi).
\]

Differentiating, we find

\[
2\frac{du}{d\omega} = F'(\omega + i\phi) + F'_1(\omega - i\phi),
\]

\[
2\frac{du}{d\phi} = iF'(\omega + i\phi) - iF'_1(\omega - i\phi);
\]

hence, squaring and adding,

\[
t^2 = F'(\omega + i\phi) \cdot F'_1(\omega - i\phi), \ldots \ldots (15)
\]

or

\[
2\tau = \log t^2 = \log F'(\omega + i\phi) + \log F'_1(\omega - i\phi);
\]

that is to say, if pairs of surfaces of the required kind exist, \( \tau = \log t \) must have the same form as \( u \), and consequently fulfil the condition

\[
\frac{d^2\tau}{d\omega^2} + \frac{d^2\tau}{d\phi^2} = 0.
\]

18. When the given function \( \tau \) satisfies this condition, the pairs of equally attracting surfaces in the group \( (\psi) \), which have their corresponding normal vector-planes perpendicular to each other, may usually be determined with great facility. To do so it is only necessary to determine \( F' \) and \( F'_1 \)—and hence \( F \) and \( F_1 \)—from the identical equation (15). If in the latter we set \( \omega = \phi = 0 \), and denote the result of this substitution in \( t \) by \( t_0 \), we have

\[
t_0^2 = F'(0) \cdot F'_1(0),
\]

which relation, since \( F' \) and \( F'_1 \) differ from each other only in the sign of \( i \), will be fulfilled in the most general manner by setting

\[
F'(0) = t_0e^{i\alpha}, \quad F'_1(0) = t_0e^{-i\alpha},
\]

where \( \alpha \) is a real arbitrary constant. Again, setting \( \omega = i\phi = \frac{\xi}{2} \), and denoting by \( t_1 \) the result of this substitution in \( t \),

\[
t_1^2 = F'(\xi) \cdot F'_1(0) = t_0e^{-i\alpha} \cdot F'(\xi),
\]

and

\[
F(\xi) = \frac{e^{i\alpha}}{t_0} \int t_1^2 d\xi + \text{const}.
\]

By changing \( i \) into \(-i\) we at once obtain \( F_1(\xi) \), and thus solve
the problem. The case where \( t_0 \) or \( t_1 \) vanishes, here excluded, requires a different, but equally simple mode of treatment.

19. As an example, let us examine a class of surfaces with which we shall afterwards be occupied,—the class for which \( \Psi \) is a function of \( \theta \) alone. This class contains as many groups of surfaces as the function \( \Psi \) of \( \theta \) is susceptible of different forms, and each group includes as many sub-groups of equally attracting surfaces as there are different varieties of \( \Psi \) under the same general form. The class clearly includes, too, all surfaces of revolution around the \( x \)-axis; in fact the general characteristic property of all surfaces of this class is that the attraction of a portion of any one upon the pole is the same as that of the corresponding portion of the same surface after it has been caused to rotate through any angle around the \( x \)-axis.

With such properties, however, we shall afterwards be more concerned; our immediate object is to determine whether, in the class in question, there are one or more groups of surfaces which contain pairs of the kind considered in the preceding articles, and if so, to determine such groups and such pairs of surfaces. By hypothesis, \( t \) and \( \tau \) are functions of \( \omega \) alone, so that the condition which, by art. 17, \( \tau \) must fulfil, reduces itself to

\[
\frac{d^2 \tau}{d\omega^2} = 0;
\]

whence, as in art. 16, we conclude that \( \tau \) must have the form

\[ \tau = m \omega + \text{const.}, \]

and

\[ t = e^\tau = mA e^{m\omega} = mA \tan^m \frac{\theta}{\omega}; \]

that is (see art. 17), in the class in question the only groups of the required kind correspond to a value of \( \Psi \) of the form

\[ \tan^m \frac{\theta}{\omega} = \frac{\tan m \theta}{\sin \theta}, \ldots \]

where \( m \) and \( A \) are arbitrary constants.

Further,

\[ t_0 = mA \text{ and } \int t^2 d\xi = mA^2 e^{m \xi}; \]

hence

\[ F(\xi) = Ae^{m \xi+i\alpha}, \text{ and } F_1(\xi) = Ae^{m \xi-i\alpha}; \]

so that by art. 15,

\[ u = -u' = \Lambda \tan^m \frac{\theta}{\omega} \cos (m\phi + \alpha), \]

\[ u_1 = -u'_1 = \Lambda \tan^m \frac{\theta}{\omega} \sin (m\phi + \alpha), \]

from which we deduce without difficulty the four pairs of surfaces of the required kind which each group, determined by the
176 Dr. Hirst on Equally Attracting Bodies.

value of \( \psi \) in (16), contains. The arbitrary constant \( a \) indicates, as above remarked, that a surface may be turned in any manner about the \( x \)-axis without ceasing to belong to the same group of equally attracting surfaces.

20. Between every pair of equally attracting surfaces, however, a general relation exists which leads easily to the determination of as many such pairs as we please. With a view of finding this relation, we observe that the equation (11) of art. 12 may be written thus:

\[
\left( \frac{du}{d\theta} + \frac{du_1}{d\theta} \right) \left( \frac{du}{d\theta} - \frac{du_1}{d\theta} \right) + \frac{1}{\sin^2 \theta} \left( \frac{du}{d\phi} + \frac{du_1}{d\phi} \right) \left( \frac{du}{d\phi} - \frac{du_1}{d\phi} \right) = 0;
\]

or, setting for brevity,

\[
2v = u + u_1, \quad 2v_1 = u - u_1,
\]

thus:

\[
\frac{dv}{d\theta} \cdot \frac{dv_1}{d\theta} + \frac{1}{\sin^2 \theta} \frac{dv}{d\phi} \cdot \frac{dv_1}{d\phi} = 0.
\]

But if \( \rho \) and \( \rho_1 \) be radii vectores of new surfaces, such that

\[
v = \log \frac{\rho}{\gamma}, \quad v_1 = \log \frac{\rho_1}{\gamma_1},
\]

where \( \gamma, \gamma_1 \) are arbitrary lines, it is evident, on the one hand, from (17), that

\[
\frac{\rho}{\gamma} = \sqrt{\frac{r \cdot r_1}{c \cdot c_1}}, \quad \frac{\rho_1}{\gamma_1} = \sqrt{\frac{r \cdot r_1}{c \cdot c_1}};
\]

that is to say, that \( \rho \) is proportional to the geometrical mean between \( r \) and \( r_1 \), and \( \rho_1 \) to the geometrical mean between \( r \) and \( r_1' \), the inverse of \( r_1 \); whilst on the other hand, it appears from the equations (18) and (10) that these new surfaces (\( \rho \)) and (\( \rho_1 \)) have their corresponding normal vector-planes perpendicular to each other. We conclude, then, that if two surfaces (\( r \)) and (\( r_1 \)) attract the pole in the same manner, and (\( r_1' \)) be a third surface inverse to either of the former, the two surfaces \( \rho = \sqrt{r \cdot r_1} \) and \( \rho_1 = \sqrt{r \cdot r_1'} \), whose radii vectores are respectively the mean proportionals between \( r \) and \( r_1 \), and between \( r \) and \( r_1' \), will have their corresponding normal vector-planes perpendicular to each other.

21. The converse of this theorem is also true, and will be useful to us; that is to say, if (\( \rho \)) and (\( \rho_1 \)) be any two surfaces whose corresponding normal vector-planes are perpendicular to each
other, and if \( (\rho'_1) \) be a third surface inverse to either of the former, the pole will be attracted in the same manner by the two surfaces
\[
r = \sqrt{\rho \rho_1} \quad \text{and} \quad r_1 = \sqrt{\rho_1 \rho'},
\]
whose radii vectores are, respectively, mean proportionals between \( \rho \) and \( \rho_1 \), and between \( \rho \) and \( \rho'_1 \).

For by hypothesis,
\[
2u = v + v_1,
2u_1 = v - v_1;
\]
whence, differentiating, squaring, &c., due regard being had to the equation (15), which by hypothesis is here satisfied, we easily find that
\[
\left( \frac{du}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{du}{d\phi} \right)^2 = \left( \frac{du_1}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{du_1}{d\phi} \right)^2,
\]

since each is equal to
\[
\frac{1}{2} \left\{ \left( \frac{dv}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{dv}{d\phi} \right)^2 + \left( \frac{dv_1}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{dv_1}{d\phi} \right)^2 \right\};
\]
or, as otherwise expressed,
\[
\tan^2 \psi = \tan^2 \psi_1 = \frac{1}{2} (\tan^2 \varpi + \tan^2 \varpi_1),
\]
where \( \psi, \psi_1, \varpi, \varpi_1 \) represent respectively the acute angles between any radius vector and the corresponding normals to the surfaces \((r), (r_1), (\rho), (\rho_1)\).

22. From the above it appears that there is no immediate relation between the attractions of either of the surfaces \((r), (r_1)\) and those of \((\rho)\) and \((\rho_1)\); nevertheless, from the theorems of the last two articles, we may deduce the corollary, that when the surfaces \((r)\) and \((r_1)\) not only attract equally, but also have their corresponding normal vector-planes perpendicular to each other, the surfaces \((\rho)\) and \((\rho_1)\) enjoy the same properties, and then
\[
\tan^2 \psi = \tan^2 \psi_1 = \frac{1}{2} \tan^2 \varpi = \frac{1}{2} \tan^2 \varpi_1.
\]
For instance, since, from art. 16, the two surfaces
\[
\rho = \gamma \tan^m \frac{\theta}{2},
\rho_1 = \gamma_1 e^m \varphi
\]
attract the pole equally, and at the same time have their corresponding normal vector-planes perpendicular to each other, it follows from the above that the surfaces
\[
r = c \cdot e^\frac{m \varphi}{2} \cdot \tan^m \frac{\varphi}{2},
\]
\[
r_1 = c_1 \cdot e^{-\frac{m \varphi}{2}} \cdot \tan^m \frac{\varphi}{2}
\]
enjoy the same properties, and in this case
\[
\tan^2 \psi = \tan^2 \psi_1 = \frac{1}{2} \tan^2 \varpi = \frac{1}{2} \tan^2 \varpi_1 = \frac{m^2}{2} \cdot \frac{1}{\sin^2 \theta}.
\]
[To be continued.]

By Professor Schönbein*.

My dear Faraday,

These last six months I have been rather busily working on oxygen, and flatter myself not to have quite in vain maltreated my favourite; for I think I can now prove the correctness of that old idea of mine, according to which there are two kinds or allotropic modifications of active oxygen, standing to each other in the relation of + to —, i.e. that there is a positively-active and a negatively-active oxygen,—an ozone and an antozone, which on being brought together neutralize each other into common or inactive oxygen, according to the equation \( \Theta + \bar{\Theta} = 0 \).

The space allotted to a letter being so small, I cannot enter into the details of my late researches, and must confine myself to some general statements, which I hope, however, will give you a clear notion of the nature of my recent doings. Having written a paper on the subject, that will before long be published in the Transactions of the Academy of Munich, I shall not fail to send it to you as soon as possible.

Ozonized oxygen, as produced from common oxygen by the electrical spark or phosphorus, is identical with that contained in a number of oxy-compounds, the principal ones of which are the oxides of the precious metals, the peroxides of manganese, lead, cobalt, nickel and bismuth,—permanganic, chromic and vanadic acids; and even the peroxides of iron and copper may be numbered amongst them.

The whole of the oxygen of the oxides of the precious metals exists in the ozonic state, whilst in the rest of the oxy-compounds named, only part of their oxygen is in that condition. I call that oxygen negatively-active, or ozone par excellence, and give it the sign \( \Theta \) on account of its electromotive bearing. Though generally disinclined to coin new terms, I think it convenient to denominate the whole class of the oxy-compounds containing \( \Theta \) "ozonides." There is another less numerous series of oxy-compounds in which part of their oxygen exists in an opposite active state, i.e. as \( \bar{\Theta} \) or antozone, wherefore I have christened them "antozonides." This class is composed of the peroxides of hydrogen, barium, strontium, and the rest of the alkaline metals; and on this occasion I must not omit to add, that what I have hitherto called ozonized oil of turpentine, aether, &c., contain their active oxygen in the \( \Theta \) state, and belong therefore to the class of the "antozonides."

* Communicated by Professor Faraday.
Now, on bringing together (under proper circumstances) any ozonide with any antozonide, reciprocal catalysis results, the \( \Theta \) of the one and the \( \Theta \) of the other neutralizing each other into \( O \), which, as such, cannot be retained by the substances with which it had been previously associated in the \( \Theta \) or \( \Theta \) condition. The proximate cause of the mutual catalysis of so many oxy-com- pounds depends therefore upon the opposite states of the active oxygen contained in those compounds.

I will now give you some details on the subject.

1. Free ozonized oxygen = \( \Theta \), and peroxide of hydrogen = \( HO + \Theta \), or peroxide of barium = \( BaO + \Theta \) (the latter suspended in water), on being shaken together destroy each other, \( HO + \Theta \) or \( BaO + \Theta \) being reduced to \( HO \) or \( BaO \), and \( \Theta \) and \( \Theta \) transformed into \( O \).

2. Aqueous permanganic acid = \( Mn^{2+}O^3 + 5\Theta \), or a solution of permanganate of potash mixed with some dilute nitric acid, is almost instantaneously discoloured by peroxide of hydrogen or peroxide of barium, the nitrate of the protoxide of manganese being formed in the first case, and in the second, besides this salt, the nitrate of baryta. It is hardly necessary to state, that in both cases the \( \Theta \) of the permanganic acid and the \( \Theta \) of the peroxides of hydrogen or barium are disengaged as \( O \).

3. An aqueous solution of chromic acid containing some nitric or sulphuric acid and peroxide of hydrogen, are rapidly transformed into the nitrate or sulphate of oxide of chromium, \( HO \), and inactive oxygen, which is of course disengaged. A solution of chromic acid mixed with some nitric acid and \( BaO^2 \) gives a similar result, nitrate of baryta and oxide of chromium being formed, and \( O \) disengaged.

4. If you add to a mixture of any peroxide salt of iron and the red ferro-sesquicyanuret of potassium (both substances dis- solved in water) some peroxide of hydrogen, prussian blue will be thrown down and inactive oxygen set free. On introducing into a mixture of nitrate of peroxide of iron and the ferro-ses- quicyanuret of potassium the peroxide of barium, a similar reaction takes place, prussian blue, nitrate of baryta, &c. being formed, and inactive oxygen eliminated. From these facts it appears that, under certain conditions, even peroxide of iron and \( HO^2 \) or \( BaO^2 \) are capable of catalyzing each other into \( FeO \) and \( HO \), or \( BaO \) and \( O \).

5. Under certain circumstances \( PbO^2 \) or \( MnO^2 \) are soluble in strong acetic acid, as you may see in one of my papers joined to this letter; now if you add to such a solution \( HO^2 \) or \( BaO^2 \), the peroxides will be reduced to \( HO \) or \( BaO \), and \( PbO \) or \( MnO \), inactive oxygen being disengaged.

6. It is a well known fact that the oxide of silver = \( Ag\Theta \), or
the peroxide of that metal = AgO², and the peroxide of hydrogen = HO + Ø, catalyze each other into metallic silver, water and inactive oxygen. Other ozonides, such as PbO + Ø or MnO + Ø, on being brought in contact with HO + Ø, are transformed into PbO or MnO, HO and O. Now the peroxide of barium = BaO + Ø, acts like HO + Ø. If you pour water upon an intimate mixture of AgO, or AgO² and BaO², a lively disengagement of inactive oxygen will ensue, AgO, AgO² and BaO² being reduced to metallic silver and baryta. In concluding the first part of my letter, I must not omit to state the general fact, that the oxygen disengaged in all cases of reciprocal catalysis of oxy-compounds, behaves in every respect like inactive oxygen.

There is another set of chemical phenomena, in my opinion, closely connected with the polar states of the active oxygen contained in the two opposite classes of peroxides. You know that a certain number of oxy-compounds, for instance the peroxides of manganese, lead, nickel, cobalt, bismuth, silver, and also permanganic, chromic, and vanadic acids, furnish with muriatic acid chlorine, whilst another set, such as the peroxides of barium, strontium, potassium, &c., are not capable of eliminating chlorine either out of the said acid or any other chloride. This second class of oxy-compounds produces, however, with muriatic acid, the peroxide of hydrogen; and it is quite impossible in any way to obtain from the first class of the peroxides HO², or from the second chlorine.

You are aware that, from reasons of analogy, I do not believe in the doctrine of chlorine, bromine, &c. being simple bodies, but consider those substances as oxy-compounds, analogous to the peroxides of manganese, lead, &c., in other terms, as "ozonides." Chlorine is therefore to me the peroxide of muriur = MuO + Ø, hydrochloric acid = MuO + HO, and, as already mentioned, the peroxide of barium = BaO + Ø, that of hydrogen = HO + Ø, and the peroxide of manganese = MnO + Ø. Proceeding from these suppositions, it is very easy to account for the different way in which the two sets of peroxides are acted upon by muriatic acid.

From reasons as yet entirely unknown to us, HO can be chemically associated only with Ø, and with no other modification of oxygen, to constitute what is called the peroxide of hydrogen; and in a similar way MuO (the hypothetically anhydrous muriatic acid of older times) is capable of being united only to Ø to form the so-called chlorine, which I denominate peroxide of muriur. If we cause MuO + HO to react upon BaO + Ø, MuO unites with BaO, and HO with Ø; but if you bring together MuO + HO with Mn + Ø, part of MuO is associated to MnO, another part to Ø, water being eliminated, according
to the equation

$$2(\text{MuO}, \text{HO}) + \text{MnO} + \Theta = \text{MuO, MnO} + \text{MuO}, \Theta + 2\text{HO}.$$  

As you will easily perceive, from these views it would follow that, under proper circumstances, two opposite peroxides, on being intimately and in the right proportion mixed together and acted upon by muriatic acid, could yield neither chlorine nor peroxide of hydrogen, but mere inactive oxygen. If somewhat dilute muriatic acid be poured upon an intimate mixture of five parts of peroxide of barium and two parts of peroxide of manganese, the whole will be rapidly transformed into the muriates of baryta and protoxide of manganese, the active oxygen of both the peroxides being disengaged in the inactive condition, and not a trace of free chlorine making its appearance. The same result is obtained from dilute hydrobromic acid.

Another consequence of my hypothesis is this: that an intimate and correctly proportioned mixture of two opposite peroxides, such as the peroxide of barium and that of lead, on being acted upon by any oxy-acid, cannot produce the peroxide of hydrogen; or, to express the same thing in other terms, muriatic acid must act upon the said mixture exactly in the same way as the oxy-acids do; and that is indeed the case. Mixtures of the peroxides just mentioned and acetic or nitric acids, are readily converted into the acetates or nitrates of baryta and protoxide of manganese, the active oxygen of both the peroxides being of course disengaged in the inactive condition.

Before I close my long story I must mention one fact more, which, in my opinion, is certainly a very curious one. If you mix an aqueous and concentrated solution of bromine with a sufficient quantity of peroxide of hydrogen, what happens? A very lively disengagement of inactive oxygen takes place, the colour and the odour of the bromine solution disappear, the liquid becomes sour, and on adding some aqueous chlorine to it, bromine reappears. From hence we are allowed to conclude, that, on bringing bromine in contact with peroxide of hydrogen, some so-called hydrobromic acid is produced. The hypothesis at present prevailing cannot account for the formation of that acid otherwise than by admitting that bromine takes up the hydrogen of HO₂, eliminating the two equivalents of oxygen united to H. I, of course, take another view of the case; bromine is to me an ozonide like peroxide of lead, &c., i. e. the peroxide of bromium = BrO + Θ. Now HO + Θ and BrO + Θ catalyze each other into HO, BrO, and inactive oxygen, BrO + HO forming hydrobromic acid, or what might more properly be called hydrate of bromiatic acid.

You see that I am growing more and more hardened in my
heretical notions, or, to speak more correctly, in my orthodox views; for it was Davy who acted the part of a heretic in overthrowing the old, venerable, true creed. Indeed the longer I compare the new and old doctrine on the nature of chlorine, &c. with the whole material of chemical facts bearing upon them, the less I am able to conceive how Davy could so lightly and slightly handle the heavy weight of analogies which, in my opinion, speak so very strongly and decisively in favour of Berthollet's views. There is no doubt Sir Humphry was a man of great genius, and consequently very imaginative; but I am almost inclined to believe that, by a certain wantonness, or by dint of that transcendent faculty of his mind, he was seduced to conjure up a theory intended to be as much out of the way and "inpraesimble" as possible, and serve nevertheless certain theoretical purposes; and certainly, if he entertained the intention of solving such a problem, he has wonderfully succeeded. But what I still more wonder at is both the sudden and general success which that far-fetched and strained hypothesis met with, and the tenacity with which the whole chemical world has been sticking to it ever since its imaginative author pleased to divulge it: and all this could happen in spite of the fact that the new doctrine, in removing from the field of chemistry a couple of hypothetical bodies, was, for analogy's sake, forced to introduce fictitious compounds, not by dozens only, but by hundreds,—the oxy-sulphion, oxy-nitron, and the rest of those "nonentia." But enough of this subject, upon which I am apt to grow warm and even angry. Although the results I have obtained from my recent investigations cannot but induce me to begin another, and, I am afraid, endless series of researches, I shall for the present cut short the matter and indulge for some time in absolute idleness.

I am, my dear Faraday,  
Yours most truly,  
Basle, June 25, 1858.  
C. F. Schönbein.

XX. Preliminary Researches on the alleged Influence of Solar Light on the Process of Combustion. By John Le Conte, M.D., Professor of Natural Philosophy in the South Carolina College.*

A POPULAR opinion has long prevailed in England, and perhaps in other countries, that the admission of the light of the sun to an ordinary fire tends to retard the process of combustion. In some instances, the practice of placing screens

before the fireplace, or of closing the shutters of the apartment, may be traced to the prevalent belief, that the access of sunlight to the burning materials is unfavourable to the continuance of the phænomenon of combustion. Most physical philosophers very naturally regard this opinion as a mere popular prejudice; probably originating in the well-known apparent dulling or obscuration of flames and of solid bodies in a state of ignition, which takes place when they are exposed to strong light. The flame of a jet of burning hydrogen is scarcely visible in the diffused light of a clear day; that of an ordinary alcohol lamp is barely appreciable to the eye when exposed to the direct sunshine, while a portion of ignited charcoal which glows in the dark appears to be extinguished when placed in the sunlight. These familiar phænomena, attributable to well-established physico-physiological laws, seem to afford a much more rational explanation of the origin of the popular opinion, than to suppose it to be based upon observations relating to the actual rapidity of burning.

About thirty-two years ago, Dr. Thomas M'Keever published a series of experiments in the 'Annals of Philosophy*,' which seemed to show that there is a real foundation for the popular impression, and that solar light does actually retard the process of combustion. So far as I am aware, these remarkable experiments have never been repeated. Leopold Gmelin, in his 'Handbook of Chemistry†,' announces Dr. M'Keever's results without comment. The important bearing which they appear to have on the influence of solar light on chemical processes, as well as on the modern dynamical theory of the mutual convertibility of the so-called imponderables, induced me, during the months of May and June last, to undertake a series of experiments with the view of testing the validity of Dr. M'Keever's conclusions. The subjoined Table will exhibit his results in a convenient form for future reference:

<table>
<thead>
<tr>
<th>Exp. 1.—Green Wax Taper lost in 5 minutes</th>
<th>Exp. 2.—Taper lost, by burning 7 minutes, in</th>
</tr>
</thead>
<tbody>
<tr>
<td>In Sunshine, Temp. 78°, 8.5 grs.</td>
<td>Sunshine, Temp. 78°, 10 grs.</td>
</tr>
<tr>
<td>Ratio.</td>
<td>Ratio.</td>
</tr>
<tr>
<td>1 : 1.088</td>
<td>1 : 1.100</td>
</tr>
</tbody>
</table>

† Leopold Gmelin's 'Handbook of Chemistry' (Cavendish Society's Translation), vol. ii. p. 35. London, 1849. A contemporary journal, in noticing these results, remarks, "It has always been considered a vulgar error that the sun's light extinguishes a fire, but the following experiments by Dr. M'Keever put the matter beyond a doubt." (Brewster's Edinb. Journ. of Science, vol. v. p. 180. 1826.)
Exp. 3.—Mould Candle, to consume 1 inch, took in

| Dark, Temp. 68°, 56° 0′ | Sunshine, Temp. 80°, 59° 0′ | Ratio. | 1 : 1-053 |

Exp. 4.—Taper, to consume 1 inch, took in

| Dark, Temp. 67°, 4° 30′ | Sunshine, Temp. 79°, 5° 0′ | Ratio. | 1 : 1-111. |

Exp. 5.—Taper in Sunshine lost in 10 minutes

| In Painted Lantern, 16-5 grs. | In Uncoated Lantern, 15 grs. | Ratio. | 1 : 1-100 |

A *sixth* experiment, of a similar character, made in a strong *moonlight*, indicated no such diminution in the rate of consumption.

The conclusion to which Dr. M'Keever came was, that solar light *does exercise* a positive retarding influence on the process of combustion. He supposes this effect to be owing to the well-known influence of the solar rays on many chemical processes,—in some instances accelerating them, but in others retarding them. Under this point of view, the *chemical rays* may be supposed to exercise a *deoxidizing* power, which to some extent interferes with the rapid oxidation of the combustible matter. In confirmation of this opinion, Dr. M'Keever made an experiment which appears to indicate that a taper burns more rapidly in the *red* than in the *violet* extremity of the solar spectrum.

In attempting a repetition of Dr. M'Keever's experiments, I found it impossible to secure that freedom from agitation in the atmosphere, during the exposure of the burning body to the influence of sunshine in the *open air*, which such an investigation demanded. This was his method of conducting the *first four* experiments given in the Table. The powerful influence exercised by comparatively slight disturbances in the air on the rapidity of combustion, renders attention to this circumstance of controlling importance. His method of obviating this difficulty by the use of lanterns (as indicated in experiment 5) is objectionable, from the impossibility of securing precisely identical conditions in relation to the supply of air in the interior. There were likewise other considerations which urged me to modify his method of conducting the investigation. It occurred to me that, as in his experiments the *temperature* of the air which supplied oxygen for combustion in the sunshine was about 12° F. above that in the darkened room, the *rarefaction* produced by heat might exercise some influence in retarding the rate of burning in the sunlight.

In conducting my experiments, I endeavoured to secure *two conditions*, viz.—

1. Absolute *calmness* in the atmosphere.
2. Exposure of the flame to the influence of intense solar light, without heating the surrounding air.

The first condition was secured by performing all the experiments in a large lecture-room, with all the doors and windows closed. To secure the second condition, I employed a portion of the apparatus belonging to a large solar microscope, consisting of the reflecting mirror, the condensing lens and tube, together with the mechanical arrangements for adjusting the direction of the light. As the condensing lens was upwards of 4 inches in diameter, I hoped to exaggerate enormously whatever effect the light might exert, by concentrating it on a comparatively small area. Inasmuch as the aperture in the window through which the light was admitted was completely closed by this arrangement, the exterior agitations of the atmosphere were not felt in the room, while the pencil of light thus thrown on the flame traversed it, as well as the surrounding air, without imparting a sensible amount of heat to the latter.

I used the best wax-candles (as they are called in the shops), four to the pound, costing about 15 cents apiece*. By allowing them to burn a sufficient length of time to form a well-defined cup for the melted wax, and carefully turning the wicks so as to render them self-snuffing, the combustion was found to go on with remarkable uniformity in a calm atmosphere. The rate of burning was determined in the following manner:—A portion of candle, three or four inches in length, was secured to the bottom of one of the scale-pans of a tall balance and ignited; after allowing it to burn for ten or fifteen minutes, so as to secure a steady flame of constant size, it was nearly balanced by adding weights to the opposite scale-pan, allowing a slight preponderance to the candle-pan. In a short time the equilibrium was established by the burning of the candle: the precise time at which the balance indicated a condition of equilibrium was accurately noted. Next, a given weight (say 60 or 100 grains) was withdrawn from the weight-pan, and the time of restoring the equilibrium by the loss of weight in the burning candle was in like manner recorded. In this manner the rate of combustion was determined by observing the time occupied in consuming a given weight of the burning matter. The arrangements described above enabled me to perform such experiments alternately in

* From the close approximation to identity in the rate of consumption, it is probable that these are the same as Dr. Ure's "genuine wax-candles." He found the consumption to be, "upon an average of many experiments, 125 grains per hour." (Dict. of Arts, Manufactures, and Mines, 4th edition, article "Illumination, Cost of." My experiments gave respectively 136\(^{\text{\textordervinum}}\), 125\(^{\text{\textordervinum}}\), and 124\(^{\text{\textordervinum}}\) grains per hour. Other kinds of candles burn at a much more rapid rate.
the darkened room and in the concentrated sunbeam, without moving any portion of the apparatus in the room, and under external conditions as nearly identical as could be desired. Many preliminary experiments were made for the purpose of testing the delicacy of the arrangements, which very soon convinced me that no reliable results could be obtained unless the air was calm, and also unless the candle was allowed to burn a sufficient length of time to establish regularity in the process of combustion. The days selected for the experiments were perfectly cloudless. The state of the barometer and thermometer were carefully noted. I now regret that I neglected to record the hygrometric condition of the air. The cone of sunlight was so directed that its lower margin illuminated the charred portion of the wick of the candle, while the upper boundary of the pencil traversed the flame near its apex. The following Table presents the results furnished by three sets of experiments performed on as many separate days*.

<table>
<thead>
<tr>
<th>Date</th>
<th>Barometer reduced to 32° F.</th>
<th>Temp. of air, Fahr.</th>
<th>Time of consuming 60 grams.</th>
<th>Amount consumed in 10 minutes.</th>
<th>Diff. grs.</th>
</tr>
</thead>
<tbody>
<tr>
<td>May 9</td>
<td>29-92</td>
<td>67</td>
<td>26</td>
<td>24</td>
<td>26</td>
</tr>
<tr>
<td>June 6</td>
<td>29-72</td>
<td>75·5</td>
<td>28</td>
<td>39</td>
<td>28</td>
</tr>
<tr>
<td>10</td>
<td>29-62</td>
<td>84</td>
<td>28</td>
<td>55</td>
<td>28</td>
</tr>
</tbody>
</table>

It will be observed that these experiments indicate no sensible difference in the rate of combustion of the candle in the darkened room and (in the same apartment) with a pencil of concentrated sunlight directed on the flame, provided the comparison is restricted to the results obtained on any given day. In two instances there was a slight excess in the rate of burning in the

* The difficulty of keeping the pencil of solar light properly directed on a flame of variable altitude, induced me to try a "burning fluid" lamp, having a short cylindrical reservoir, furnished with two wicks. The following results were obtained:

<table>
<thead>
<tr>
<th>Date</th>
<th>Barometer. Temperature.</th>
<th>Amount consumed in 10 minutes.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Dark = 40·82 grs.</td>
</tr>
<tr>
<td>June 3</td>
<td>29·72 in. 74° F.</td>
<td>&quot; = 40·95</td>
</tr>
<tr>
<td>4</td>
<td>29·78</td>
<td>&quot; = 38·34</td>
</tr>
<tr>
<td>4</td>
<td>29·78</td>
<td>&quot; = 37·52</td>
</tr>
</tbody>
</table>

The progressive decrease in the rate of combustion, as indicated by the experiments of the 4th of June, shows that the variations in the height of the liquid are sufficient to vitiate the results obtained from burning any fluid material.
sunshine, and in the other the excess was in favour of the dark; but the differences are probably within the limits of the experimental inaccuracies incident to such delicate investigations.

These negative results are the more striking from the fact that, if solar light exercised the decided influence on the process of combustion which Dr. M'Keever's experiments seem to indicate, we should expect the effects to be much more marked and conspicuous when the light was increased in intensity from *eight-to tenfold* by the concentration of a lens. The fact that the rays of the sun traversed the glass lens before they fell on the flame, can scarcely be urged as a possible explanation of the discrepancy; for Dr. M'Keever obtained analogous results when he employed lanterns. (*Vide* experiment No. 5.)

The obvious variation in the rapidity of combustion on *different days* (as exhibited in my experiments) illustrates in a most striking manner the decided influence exercised on the process by comparatively slight alterations in the external conditions. This fact should inspire us with wholesome caution, and check the spirit of rash generalization. Throwing out of consideration the possible fluctuations in the rate of burning arising from want of homogeneity in the combustible materials, and imperfections in the mechanical arrangements by which they are consumed, there are *three external conditions* which may be supposed to exercise more or less influence on the rapidity of the process. These are, 1st, barometric pressure; 2nd, the temperature of the air; and 3rd, the amount of aqueous vapour present. I propose to consider each of these separately.

1. Barometric Pressure.

From *a priori* considerations, we should be led to expect that an increase of barometric pressure, through the consequent condensation of the air, would, *caeteris paribus*, tend to *augment* the rapidity of combustion, by furnishing the burning matter with a greater amount of oxygen in a given volume. Unfortunately, direct experiments are wanting to test this in as satisfactory a manner as we should desire. The older experiments in the Boylean vacuum, inasmuch as they relate to the degree of rarefaction at which combustion *ceased*, do not give us information in regard to the *rapidity* of the process at the various stages of exhaustion. The same remark applies to the later experiments of Grotthuss, as well as to the admirable "*Researches*" of Sir Humphry Davy on the "*Effects of Rarefaction, by partly removing the Pressure of the Atmosphere, upon Flame and Explosion*." [*Vide* Davy's "*Researches on Flame*," Phil. Trans. for 1817, p. 45 *et seq.*; Also *'Works of Sir H. Davy,*' edited by Dr. John Davy, vol. vi. p. 51 *et seq.* London, 1840.]
The experiments of the latter show that rarefaction produces striking alterations in the size and character of the flame, but do not touch the question of the relative rate of burning under different pressures: they test the comparative combustibility of different bodies, rather than the rapidity of consumption of a given body under various degrees of rarefaction. Nevertheless Davy informs us that he determined from actual experiment, that the amount of heat developed in a given time by combustion is slowly diminished by rarefaction, "the diminution of the cooling power of the nitrogen being apparently in a higher ratio than the diminution of the heating powers of the burning bodies." Speaking of the phenomena of combustion in condensed air, he says, "I ascertained, however, that both the light and heat of the flames of the taper, of sulphur, and hydrogen were increased by acting on them by air condensed four times; but not more than they would have been by an addition of one-fifth of oxygen." Again, he says, "But by compression, there can be no doubt the heat of flames from pure supporters and combustible matter may be greatly increased, probably in the ratio of their compression:" in the case of air he does not think the effect would be so great. Inasmuch as the quantity of heat developed in a given time by the burning of a given substance is known to be a measure of the amount of matter undergoing oxidation, we are justified in the inference, that the foregoing results of Sir H. Davy's experiments show that the rate of combustion was retarded by the rarefaction, and accelerated by the condensation of the air.

The most satisfactory results in relation to the influence of condensed air on the process of combustion, are those incidentally furnished about sixteen years ago by M. Triger, a French civil engineer, during the operations necessary for working a bed of coal lying under the alluvium bordering the river Loire, near Languvin in the department of Maine-et-Loire. In traversing an overlying stratum of quicksand from 59 to 65½ feet thick, he found it requisite to devise some means of excluding the semi-fluid quicksand and water, which found their way, under every arrangement analogous to ordinary cofferdams, in such quantity as to defy all pumping operations intended to keep them dry. For this purpose, M. Triger employed large sheet-iron cylinders, about 3'39 feet in interior diameter, securely closed at the top, in which, by means of a condensing-pump incessantly worked by a steam-engine, air was condensed to an amount sufficient to counteract the external hydrostatic pressure. The ingenious contrivance fully justified the expectations of the engineer; but the workmen were thus compelled to labour in air condensed under a pressure of about three atmospheres. Among other curious results of this state of things, noticed by M. Triger, were the
remarkable effects of condensed air on combustion. Much anno-
yance was at first experienced from the rapid combustion of the

candles, which was only obviated by substituting flax for cotton
threads in the wicks*. Similar phenomena were observed a few
years ago by the engineers of the Wilmington and Manchester
Railway, who employed analogous apparatus for securing the
foundations for the piers of the railroad bridge across the Great
Pee Dee river in South Carolina. So far as I have been able to
ascertain, the results manifested in this case were identical with
those recorded by M. Triger, and afford a most striking confirm-
ation of the influence of condensed air in accelerating the process
of combustion.

On the other hand, facts are not wanting to prove that com-
bustion is retarded at considerable elevations above the ocean,
where the air is rarefied by diminished pressure. In a letter
recently communicated to the Royal Society of London, from J.
Mitchell, Esq., Quartermaster of Artillery at Bangalore, India,
"On the Influence of Local Altitude on the Burning of the
Fuses of Shells," this officer shows that there was a progressive
retardation of the rate of combustion of the fuses at altitudes of
3000, 6500, and 7300 feet, as contrasted with the rapidity of
burning at the Artillery Depot yard. This difference Mr.
Mitchell very rationally attributes "to the rarity of the atmo-
spheric air, and of its constituent oxygen, at the higher sta-
tions†. The following Table, in which I have reduced the
barometric heights to the freezing-point, exhibits the mean results
of his experiments:

<table>
<thead>
<tr>
<th>Height in feet.</th>
<th>Barometer at 32° F.</th>
<th>Temperature, Fahr.</th>
<th>Average time of burning 3 inches of fuse.</th>
<th>Number of experiments.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Depôt.</td>
<td>29-610</td>
<td>89</td>
<td>14:25 seconds.</td>
<td>6</td>
</tr>
<tr>
<td>3000 feet.</td>
<td>26-765</td>
<td>82</td>
<td>15:78</td>
<td>5</td>
</tr>
<tr>
<td>6500 &quot;</td>
<td>23-951</td>
<td>61-8</td>
<td>17:10</td>
<td>3</td>
</tr>
<tr>
<td>7300 &quot;</td>
<td>22-979</td>
<td>54-2</td>
<td>18:125</td>
<td>2</td>
</tr>
</tbody>
</table>

These experiments seem to have been made with great care,

The following are the words of M. Triger:—"À la pression de trois atmo-
sphères cette accélération devient telle que nous avons été obligés de renoncer
aux chandelles à mèches de coton pour les remplacer par des chandelles à
mèches de fil. Les premières brûlaient avec une telle rapidité, qu'elles
duraient à peine un quart d'heure, et elles répandaient en outre une fumée
intolérable."

† Philosophical Magazine, S. 4. vol. x. p. 48, July 1855. Fuses
burn without air; but the rate of burning is influenced by atmospheric
oxygen.
and all in the presence of artillery officers, who were furnished with the most accurate methods of measuring time. They amply prove the fact that combustion is retarded at considerable elevations.

Thus a variety of well-established facts concur in fortifying the conclusions to which we are led by a priori reasoning,—namely, that the process of combustion is retarded by diminution of the density of the air, while it is accelerated by its condensation. It has long been a matter of common observation, that ordinary wood-fires burn more freely when the barometer is high; but Mr. Marcus Bull and others maintain* that this result is not owing to the augmented density of the air, but to the greater dryness of the atmosphere. The facts brought forward in this paper are strongly opposed to this explanation; for there are not the slightest grounds for supposing that there was less than the ordinary amount of aqueous vapour present in the condensing cylinders of M. Triger, or more than the usual quantity mixed with the air at the elevated stations in India. On the contrary, physical considerations lead us to precisely opposite conclusions.

2. Temperature of the Air.

In relation to the influence of the temperature of the air on the rate of combustion, our information is still more meagre. The experiments of Grotthuss and Sir H. Davy on the “Effects of Rarefaction by Heat on Combustion and Explosion,” give contradictory results†; but as they relate exclusively to the influence of temperature on the ignition of explosive mixtures of gases, they test its effects on combustibility, and are obviously inapplicable to the question under consideration. The well-known effects of the “hot blast” in increasing the temperature of furnaces, cannot be applied as a test of the influence of warm air on the rate of combustion under ordinary circumstances. First, because the air of the “hot blast” is not in its natural state of density; and secondly, because the augmentation of temperature observed in such cases probably arises from its greater availability, growing out of the fact that less heat is carried off in the products of combustion, rather than an absolute increase in the rapidity of burning.

In the absence of direct experimental evidence, it may be admissible to apply general reasoning based upon well-known physical principles. So far as an increase of temperature influences the density of the air, it is sufficiently evident that its

---


† Phil. Trans. for 1817, p. 53.
effect must be equivalent to a diminution of barometric pressure, and consequently must tend to \textit{retard} the process of combustion. Assuming the temperature of the flame to be constant, it is likewise plain that the draught created by it—depending, as it is known, on the \textit{difference} of temperature between the flame and that of the surrounding air—must be \textit{diminished} in a warm atmosphere, and therefore also tend to \textit{retard} the rate of combustion, \textit{ceteris paribus}, during hot seasons. But inasmuch as the variations in the velocity of the draught are proportional to the square roots of these differences of temperature, it is obvious that its effects must be insignificant under ordinary fluctuations of atmospheric temperature. For example, supposing the temperature of the flame to be 1500° F., then the fluctuation of the draught between the temperatures of 80° and 60° F. would be in the ratio of

$$\sqrt{1500 - 80} : \sqrt{1500 - 60} = \sqrt{1420} : \sqrt{1440} = 1 : 1.0070.$$  

When, however, the comparison is made between bodies burning in summer and in winter, the influence from this cause will be more sensible, and ought not to be entirely overlooked*.

On the contrary, it is \textit{possible} that an augmentation of temperature might tend to \textit{accelerate} the process of combustion by favouring the liquefaction of the wax, and perhaps facilitating the oxidation of the combustible matter. If, however, any such influence is exercised, it is probable that its effect must be inappreciable under ordinary circumstances. Under this view of the subject, the only obvious influence which atmospheric heat exercises on the rapidity of combustion is connected with its effects on the \textit{density} of the air; and consequently an increase of temperature should, \textit{ceteris paribus}, \textit{retard} combustion, and \textit{vice versa}.

3. \textit{Amount of Aqueous Vapour present.}

Sir Humphry Davy found that "a very large quantity" of \textit{steam} was required to prevent sulphur from burning; that an explosive mixture of oxygen and hydrogen, when mixed with five times \textit{its volume} of \textit{steam}, still exploded by the electric spark; and that a mixture of air and carburetted hydrogen gas required "a \textit{third} of \textit{steam} to prevent its explosion, whereas one-fifth of azote produced the effect†." Under any point of view, it is obvious that the presence of aqueous vapour can only tend to \textit{retard} the

---

* I endeavoured to test the influence of \textit{temperature} on the rate of combustion by placing the burning candle over a large heated plate; but, as might have been expected, the unsteadiness of the flame rendered the experiment unsatisfactory.

† Phil. Trans. for 1817, p. 65.
process of combustion, first, because it diminishes the amount of oxygen in a given volume of air; and secondly, because an admixture of any inactive gas tends to extinguish the burning body, as is abundantly proved by the experiments of Sir H. Davy and others. When vapour is present in large quantities, there can be no doubt of its controlling agency on combustion. This is illustrated by the successful application of the plan proposed by M. Dujardin of Lille, in 1837, for extinguishing fires occurring in steam-ships, by permitting the steam from the boilers to escape into the apartment in which the combustion originates*. But experiments are still wanting for determining its influence on the rate of burning, when existing in the small quantities in which it is usually associated with the atmosphere†. The experimental researches of Mr. David Waldie, in relation to the mixture of various gases with air, led him to the general law, that "of incombustible gases which remain undecomposed, the power of preventing combustion is in the order of their density;" and that "this effect of density in cooling the flame depends on the excessive diffusion of the flame in the denser gas." Under ordinary circumstances, the density of the aqueous vapour existing in the air is comparatively small, so that, according to Mr. Waldie's law, its influence on combustion ought not to be very striking. It is very desirable that this point should be submitted to a more rigorous experimental investigation.

Having discussed the probable influence of the three external conditions on the rate of combustion, we are in a measure prepared to investigate their adequacy to explain the variations in the rapidity of burning, as indicated by the experiments which I have brought forward. In none of them have we the observations necessary for ascertaining the hygrometric condition of the atmosphere; this must therefore be thrown out of consideration. In Dr. M'Keever's experiments the barometric indications are not given, neither is it known how many of them were performed on any one day; in my experiments, as well as in those of Mr. Mitchell, we are furnished with the data requisite for estimating the combined influence of pressure and temperature. Assuming, with Sir H. Davy, that the rapidity of combustion is in the direct ratio of the density of the air, we may submit these

† The curious results obtained by Mr. J. F. Dana, and subsequently Mr. Samuel Morey, in relation to increasing the brightness of the flames of highly carbonaceous combustibles, by throwing a jet of steam into them, are obviously inapplicable to candles. (Vide Silliman's Journal, 1st series, vol. i. p. 401; vol. ii. pp. 118, 122; and vol. vii. p. 141.)
two effects to a *quantitative* estimation, by using Mariotte’s law and Regnault’s coefficient of expansion for air.

1. Presuming that *each set* of experiments made by Dr. M’Keever, alternately in the dark and in the sunshine, were performed on the *same day*, and therefore under *identical* barometric conditions, we may form some estimate of the adequacy of *temperature* to account for the difference in the rate of burning observed by him. The subjoined Table, which I have constructed from the data previously given, will place this in a clear light:

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>No. 1</td>
<td>67° F.</td>
<td>78° F.</td>
<td>18:50 grs.</td>
<td>17:00 grs.</td>
<td>1:1.0088</td>
<td>1:1.021</td>
</tr>
<tr>
<td>No. 2</td>
<td>67</td>
<td>78</td>
<td>15:72</td>
<td>14:29</td>
<td>1:1.0100</td>
<td>1:1.021</td>
</tr>
<tr>
<td>No. 3</td>
<td>68</td>
<td>80</td>
<td>......</td>
<td>......</td>
<td>1:1.053</td>
<td>1:1.023</td>
</tr>
<tr>
<td>No. 4</td>
<td>67</td>
<td>79</td>
<td>......</td>
<td>......</td>
<td>1:1.111</td>
<td>1:1.023</td>
</tr>
</tbody>
</table>

The remarkable discrepancies indicated by the numbers in the first column of *ratios* afford a striking illustration of the existence of some disturbing cause, tending to vitiate the accuracy of these experiments. But a glance at the numbers contained in the two columns of *ratios* is sufficient to show that *temperature alone* is entirely *inadequate* to account for the diminished rate of combustion in the sunshine. A remarkable difference is observed in the *rate* of consumption in experiments 1 and 2. No. 1 was made with a “green wax taper,” and No. 2 with a “taper;” but, as from the context, the second experiment appears to be a repetition of the first, the presumption is, that the same kind of taper was used in both cases. The rate of burning in experiments Nos. 3 and 4 was determined by the *time* required to consume a *given length*; and as one of them was made with a mould-candle and the other with a taper, no comparison can be extended to them, so far as the rates of consumption in these two cases are concerned. The irregularities exhibited in these results probably arose from the agitations of the atmosphere, which were incident to the method of exposing the burning body to the sunshine in the *open air*. As the excess of the consumption in the dark varied from 5 to 11 per cent., whereas the excess in the density of the air was only 2-3 per cent., it is evident that some other cause than temperature must be evoked to explain the difference.

2. In my experiments, the conditions were such as to eliminate the effects of *temperature* on the results obtained in the dark and in the sunshine on any given day; and it has been shown that for *each pair* of experiments thus conducted, the variations

in the rate of combustion do not exceed the probable limits of experimental error. In this case, therefore, the question to be determined is, whether the differences in the rapidity of burning observed on different days can be explained by the variations of the barometer and thermometer? For this purpose I shall take the average of each pair of experiments, as a nearer approximation to the correct rate of burning on each of the three days. The following Table, in which the relative densities of the air have been calculated by combining the effects of barometric and thermometric oscillations, will serve to illustrate this point:—

<table>
<thead>
<tr>
<th>Ratio of consumption.</th>
<th>Ratio of density of air.</th>
<th>Difference.</th>
</tr>
</thead>
<tbody>
<tr>
<td>In exps. 1 &amp; 2 = 1:1.0904</td>
<td>1:1.0230</td>
<td>+6.74 per cent.</td>
</tr>
<tr>
<td>&quot; 2 &amp; 3 = 1:1.0663</td>
<td>1:1.0193</td>
<td>-1.30 &quot;</td>
</tr>
<tr>
<td>&quot; 1 &amp; 3 = 1:1.0967</td>
<td>1:1.0428</td>
<td>+5.39 &quot;</td>
</tr>
</tbody>
</table>

It will be seen that the rate of combustion increases in a decidedly higher ratio than the density of the air. If therefore we assume that the rapidity of burning is, cæteris paribus, in the direct ratio of the density of the air, it follows that some other agency must have cooperated in these cases.

3. The results of Mr. Mitchell’s experiments at different altitudes may in like manner be subjected to a similar numerical test. The subjoined Table is constructed from the data contained in that which is given on a previous page:—

<table>
<thead>
<tr>
<th>Ratio of rate of burning.</th>
<th>Ratio of density of air.</th>
<th>Difference.</th>
</tr>
</thead>
<tbody>
<tr>
<td>In exps. 1 &amp; 2 = 1:1.1074</td>
<td>1:1.0926</td>
<td>+1.48 per cent.</td>
</tr>
<tr>
<td>&quot; 2 &amp; 3 = 1:1.0836</td>
<td>1:1.0755</td>
<td>+0.81 &quot;</td>
</tr>
<tr>
<td>&quot; 3 &amp; 4 = 1:1.0589</td>
<td>1:1.0270</td>
<td>+3.29 &quot;</td>
</tr>
<tr>
<td>&quot; 1 &amp; 3 = 1:1.2000</td>
<td>1:1.1751</td>
<td>+2.49 &quot;</td>
</tr>
<tr>
<td>&quot; 2 &amp; 4 = 1:1.1486</td>
<td>1:1.1045</td>
<td>+4.41 &quot;</td>
</tr>
<tr>
<td>&quot; 1 &amp; 4 = 1:1.2719</td>
<td>1:1.2068</td>
<td>+6.51 &quot;</td>
</tr>
</tbody>
</table>

This comparison places in a still stronger light the fact, that the augmentation in the rate of burning increases in a somewhat higher ratio than the density of the air, while at the same time it clearly demonstrates the controlling influence of atmospheric den-

* In making these calculations I used the following formula, based upon the two well-known physical laws, that the density of any permanent gas varies directly as the compressing force, and inversely as the volume:—

\[
d : d' = \frac{b}{1 + 0.002036(t - 32)} : \frac{b'}{1 + 0.002036(t' - 32)};
\]

in which \(d\) and \(d')\) represent the densities; \(b\) and \(b'\) the barometric heights reduced to the freezing-point; and \(t\) and \(t'\) the temperatures on Fahrenheit’s scale.
sity on the phenomenon of combustion. The extreme rates of burning are as the numbers 100 to 127, while the corresponding densities of the air are as 100 to 121 nearly; in the other cases the approximation to identity in the ratios is still closer. Would the variations in the hygrometric state of the atmosphere, which we have left out of consideration, explain this discrepancy? In the absence of the experiments necessary for testing this question, it would be premature to hazard any conjecture. I may remark, however, that in the case of Mr. Mitchell’s experiments, the correction for the effects of aqueous vapour would probably in one point of view operate in the wrong direction, and thus tend to increase the discrepancy in the ratios. For as the temperature was decidedly higher at the lower stations, it is more than probable that the tension of vapour was greater there than at the upper ones, and consequently that its influence in retarding combustion should be relatively greater at the points nearer the sea-level. This of course would tend to equalize the rates of burning at lower and higher altitudes, when no correction is made for this cause. On the contrary, it is obvious that the influence of vapour having a given tension in altering the relative amount of air in a given volume, must be greater when the barometer is low. From this cause the aqueous vapour at the upper stations might have had a greater effect in retarding combustion, and thus tended to exaggerate the difference in the rates of burning.

The comparatively large rate of consumption indicated by my first experiment of the 9th of May (being more than 9 per cent. above the other) was most probably attributable to a combination of causes. All the three external conditions concurred in accelerating the process. The barometer was high, the temperature low, and the atmosphere excessively dry. The last-mentioned condition was accidentally forced upon my attention from the fact, that on that day I failed in an experiment for determining the dew-point by means of Daniell’s hygrometer*.

* Collaterally related to this subject are the effects of condensed and rarefied air and of temperature on the process of respiration and the elimination of carbonic acid in men and other warm-blooded animals. M. Legallois found that, when warm-blooded animals breathed air under pressure reduced to 11.811 inches, the amount of oxygen consumed was diminished (Ann. de Chim. et de Phys. vol. iv. p. 113. 1817). M. Théodore Junod’s experiments show that condensed air produced deep inspirations and an agreeable glow throughout the system, while rarefied air had an opposite effect. (Archives Générales de Médecine, ser. 2. vol. ix. p.157. Paris, 1835. Also Magendie’s Report on the same Memoir, Comptes Rendus, vol. i. p. 60. Paris, 1835.) The observations of M. Triger, already referred to, indicate analogous effects on those who laboured in the condensed air. They could do double work without fatigue, and even old asthmatics seemed to recover their vigour. (Comptes Rendus, vol. xiii. p.884 et seq. Paris, 1841.)
From the foregoing discussion it is evident that the subject demands a thorough experimental investigation, with a minute attention to all the external conditions which may influence the results. This I propose to undertake during the next twelve months. In the meantime it is hoped that these preliminary researches may prepare the way for a clearer appreciation of the difficulties which are to be encountered. Perhaps, however, in the present stage we may be warranted in deducing two conclu-

M. Vierordt tested the effects of barometric pressure between 29.309 and 30.197 inches. The average rise of 0.5036 of an inch

*Increased* the air expired 35.746 cubic inches per minute.

" " number of respirations 0.74 " " 

" " pulse 1.30 " " 

Dr. Hutchinson found that in a mine 1488 feet deep, where the pressure was 1.54 inch more than at the sea-level, the respiration was increased 2.4 per minute, and the pulse 1.3 per minute. (Cyc. of Anat. et Physiol. [art. Respiration], vol. iv. pp. 348, 349. Lond. 1832.)

Analogous effects are produced by *temperature.* In the famous experiments of Séguin and Lavoisier, at 82° F., the former (fasting and at rest) consumed 1210 French cubic inches of oxygen per hour; whereas at 57°, he consumed 1344 cubic inches per hour. (Mémoires de l'Acad. Royale for 1789.) Dr. Crawford found that a guinea-pig at 55°-5 F. abstracted twice as much oxygen from the air as at 104° F. (Experiments and Observations on Animal Heat, 2nd edit. p. 311-315. Lond. 1788.) Dr. W. F. Edwards found that birds consume more oxygen in winter than in summer. (De l'Influence des Agens Physiques sur la Vie, chap. 6. p. 195. Paris, 1824.)

The best experiments are those of M. Vierordt. (Op. cit. supra.) He obtained the following results between 37°-4 and 75°-2 F.:

<table>
<thead>
<tr>
<th>Pulse per minute</th>
<th>Average temp. 47°-24</th>
<th>Average temp. 66°-92</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Respiration per minute</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Volume of air expelled per minute in cubic inches.</td>
<td>407.00</td>
<td>367.00</td>
<td>40.00</td>
</tr>
<tr>
<td>&quot; &quot; CO²</td>
<td>18:25</td>
<td>15:72</td>
<td>2:53</td>
</tr>
</tbody>
</table>

M. Felix Letellier's experiments on warm-blooded animals confirm these results. He found the amount of *carbonic acid* evolved per hour at different temperatures to be as follows:

<table>
<thead>
<tr>
<th></th>
<th>85° to 104° F.</th>
<th>59° to 65° F.</th>
<th>32° F.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Canary</td>
<td>0.129 grm.</td>
<td>0.250 grm.</td>
<td>0.325 grm.</td>
</tr>
<tr>
<td>Pigeon</td>
<td>0.366</td>
<td>0.684</td>
<td>0.974</td>
</tr>
<tr>
<td>Two Mice</td>
<td>0.268</td>
<td>0.498</td>
<td>0.531</td>
</tr>
<tr>
<td>Guinea Pig</td>
<td>1.453</td>
<td>2.080 grms.</td>
<td>3.006 grms.</td>
</tr>
</tbody>
</table>

(Vide Comptes Rendus, vol. xx. p. 795. Paris, 1845. Also Ann. de Chim. et de Phys. 3rd series, vol. xiii. p. 478. Paris, 1845.) Doubtless physiological reactions exercise a powerful influence over the results of such experiments; nevertheless, as respiration is essentially a process of combustion, they have a general bearing on the question under consideration.
On the Separation of Nickel and Cobalt from Manganese. 197

sions:—1st, that solar light does not seem to exercise any sensible influence on the process of combustion; and 2ndly, that variations in the density of the air do exert a striking effect in retarding or accelerating the rapidity of the process, the rate of burning augmenting with every increment of density, and vice versa; but the exact ratio between them remains to be determined.

Columbia, S.C., June 1857.

XXI. On the Separation of Nickel and Cobalt from Manganese.
By T. H. Henry, Esq., F.R.S.*

The methods given by the best authorities for the separation of nickel and cobalt from manganese are either inconvenient or inaccurate. The only method which affords exact results is that of Ebelen, in which the sulphides formed at a high temperature are acted upon by dilute hydrochloric acid; but it is both tedious and unpleasant to pass sulphuretted hydrogen over the oxides contained in a porcelain tray in a porcelain tube at a red heat; and the modification suggested by Wohler, of converting the oxides into sulphides by fusing them with sulphur and carbonate of soda, still furnishes the sulphide of nickel in such a state that it is slightly acted upon by very dilute hydrochloric acid.

I have obtained very accurate results by a process differing altogether from those described, but as simple as any.

When chloride of ammonium and ammonia are added to a warm solution of sulphate of manganese or chloride of manganese, and afterwards phosphoric acid till the precipitation ceases, the whole of the manganese is precipitated; and after filtration no precipitate or even cloudiness is produced by the addition of sulphide of ammonium to the solution. The salt formed is, according to Otto, NH₄O₂MnO₃PO₄ + 2HO, and after ignition 2MnO + PO₄. When a solution of chloride or sulphate of nickel is treated in a similar manner, no precipitate occurs, even on standing a few days in a vessel lightly covered, when sufficient chloride of ammonium and free ammonia are present.

The following example will show the mode by which I operate.

12.6 grs. of pure sulphate of manganese, MnO SO₄ + 4HO, were gently ignited, and weighed 8.73 grs. = 4.11 MnO. 6 grs. of oxide of nickel, containing a trace of silica, were ignited and weighed then 5.63 grs.; these were dissolved together in hydrochloric acid and water, and the solution diluted; it was made acid with hydrochloric acid, phosphoric acid was added, and the whole heated until nearly boiling: when ammonia was added in

* Communicated by the Author.
excess, a white bulky precipitate was produced which rapidly contracted in volume and became crystalline; after standing twelve hours it was filtered and washed with a solution of chloride of ammonium and ammonia (the chloride of ammonium is absolutely necessary). The precipitate was perfectly white, and remained so after ignition, when it weighed 8·67 grs. = 4·33 MnO; it was decomposed and examined, it did not contain a trace of nickel.

The ammoniacal solution was treated by sulphuretted hydrogen, and the sulphide converted into oxide by roasting with a little carbonate of ammonia. It weighed 5·57 grs.

<table>
<thead>
<tr>
<th>Taken.</th>
<th>Obtained.</th>
</tr>
</thead>
<tbody>
<tr>
<td>MnO</td>
<td>4·11</td>
</tr>
<tr>
<td>NiO</td>
<td>5·63</td>
</tr>
</tbody>
</table>

I probably drove off a little sulphuric acid on igniting the sulphate of manganese. I have obtained more accurate results with the manganese since.

In operating with cobalt and manganese in the same manner, I obtained a slight excess of manganese, and the salt was slightly pink, but on repeating the operation the separation was complete. I took 3·22 grs. of oxide of manganese, and obtained 3·15 grs. free from cobalt.

---

XXII. On the Geometry of the Elliptic Equation.

By Charles W. Merrifield, Esq. *

It is now some years since Mr. John Riddle (of the Nautical School, Greenwich Hospital) remarked, that if circles on a sphere be projected on Mercator's chart, their projections are rectified by the elliptic integral

$$\int (1 - \sin^2 \theta \sin^2 \phi)^{-1/2} d\phi.$$

This observation has led me to a geometrical exhibition of the elliptic equation

$$\cos \phi_0 = \cos \phi_1 \cos \phi_2 + \sin \phi_1 \sin \phi_2 (1 - \sin^2 \theta \sin^2 \phi)^{1/2},$$

and at the same time of its corresponding transcendental equation

$$F(\theta, \phi_0) = F(\theta, \phi_2) - F(\theta, \phi_1),$$

which seems to me to be simpler than anything which has yet been given on the subject. My investigation has also presented me with Lagrange's modular scale, as the mere consequence of passing from the pole or centre of the sphere to the circumference of the circle in the stereographic projection.

* Communicated by the Author.
With great geometric simplicity, my method has the disadvantage of using a transcendental ordinate; but the only other that I know, has less simplicity—I mean Dr. Booth's spherical parabola, which primarily belongs to the integral of the third class, \( \int \frac{d\phi}{(1-k \sin^2 \phi)(1-k^2 \sin^2 \phi)^{1/2}} \), and requires reduction to pass to the simpler integral \( F(\theta, \phi) \).

As a particular case, I have discussed the integral \( \int \frac{d\phi}{\cos^2 \phi} \) in a way free from some of the complexities attending the trigonometry of the parabola, which relates more directly to the integral \( \int \frac{d\phi}{\cos^3 \phi} \).

For the purposes of Mercator's chart, circles divide themselves into three sorts. Let \( \alpha \) be the distance of the centre from (say) the north pole, and \( \beta \) the radius, \( \alpha \) and \( \beta \) being both arcs of great circles. Let the meridian of reference be the one passing through the centre.

(1) If \( \alpha < \beta \). The circle has one pole within and one pole without it. Of these the great circle is the type. Let \( \frac{\sin \alpha}{\sin \beta} = \sin \theta \) be called the modulus; then it is evident that the pole is a centre of similitude to all circles having the same modulus, and, from the principle of Mercator's chart, their projections will all be similar and equal, each being symmetric to a parallel of latitude \( \mu \), such that \( \sin \mu = \frac{\cos \beta}{\cos \alpha} \). This we shall call the mean latitude.

(2) \( \alpha = \beta \), a critical case. In this case the circle passes through a pole. These curves have the same simplicity on the chart that great circles have on the sphere, or right lines on the plane. If \( s \) be the arc and \( \phi \) the longitude, both measured from the meridian of the centre, \( ds = \frac{d\phi}{\cos \phi} \), whence \( s = \log \tan \left( \frac{\pi}{4} + \frac{\phi}{2} \right) \), the common formula for meridional parts. We shall therefore call its projection on Mercator's chart the meridional curve.

(3) \( \alpha > \beta \). In this case the circle is necessarily a small one, and does not include a pole. If \( \frac{\sin \beta}{\sin \alpha} = \sin \theta \), and \( \frac{\cos \alpha}{\cos \beta} = \sin \mu \), it will be projected into an oval divided symmetrically by the parallel of \( \mu \), and touched by the meridians of \( \theta \) and \( -\theta \). Evidently, provided \( \theta \) is constant, the curves will be similar and equal.

If \( \phi \) be the current longitude and \( \lambda \) the latitude, the formula
of rectification of a curve in the projection is

\[ ds^2 = d\phi^2 + \left( \frac{d\lambda}{\cos \lambda} \right)^2. \]

(1) In the first case, taking for simplicity the case of the great circle (in which \( \beta = \frac{\pi}{2} \) and \( \mu = 0 \)), the complement of the longitude \( \left( \frac{\pi}{2} - \phi \right) \), and the latitude \( \lambda \), are the sides of a right-angled triangle. The great circle itself forms the hypothenuse, and \( \theta \) (the declination or modulus) is the angle opposite \( \lambda \). Therefore \( \tan \lambda = \tan \theta \cos \phi \), whence we easily obtain

\[ \frac{ds}{d\phi} = (1 - \sin^2 \theta \sin^2 \phi)^{-\frac{1}{2}}. \]

If we change the variable to \( \tau \), the angle which the circle makes with the parallel, we have \( \sin \tau = \sin \theta \sin \phi \), and consequently

\[ \frac{ds}{d\tau} = (\sin^2 \theta - \sin^2 \tau)^{-\frac{1}{2}}. \]

(3) In the third case, we take for simplicity the case where \( \alpha = \frac{\pi}{2} \), and therefore \( \mu = 0 \). The oval is symmetrical to the equator, and the radius of the circle is \( \theta \). The radius \( \theta \) is the hypothenuse of a right-angled triangle, of which the latitude and longitude are the other sides. Therefore \( \cos \theta = \cos \lambda \cos \phi \), which gives

\[ \frac{ds}{d\phi} = \frac{\sin \theta}{(\sin^2 \theta - \sin^2 \phi)^{\frac{1}{2}}}. \]

If we change the variable to \( \tau \), the angle which the circle makes with the parallel, we have

\[ \sin \tau = \frac{\sin \phi}{\sin \theta} \quad \text{and} \quad \frac{ds}{d\tau} = \frac{\sin \theta}{(1 - \sin^2 \theta \sin^2 \tau)^{\frac{1}{2}}}. \]

The inverse correlation between these cases saves us from the necessity of considering more than one of them. We shall choose the first, because it is not encumbered with the constant factor \( \sin \theta \).

(2) The second case may be derived from either the first or the third by making \( \sin \theta = 1 \), or \( \sin \alpha = \sin \beta \). The mean latitude merges in the pole, since \( \frac{\cos \beta}{\cos \alpha} = \pm 1 \). In this critical case, therefore, the mean latitude and modulus do not determine the circle. We shall revert to this hereafter.
Returning therefore to the first case in which \( \frac{\sin \alpha}{\sin \beta} = \sin \theta \), let us project the circle from the south pole stereographically on the plane of the equator. Let \( m \) be the distance between the centres of the sphere and of the stereographic circle, and \( n \) the radius of the stereographic or subcontrary circle. We have at once

\[
\begin{align*}
  m + n &= \tan \frac{1}{2}(\alpha + \beta) = \frac{\sin \alpha + \sin \beta}{\cos \alpha + \cos \beta}, \\
  m - n &= \tan \frac{1}{2}(\alpha - \beta) = \frac{\sin \alpha - \sin \beta}{\cos \alpha + \cos \beta'}, \\
  \therefore m &= \frac{\sin \alpha}{\cos \alpha + \cos \beta'}, \quad n = \frac{\sin \beta}{\cos \alpha + \cos \beta'}
\end{align*}
\]

whence

\[
\frac{m}{n} = \sin \theta.
\]

The centre of the sphere is therefore the centre of similitude to the family of circles subcontrary to those on the sphere whose modulus is \( \sin \theta \). Indeed, it is evident that the axis of the sphere is an axis of similitude to the family of stereographic cones having the family of circles for their bases.

Let \( 2\psi \) be the \( \frac{\text{arc radius}}{\text{of the subcontrary circle intercepted by the meridian planes of longitude zero and } \phi} \). In the annexed figure, \( O \) is the centre of the circle, \( C \) of the sphere, \( M C P \) is the longitude, \( MOP = 2\psi \). We have

\[
\tan \phi = \frac{PM}{MC} = \frac{n \sin 2\psi}{n \cos 2\psi + m} = \frac{\sin 2\psi}{\cos 2\psi + \sin \theta'},
\]

If we change the variable from \( \phi \) to \( \psi \) in the expression

\[
\frac{d\phi}{(1 - \sin^2 \theta \sin^2 \phi)^{\frac{1}{2}}} = \frac{2}{1 + \sin \theta} \left\{ \frac{d\psi}{(1 + \sin \theta)^2 \sin^2 \psi} \right\}^{\frac{1}{2}},
\]
Now the modulus \[ \frac{2 \sqrt{\sin \theta}}{1 + \sin \theta} \] is the modulus next following \( \sin \theta \) in Lagrange’s scale (ascending), and the equation of the amplitudes, \( \tan \phi = \frac{\sin 2\psi}{\cos 2\psi + \sin \phi'} \), is only another form of
\[ \sin (2\psi - \phi) = \sin \theta \sin \phi, \]
the well-known equation of Lagrange, which may also be put in the form
\[ \tan (\phi - \psi) = \frac{1 - \sin \theta}{1 + \sin \theta} \tan \psi = \cos \theta \tan \psi. \]

The reader will not fail to notice the important relation
\[ \psi = \frac{1}{2} (\phi + \tau), \]
which follows from the equation, as well as from the geometry of the figure.

In the annexed figure, let the radius \( CA \) of the outer circle \( M_0 M_1 M_2 \) be unity, the radius \( DA \) of the inner circle \( r \), and the distance between the centres \( CD = e \).

Also let \( AM_0 \) and \( M_1 M_2 \) be tangents to the inner circle, and let the arcs
\[ \widehat{AM_0} = 2\psi_0, \quad \widehat{AM_1} = 2\psi_1, \quad \widehat{AM_2} = 2\psi_2. \]

Jacobi has shown that these three arcs will fulfill the elliptic equation
\[ \cos \psi_0 = \cos \psi_1 \cos \psi_2 + \sin \psi_1 \sin \psi_2 \Delta (\theta' \psi_0), \]
provided
\[ e = \frac{1 - \Delta \psi_0}{1 + \Delta \psi_0}, \quad r = (1 + e) \cos \psi_0 = \frac{2 \cos \psi_0}{1 + \Delta \psi_0}, \]
\[ \therefore r^2 = (1 + e)^2 - \frac{4e}{\sin^2 \theta}, \]
where
\[ \Delta \psi_0 = (1 - \sin^2 \theta' \sin^2 \psi_0)^{1/2}. \]
The proof may be found in Legendre's *Fonctions Elliptiques*, vol. iii. p. 174. Curiously enough, the enunciation is confined to the multiplication of the functions, while the proof is perfectly general. Whether its generality escaped the notice of Legendre and Jacobi I am uncertain.

It must be remarked, that when \( \psi_0 \) is an odd multiple of \( \frac{\pi}{2} \), we have \( r = 0 \), and \( e = \frac{1 - \cos \theta'}{1 + \cos \theta'} = \sin \theta \); but in no other case. The variation of \( \psi_0 \) does not yield a family of circles having a common centre of similitude.

It should also be noticed that the theorem is one-sided: it will not do to start from the other end \( B \) of the diameter.

Now let us suppose the outer circle of this diagram to be a circle of the stereographic projection, the distance between its centre and that of the sphere being \( \sin \theta \); then if the longitudes of the points \( M_0, M_1, M_2 \) be \( \phi_0, \phi_1, \phi_2 \), the equation

\[
F(\theta, \phi_2) - F(\theta, \phi_1) = F(\theta, \phi_0),
\]

and consequently the elliptic equation

\[
\cos \phi_0 = \cos \phi_1 \cos \phi_2 + \sin \phi_1 \sin \phi_2 (1 - \sin^2 \theta \sin^2 \phi)^{1/2}
\]

must also hold; since in \( \frac{d\phi}{\Delta(\theta, \phi)} = k \frac{d\psi}{\Delta(\theta', \psi')} \) the factor \( k \) is constant, and therefore the equation \( F(\theta, \phi_2) - F(\theta, \phi_1) = F(\theta, \phi_0) \) is merely the equation \( F(\theta', \psi_2) - F(\theta', \psi_1) = F(\theta', \psi_0) \), with each term multiplied by a constant factor.

To fix our ideas, it will be as well to exhibit this theorem by a diagram. As before, let \( \overline{AM_0} = 2\psi_0, \overline{AM_1} = 2\psi_1, \overline{AM_2} = 2\psi_2 \).

![Diagram](image)

Then the angles \( \Lambda OM_0 = \phi_0, \Lambda OM_1 = \phi_1, \Lambda OM_2 = \phi_2 \); and, as before,

\[
CD = \frac{1 - \Delta(\theta', \psi_0)}{1 + \Delta(\theta', \psi_0)} \quad \text{and} \quad r = (1 + e) \cos \psi_0 = \frac{2 \cos \psi_0}{1 + \Delta(\theta', \psi_0)}.
\]
Moreover \( CO = \sin \theta = \frac{1 - \cos \theta}{1 + \cos \theta} \), which is the value of \( e \) when \( \psi_0 = \frac{\pi}{2} \). With these conditions, we have between \( \phi_0, \phi_1, \phi_2 \) the equations

\[
F(\theta, \phi_0) = F(\theta, \phi_1) - F(\theta, \phi_2),
\]

\[
\cos \phi_0 = \cos \phi_1 \cos \phi_2 + \sin \phi_1 \sin \phi_2 \Delta(\theta, \phi_0).
\]

We may remark that this theorem has not the one-sidedness of Jacobi's. Indeed, as the angle \( \phi \) corresponds to \( 2\psi \) (*twice* \( \psi \)), \( \phi \) may be measured either from \( A \) or from \( B \).

The last observation enables us to determine the auxiliary circle without the help of Lagrange's scale. Draw \( PQ, p q \), through \( a \) and \( b \) perpendicular to \( AB \), then

\[
POA = AOQ = POB = BOq = \phi_2^1,
\]

by which we understand an amplitude such that \( F(\phi_2^1) = \frac{1}{2} F(\phi) \).

If therefore we suppose \( \phi_2^1 \) to be known = \( \omega \), it is easy to determine the auxiliary circle by marking off the points \( a \) and \( b \), and thence determining the other elements.

Now suppose \( O \) to be the centre of the sphere, and let us suppose the last figure to be a stereographic projection from the south pole on the plane of the equator. The longitudes will still remain unaltered. The circles will still remain circles. The straight lines passing through \( O \) will be meridians, and the tangent lines will be small circles passing through the south pole.

If we pass to Mercator's chart, the tangent lines become *meridional curves*, and the arc \( AM_0 = M_1 M_2 \) in actual measurement on the chart. This is the geometric meaning of the transcendental equation \( F(\theta, \phi_0) = F(\theta, \phi_2) - F(\theta, \phi_1) \).

We have still to determine the elements of the auxiliary circle from the value (supposed to be known) of \( \omega \) or \( \phi_2^1 \). To do this we shall return to the surface of the sphere. Here \( PQS \) is the circle which replaces the tangent line \( PQ \); \( PNS \) is the longitude \( \omega \), \( PAQ \) is the circle whose modulus is \( \theta \); and the auxiliary circle therefore passes through \( a \).

Let \( G \) be the centre of \( PSQ \). Join \( GP \), and draw \( GH \perp SP \);
the Elliptic Equation.

\[ SH = HP. \]  
Now in the right-angled triangle S G H we have the angle \( S = \omega \), and the side \( SH = \frac{\pi}{4} + \frac{\lambda}{2} \) (the latitude of \( P \) being \( \lambda \)). The hypothenuse SG is therefore found by the equation \( \tan SG \cdot \cos \omega = \tan \left( \frac{\pi}{4} + \frac{\lambda}{2} \right) \). If we call \( \gamma \) the latitude of \( a \), we have \( SG = \frac{\pi}{4} + \frac{\gamma}{2} \) and therefore

\[
\tan \left( \frac{\pi}{4} - \frac{\gamma}{2} \right) = \cos \omega \cdot \tan \left( \frac{\pi}{4} - \frac{\lambda}{2} \right).
\]

The arc \( \gamma \) has two values, depending on the double value of \( \lambda \). It must also be noted that there are two auxiliary circles, according to whether the tangent circles pass through the north or south poles. One is easily found from the other by putting \( \pi - \beta \) for \( \beta \).

The equation which gives \( \lambda \) is easily found to be (since \( \lambda \) is one side of the triangle of which \( \alpha \) and \( \beta \) are the others, and \( \omega \) is the angle opposite \( \beta \))

\[
\cos \alpha \sin \lambda \pm \cos \omega \sin \alpha \cos \lambda = \cos \beta.
\]

This gives

\[
\tan \left( \frac{\pi}{4} - \frac{\lambda}{2} \right) = \frac{1}{\cos \alpha + \cos \beta} \left\{ \sqrt{\sin^2 \beta - \sin^2 \alpha \sin^2 \omega} \pm \sin \alpha \cos \omega \right\},
\]

whence

\[
\tan \left( \frac{\pi}{4} - \frac{\gamma}{2} \right) = \frac{\cos \omega}{\cos \alpha + \cos \beta} \left\{ \sqrt{\sin^2 \beta - \sin^2 \alpha \sin^2 \omega} \pm \sin \alpha \cos \omega \right\}.
\]

This form of the equation is very inconvenient.

Knowing the meridian on which the centre of a circle lies, the circle is completely determined by its modulus \( \theta \) and its mean latitude \( \mu \), where

\[
\sin \theta = \frac{\sin \alpha}{\sin \beta}, \quad \sin \mu = \frac{\cos \beta}{\cos \alpha};
\]

and \( \therefore \cos \theta \cdot \tan \mu \cdot \tan \beta = 1, \quad \tan \alpha = \tan \theta \cos \mu. \)

In the auxiliary circle, let \( p \) be the polar distance of its centre, \( q \) its radius, \( q \) its mean latitude, and \( \xi \) its modulus; then we have

\[
\tan \left( \frac{\pi}{4} - \frac{\gamma}{2} \right) = \tan \frac{1}{2} (q \pm p),
\]

\[
\therefore \tan \frac{1}{2} (q \pm p) = \frac{\cos \omega}{\cos \alpha + \cos \beta} \left\{ \sqrt{\sin^2 \beta - \sin^2 \alpha \cdot \sin^2 \omega} \pm \sin \alpha \cdot \cos \omega \right\},
\]

\[
\therefore \tan \frac{1}{2} (q + p) \cdot \tan \frac{1}{2} (q - p) = \frac{\cos^2 \omega}{(\cos \alpha + \cos \beta)^2} (\sin^2 \beta - \sin^2 \alpha)
\]

\[
= \cos^2 \omega \cdot \tan \frac{1}{2} (\beta + \alpha) \cdot \tan \frac{1}{2} (\beta - \alpha); \]

\( \therefore \)
but
\[
\tan \frac{1}{2} (q + p) \cdot \tan \frac{1}{2} (q - p) = \frac{\cos p - \cos q}{\cos p + \cos q} = \frac{1 - \sin \eta}{1 + \sin \eta} = \tan^2 \left( \frac{\pi}{4} - \frac{\eta}{2} \right);
\]
and, similarly,
\[
\tan \frac{1}{2} (\beta + \alpha) \cdot \tan \frac{1}{2} (\beta - \alpha) = \tan^2 \left( \frac{\pi}{4} - \frac{\mu}{2} \right).
\]

Hence
\[
\tan \left( \frac{\pi}{4} - \frac{\eta}{2} \right) = \cos \omega \cdot \tan \left( \frac{\pi}{4} - \frac{\mu}{2} \right).
\]

This determines the mean latitude of the auxiliary circle from the mean latitude of the given circle very simply. To find the modulus, we have
\[
\sin \xi = \frac{\sin \omega}{\sin \eta} = \frac{\tan \frac{1}{2} (q + p) - \tan \frac{1}{2} (q - p)}{\tan \frac{1}{2} (q + p) + \tan \frac{1}{2} (q - p)} = \frac{\sin \alpha \cdot \cos \omega}{\sin \theta \cdot \cos \omega} = \frac{\sqrt{\sin^2 \beta - \sin^2 \alpha \cdot \sin^2 \omega}}{\sqrt{1 - \sin^2 \theta \cdot \sin^2 \omega}},
\]
whence we may obtain
\[
\tan \xi = \cos \omega \cdot \tan \theta.
\]

If we reckon \( \eta \) and \( \mu \) from one pole, but \( q \) and \( \beta \) from the other pole, \( \tan \left( \frac{\pi}{4} - \frac{\eta}{2} \right) \) becomes \( -\tan \left( \frac{\pi}{4} + \frac{\eta}{2} \right) \). The equation of the mean latitudes, therefore, in its complete form is
\[
\tan \left( \frac{\pi}{4} + \frac{\eta}{2} \right) = \cos \omega \cdot \tan \left( \frac{\pi}{4} + \frac{\mu}{2} \right).
\]

That for the modulus remains, without ambiguity,
\[
\tan \xi = \cos \omega \cdot \tan \theta.
\]

These formulæ remain unaltered for the third case, except that \( \omega \) is to be taken so that \( F(\theta, \omega) = \frac{1}{2} F(\theta, \tau) \) instead of \( \frac{1}{2} F(\theta, \phi) \).

In order to get the value of \( \omega \) from \( \phi \), we have only to equate \( \phi_0 \) and \( \phi_1 \) to \( \omega \) in the elliptic equations
\[
\cos \phi_0 = \cos \phi_1 \cos \phi_2 + \sin \phi_1 \sin \phi_2 \Delta \phi_0,
\]
\[
\cos \phi_2 = \cos \phi_0 \cos \phi_1 - \sin \phi_0 \sin \phi_1 \Delta \phi_1.
\]

The first gives
\[
\frac{1 - \cos \phi}{\sin \phi} = \tan \frac{1}{2} \phi = \tan \omega \Delta (\theta, \omega);
\]
the second gives
\[
\sin^2 \omega = \frac{1 - \cos \phi}{1 + \Delta \phi}, \quad \cos^2 \omega = \frac{\Delta \phi + \cos \phi}{1 + \Delta \phi}.
\]

If, as before, we put \( \sin \tau = \sin \theta \sin \phi \), we have \( \Delta \phi = \cos \tau \);
the Elliptic Equation.

207

\[ \sin \omega = \frac{\sin \frac{1}{2} \phi}{\cos \frac{1}{2} \tau} \quad (\cos \omega)^2 = \frac{\cos \frac{1}{2} (\phi + \tau) \cdot \cos \frac{1}{2} (\phi - \tau)}{(\cos \frac{1}{2} \tau)^2}; \]

and by combining these with the value of \( \tan \frac{1}{2} \phi \), we have

\[ (1 - \sin^2 \theta \sin^2 \omega) = \frac{\cos \frac{1}{2} (\phi + \tau) \cdot \cos \frac{1}{2} (\phi - \tau)}{(\cos \frac{1}{2} \phi)^2}. \]

We may also put the equation between the moduli under the more succinct form

\[ \sin \xi = \frac{\cos \frac{1}{2} \phi}{\cos \frac{1}{2} \tau} \cdot \sin \theta. \]

I now proceed to a discussion of the critical case, in which \( \alpha = \beta \) or the circle passes through a pole, and its projection is the meridional curve.

One remarkable property is derived from the equation \( \sin \tau = \sin \theta \sin \phi \). Since \( \sin \theta = 1 \) in the critical case, the angle which the curve makes with the parallel is always the same as the longitude measured from its axis. By the term axis I mean the meridian passing through the centre of the circle.

The angle \( \omega \) is therefore obtained more simply from \( \phi \). In fact, the equation \( \sin \omega = \frac{\sin \frac{1}{2} \phi}{\cos \frac{1}{2} \tau} \) becomes \( \sin \omega = \tan \frac{1}{2} \phi \), whence

\[ (\cos \omega)^2 = \frac{\cos \phi}{(\cos \frac{1}{2} \phi)^2}. \]

It is evident that my theorem for the comparison of elliptic amplitudes applies also to the meridional curve; but since the modulus is constant, and the mean latitude nugatory, in this case, we must make \( \alpha = \beta \) in the expression for the latitude where the curve crosses the axis. This gives

\[ \tan \left( \frac{\pi}{4} - \frac{\gamma}{2} \right) = \tan \alpha \cdot \cos \omega, \text{ or } \tan \rho = \tan \alpha \cdot \cos \omega, \]

since the auxiliary circle also passes through the pole.

In the stereographic projection, the circle passes through the centre of the sphere, and its polar equation is \( \rho = 2 \alpha \cdot \cos \phi \). Now \( \pi - 2\alpha \) is the latitude of the point opposite the pole; and if \( \lambda \) be the current latitude, we have \( 2\alpha = \tan \alpha \) and \( \rho = \tan \left( \frac{\pi}{4} \pm \frac{\lambda}{2} \right) \), whence \( \tan \left( \frac{\pi}{4} \pm \frac{\lambda}{2} \right) = \tan \alpha \cos \phi \). Taking the Napierian logarithm, we have for the equation on the chart,

\[ \log \tan \left( \frac{\pi}{4} \pm \frac{\lambda}{2} \right) = \log \tan \alpha + \log \cos \phi, \]

or

\[ \pm \log \tan \left( \frac{\pi}{4} + \frac{\lambda}{2} \right) = \log \tan \alpha + \log \cos \phi. \]
Now \( \log \tan \left( \frac{\pi}{4} + \frac{\lambda}{2} \right) \) is the ordinate of the chart. The only constant the equation contains is additive. It is evident, therefore, that so long as the scale of the chart is unaltered, the curve does not change either size or direction, but simply shifts north or south as its extreme latitude alters, or east and west as we shift its axis. If then we cut a pattern for any given chart, we can apply that pattern as easily as we can the ruler in plane geometry, merely keeping the axis in the meridian. The geometrical simplicity of Mercator's chart thus appears in a new and very singular light.

The easiest way to draw the curve is to find the ordinate, on the supposition that the curve touches the equator. In this case \( \alpha = \frac{\pi}{4} \), and therefore \( \log \tan \alpha = 0 \). The negative value of \( \log \cos \phi \) merely means that the ordinate must be measured towards the pole through which the curve passes.

Supposing the chart to be developed into a plane, the curve in its general form very nearly resembles the catenary. It has two asymptotes, the meridians of \( \pm \frac{\pi}{2} \).

The function \( \log \tan \left( \frac{\pi}{4} + \frac{\phi}{2} \right) = \int \frac{d\phi}{\cos \phi} \) is one of the most remarkable that occurs in analysis. It fulfils the functional equation \( \sqrt{-1} \cdot f(u) = f(u \sqrt{-1}) \). This property was, I believe, first remarked by Baron Maseres, although I suppose that the old gentleman, who had a horror of the negative sign, especially when standing by itself under the radical, would be very much shocked at the above expression of it. His observation was, that if \( u \) be the function, and \( \phi \) the variable,

\[
\begin{align*}
u &= \phi + \frac{\phi^3}{6} + \frac{\phi^5}{24} + \frac{61\phi^7}{5040} + \frac{277\phi^9}{72576} + \cdots \\
\phi &= u - \frac{u^3}{6} + \frac{u^5}{24} - \frac{61u^7}{5040} + \frac{277u^9}{72576} - \cdots
\end{align*}
\]

the coefficients being numerically the same for both series; but all positive in the one, and alternately positive and negative in the other.

With regard to Lagrange's scale of moduli, it should be remembered that in this case, as in the other extreme of the circular arc (where \( \theta = 0 \)), the application of the scale alters neither modulus nor amplitude.

In applying my theorem to this case, it is obvious that there is only one auxiliary circle, and that it and the tangent circle pass through opposite poles. Any two meridional curves, of
opposite poles, which cut each other, cut off equal and similar portions from each other. In the critical case, the meridional arc, which corresponds to the intercepted tangent in the stereographic projection, is therefore also constant.

The following Table contains all the elements necessary for tracing the meridional curve. The third column contains the meridional parts corresponding to the latitude; and the fourth the actual value of log, cos φ.

<table>
<thead>
<tr>
<th>Longitude</th>
<th>Latitude</th>
<th>Meridional parts</th>
<th>Hyp. log cos of longitude</th>
<th>Longitude</th>
<th>Latitude</th>
<th>Meridional parts</th>
<th>Hyp. log cos of longitude</th>
</tr>
</thead>
<tbody>
<tr>
<td>5</td>
<td>13 6</td>
<td>13</td>
<td>0.003813</td>
<td>50</td>
<td>24 32 7</td>
<td>1319</td>
<td>0.441941</td>
</tr>
<tr>
<td>10</td>
<td>52 58</td>
<td>53</td>
<td>0.015309</td>
<td>55</td>
<td>30 19 30</td>
<td>1911</td>
<td>0.555864</td>
</tr>
<tr>
<td>15</td>
<td>1 59 10</td>
<td>119</td>
<td>0.045668</td>
<td>60</td>
<td>36 52 12</td>
<td>2388</td>
<td>0.693147</td>
</tr>
<tr>
<td>20</td>
<td>3 33 42</td>
<td>214</td>
<td>0.062202</td>
<td>65</td>
<td>41 10 50</td>
<td>2961</td>
<td>0.861286</td>
</tr>
<tr>
<td>25</td>
<td>5 37 40</td>
<td>338</td>
<td>0.083766</td>
<td>70</td>
<td>52 14 12</td>
<td>3688</td>
<td>1.072886</td>
</tr>
<tr>
<td>30</td>
<td>8 12 48</td>
<td>494</td>
<td>0.143841</td>
<td>75</td>
<td>60 58 42</td>
<td>4647</td>
<td>1.351626</td>
</tr>
<tr>
<td>35</td>
<td>11 21 16</td>
<td>683</td>
<td>0.199486</td>
<td>80</td>
<td>70 17 52</td>
<td>6018</td>
<td>1.750724</td>
</tr>
<tr>
<td>40</td>
<td>15 5 34</td>
<td>916</td>
<td>0.266515</td>
<td>85</td>
<td>80 2 16</td>
<td>8388</td>
<td>2.440059</td>
</tr>
<tr>
<td>45</td>
<td>19 28 16</td>
<td>1191</td>
<td>0.346374</td>
<td>90</td>
<td>90 0 0</td>
<td>log*∞</td>
<td>log*∞</td>
</tr>
</tbody>
</table>

6 Brompton Grove,
August 12, 1858.

XXIII. Chemical Notices from Foreign Journals.

By E. Atkinson, Ph.D.

[Continued from p. 140.]

CONSIDERABLE attention has of late been given to the question of the atomic weights of the elements; and several important papers have been published upon this subject by Dumas, Pettenkofer, Marignac, and v. Haner.

Dumas* makes known, in a preliminary communication, the results of a general revision of the equivalent weights of the elements which he has undertaken, and on which he is still engaged. He also points out and discusses various important and interesting relations which exist between the elements. In this respect his views are a development of those expressed at the meeting of the British Association in 1851.

The majority of recent determinations of the elements have led to the confirmation of Prout's law that the equivalents of all elements are exact multiples of that of hydrogen. Two of the most important found exceptions, chlorine and copper; and although the equivalent of chlorine has been ascertained with

great accuracy, Dumas judged it not unnecessary to make a determination of it by a new method. This consisted in heating pure granulated silver in a tube in a stream of chlorine, and determining the equivalent from the difference between the original weight of the silver and that of the chloride formed. The tube was provided with a stopcock by which it could be exhausted, and a similar tube was used as a counterpoise. Three weighings were necessary:—1st, that of the exhausted tube; 2nd, of the exhausted tube containing the silver; 3rd, of the exhausted tube containing the chloride of silver formed. Various experiments were made with different weights of silver. Assuming the equivalent of silver at 108, as found by Marignac's very accurate determinations, the result of Dumas's experiments gave 35·5 as the equivalent of chlorine, which thus confirms the number at present adopted.

The determination of the equivalent of copper was made partly by the reduction of oxide of copper, and partly by converting copper into sulphide of copper, but the results were not so concordant as to give any certain datum. The number lies, however, between 31 and 32.

These numbers do not agree with Prout's law; and Dumas therefore concludes that that law must be accepted in the less general form, that the equivalents of simple bodies are almost all multiples by whole numbers of the equivalent of hydrogen, but that, with reference to chlorine, the unit with which it is comparable is half as great as that of hydrogen.

Dumas next discusses the question as to whether there are elements whose equivalents have the relation 1:1 or 1:2. Molybdenum and tungsten have been considered to stand in the latter relation; and the only question has been whether their equivalents were as 46:92 or as 47:94. Dumas determined the equivalent of molybdenum by heating molybdate acid in hydrogen. The molybdate acid was prepared in a pure form, and in beautiful large crystals, by heating sulphide of molybdenum in a stream of oxygen. The molybdate acid placed in an unglazed porcelain tray was heated in hydrogen, at first in a glass tube at a moderate temperature, and then the tray was transferred to an unglazed porcelain tube, and the reduction concluded at a very high temperature. The results gave 48 as the equivalent of molybdenum.

Great difficulties were experienced in determining the equivalent of tungsten. The readiness with which tungstic acid is reduced and forms lower degrees of oxidation, renders the preparation of pure tungstic acid very difficult. It was obtained by heating pure tungstate of ammonia in a flat porcelain dish, at first at a low temperature and then at a red heat, until all the
ammonia was expelled. A source of error in previous determinations consisted in using for the reduction vessels of platinum, with which the tungsten forms an alloy. Glazed porcelain also favours the formation of an oxide, which combines with the glaze, and remains unreduced by hydrogen. This was avoided by using for the reduction tubes and trays of unglazed porcelain. Another difficulty lies in the readiness with which tungstic acid is carried away in the aqueous vapour formed by the reduction. After all the sources of error had been avoided, results were obtained which gave the number 92 as the equivalent of tungsten. Hence the equivalents of molybdenum and tungsten do not stand in the relation 1:2; but an instance of this relation is found in the equivalents of oxygen and sulphur, which are as 8:16. The equivalent of sulphur has also been redetermined by Dumas, by passing the vapour of perfectly pure sulphur over pure silver heated to redness, and ascertaining the relation between the weight of the original silver and that of the sulphide formed. Previous determinations had given the numbers 16·06 and 16·10; Dumas finds that the equivalent is 16.

As instances of metals which have the same equivalent, Dumas adduces manganese and chrome, with the equivalents 26. He states that he has redetermined the equivalent of manganese by reducing perfectly pure artificial binoxide of manganese to protoxide by means of hydrogen. His experiments leave no doubt that the equivalent is 26.

With many groups of three elements it is the case that the equivalent of the middle is exactly the arithmetical mean of that of the extremes. For instance, sulphur and tellurium have respectively the equivalents 16 and 64; the mean of this is 40, which is very nearly the equivalent of selenium. Similar relations exist between calcium, strontium, and barium, and between lithium, sodium, and potassium. Nevertheless, with certain triads this is not the case. Chlorine, bromine, and iodine have the most marked analogies, not only in the physical and chemical properties of the elements themselves, but also in those of their compounds. Yet their equivalent weights stand in the relation 35·5:80:127; and in order to exhibit the numerical relation, it would be necessary to raise the atomic weight of bromine to 81, or to lower that of iodine to 124·5. Dumas has controlled the equivalents of bromine and of iodine by converting the bromide and the iodide of silver into the chloride of silver. The iodide of silver was obtained by converting pure iodine in large crystals into iodide of zinc, and this into iodide of silver. Bromine free from iodine was digested for some time with bromide of silver, to free it from a possible admixture of chlorine, and was then converted into bromide of silver.
Assuming 35.5 as the equivalent of chlorine, Dumas found for bromine exactly the number 80, and for iodine 127, thus confirming Marignac's numbers.

Hence the arithmetical relation may be wanting in elements which belong to the same family, and exhibit the greatest analogies.

In the fourth part of his paper Dumas considers the question as to whether the equivalent weights of chemical elements have any relation corresponding to that which prevails among the members of homologous series of organic radicals.

The ether radicals, methyle, æthyle, propyle, butyle, &c., have the equivalents—

| $\text{C}_2\text{H}_3$ | 15 | $\text{C}_{10}\text{H}_{11}$ | 71 |
| $\text{C}_4\text{H}_5$ | 29 | $\text{C}_{12}\text{H}_{13}$ | 85 |
| $\text{C}_6\text{H}_7$ | 43 | $\text{C}_{14}\text{H}_{15}$ | 99 |
| $\text{C}_8\text{H}_9$ | 57 | $\text{C}_{16}\text{H}_{17}$ | 113 |

The difference between the equivalents of two members of this series is 14, and their equivalents form an arithmetical series $a + nd$, in which the first member $a$ is 15, and the difference $d$ is 14.

Many of these exhibit extraordinary relations; and if we did not know the mode of their formation, we might believe that the equivalents 57 and 113, 127 and 253, &c. had the simple relation $1:2$. And if we did not know whether the equivalent of an element were 225 or 226, the certain determination of such a high equivalent would be beyond the means which chemistry has at its disposal. If capryle, whose equivalent is 113, and cetyle, whose equivalent is 225, do not stand in the relation $1:2$, although their radicals possess the closest analogies in their compounds, and obviously belong to one and the same chemical family, we cannot be surprised that the equivalents of molybdenum and tungsten should closely approach such a relation without ever actually reaching it. From these series also we might form a numerous series of triads.

But organic radicals are not always formed by the addition of atomic groups; they are also formed by substitution, as is especially the case with the compound ammonias. From ammonium, $\text{NH}_4$, a large number of compound ammonias are formed by the substitution of 1, 2, 3, or 4 equivalents of methyle, æthyle, propyle, &c., for 1, 2, 3, or 4 equivalents of hydrogen. The equivalents thus produced are just as if we added to ammonium, $\text{NH}_4$, 1, 2, 3, or 4 times $\text{C}_n\text{H}_n$. And the general formula for the compound ammonia molecules, which consist of ammonium $a$, and such hydrocarbons, would be
M. Dumas on the Equivalents.

\[ a + nd \]
\[ d' \]
\[ d'' \]
\[ d''' \]

&c.

in which \( n \) is a whole number, either 4 or less, and \( d, d', d'', d''' \)
the equivalents of the hydrocarbons of the series \( C_n H_n \).

We might have the series—

\[
\begin{align*}
  a & \quad a + d \\
  a + d' & \quad a + 2d \\
  a + 2d & \quad a + 3d \\
  a + 3d & \quad a + 4d \\
  a + 4d & \quad a + 3d + d'' \\
  a + 2d + d'' & \quad a + 3d + d'' \\
  a + 2d + 2d'' & \quad a + 2d + 2d'' \\
  a + 2d + 2d'' & \quad a + 3d + d'' \\
  a + 3d + d'' & \quad a + d + 3d' \\
  a + d + 3d' & \quad a + d + 3d' \\
  a + d + 3d' & \quad a + d + 3d' \\
  a + d + 3d' & \quad a + d + 3d' \\
\end{align*}
\]

or even

\[ a + d + d' + d'' + d''' \]

And there are other groups in which the first member itself changes, as well as the elements added to it. Tin and æthyle form six different groups, which have all the characters of organic radicals, and comport themselves quite like metals. If we call tin \( a \) and æthyle \( d' \), we may have the formulæ—

\[
\begin{align*}
  a + d' & \quad 2a + d' \\
  2a + d' & \quad 4a + d' \\
  2a + 3d' & \quad 4a + 3d' \\
  4a + 5d' & \quad 4a + 5d' \\
\end{align*}
\]

in which \( na + nd' \) is the general formula, and in which \( a \) and \( d' \)
may be repeated to a certain extent, and \( d' \) be replaced by an
equivalent radical \( d, d'', d''' \), &c.

Dumas then proceeds to apply these principles to the consider-
ation of inorganic compounds. With reference to the group
fluorine, chlorine, bromine, and iodine, the equivalent of the first
member, fluorine, has recently been determined by Dumas by
converting pure fluorides of sodium and potassium into sulphates
of soda and potash; and he has obtained the number 19. The
numbers 19, 35·5, 80 and 127, which appear to have no arith-
metical relation, belong to a group similar to those seen to exist
among organic radicals. If we call the equivalent of fluorine \( a \),
the difference between that and the equivalent of chlorine \( d \), and
a complementary difference, which is required in order to pass
from chlorine to bromine, \( d' \), we get for fluorine, chlorine, bro-
mine, and iodine,—

\[
\begin{align*}
  a & \quad a + d \\
  a + d & \quad a + 2d + d' \\
  a + 2d + d' & \quad 2a + 2d + 2d'' \\
\end{align*}
\]

or in numbers,—
M. Dumas on the Equivalents.

19 . . . . . . . . . . . . . . . . . . Fluorine.
19 + 16:5 = 35:5 . . . . . . . . . . . . . . . . . . . Chlorine.
19 + 33 + 28 = 80 . . . . . . . . . . . . . . . . . . Bromine.
38 + 33 + 56 = 127 . . . . . . . . . . . . . . . . . . Iodine.

For nitrogen, phosphorus, arsenic, antimony, and bismuth, we should have the formulae—

\[ a + d \]
\[ a + d + d' \]
\[ a + d + 2d' \]
\[ a + d + 4d' \]

or in numbers,

\[ 14 \] Nitrogen.
\[ 14 + 17 = 31 \] Phosphorus.
\[ 14 + 17 + 44 = 75 \] Arsenic.
\[ 14 + 17 + 88 = 119 \] Antimony.
\[ 14 + 17 + 176 = 207 \] Bismuth.

Carbon has the equivalent 6. Deville has recently determined that of boron by the analysis of chloride and bromide of boron, and has found it to be 11. The equivalent of silicon deduced from analyses of the chloride gave Dumas numbers between 21 and 21:2, which represent, however, a maximum, as the chloride, notwithstanding the greatest care taken in its preparation, always contained phosgene gas in solution. The number 21 may be taken as the right one. The equivalent of zirconium is 33. The numbers 6, 11, 21, 33 are expressed by the formula—

\[ a \]
\[ a + d \]
\[ a + 3d \]
\[ 3a + 3d \]

\[ 6 \] Carbon.
\[ 6 + 5 = 11 \] Boron.
\[ 6 + 15 = 21 \] Silicon.
\[ 18 + 15 = 33 \] Zirconium.

Oxygen, sulphur, selenium, and tellurium form another series, which may be represented either by the general formula—

\[ a \]
\[ 2a \]
\[ 5a \]
\[ 8a \]

or by

\[ a + d \]
\[ a + 4d \]
\[ a + 7d \]

The analogy of other groups would lead to the adoption of the latter.
M. Dumas on the Equivalents.

\[ 8 \quad \ldots \quad \ldots \quad \ldots \quad \text{Oxygen.} \]
\[ 8 + 8 = 16 \quad \ldots \quad \ldots \quad \text{Sulphur.} \]
\[ 8 + 32 = 40 \quad \ldots \quad \ldots \quad \text{Selenium.} \]
\[ 8 + 56 = 64 \quad \ldots \quad \ldots \quad \text{Tellurium.} \]

This difference of 8 or a multiple is shown in the group magnesium, calcium, strontium, barium, and lead: the general formula of the group is—

\[
a
a + d
a + 4d
a + 7d
2a + 10d
\]

\[ 12 \quad \ldots \quad \ldots \quad \ldots \quad \text{Magnesium.} \]
\[ 12 + 8 = 20 \quad \ldots \quad \text{Calcium.} \]
\[ 12 + 32 = 44 \quad \ldots \quad \text{Strontium.} \]
\[ 12 + 56 = 68 \quad \ldots \quad \text{Barium.} \]
\[ 24 + 80 = 104 \quad \ldots \quad \text{Lead.} \]

Lithium, sodium, and potassium belong to another series, with a difference of 16, \( a, a + d, a + 2d \).

\[ 7 \quad \ldots \quad \ldots \quad \ldots \quad \text{Lithium.} \]
\[ 7 + 16 = 23 \quad \ldots \quad \text{Sodium.} \]
\[ 7 + 32 = 39 \quad \ldots \quad \text{Potassium.} \]

Dumas has redetermined the atomic weight of tin, having employed Berzelius's method. Pure tin, expressly prepared from chloride of tin, was treated in a long-necked glass globe of the hardest glass with nitric acid. The residual oxide was then heated to redness in the flask for an hour. The experiment was made with every precaution, and the number was found to be 58-8; but the oxide was still found to contain traces of water, which was driven off by being heated in a platinum crucible. The correction for this raised the equivalent to 59.

The equivalents of titanium, tin, and tantalum form a series

\[ 25 \quad 59 \quad 92, \]

which has the difference 34. This difference also obtains between chromium and uranium, which have the equivalents 26 and 60. Chromium, molybdenum, vanadium, and tungsten form also a series in which the difference is 22,—

\[ 26 \quad 48 \quad 70 \quad 92 \]

These considerations lead to the confirmation of Prout's view, that the equivalents are multiples by whole numbers of a certain basis,—with the modification, however, that for certain elements this basis is not the equivalent of hydrogen, but probably 0.5.
The existence of series tends to show the close connexion between inorganic and organic chemistry; and the various series whose existence has been pointed out have this in common with the series of organic chemistry, that the first member conditions the chemical character of the succeeding members. The chemical character of methyle is seen in the successive radicals; and the chemical character of fluorine exhibits itself in chlorine, bromine and iodine; that of oxygen in sulphur, selenium and tellurium; and that of nitrogen in phosphorus, antimony and arsenic.

With the consideration of this subject, and with the determination of other equivalents, Dumas is still engaged; and these speculations will appear more important when he publishes the study of a natural family which is derived from hydrogen as the first member, and in which the physical properties of the chemical elements stand in close connexion with the place which each assumes in the series.

Pettenkofer* has republished a paper, read by him eight years ago before the Bavarian Academy of Sciences, in which he discusses the relations between the equivalent weights of the elements. The conclusions which he endeavoured to establish were, that the equivalents of the inorganic elements which form natural families or groups exhibit among themselves just as constant differences as do the equivalents of compound radicals which belong to one natural group. And simple inorganic elements may therefore be considered from the point of view of the compound organic radicals.

For the full development of his views it was necessary to have a complete revision of the elements, which at that time Pettenkofer was unable to undertake.

Marignac has made a redetermination of the atomic weights of barium, strontium, and lead. To previous determinations of the atomic weight of barium, in which anhydrous chloride of barium was used, the objection had been raised that it was impossible to dehydrate crystallized chloride of barium without at the same time driving off some chlorine; and in fact it was found that chloride of barium, kept for some time at a red heat, does become slightly alkaline. Without going into the question whether chloride of barium, which has not been raised to a temperature sufficient for a slight decomposition, does not still retain some traces of water, Marignac obviated it by employing in his determination crystallized chloride of barium, which could be obtained perfectly pure, of a definite composition, and which

* Liebig's Annalen, February 1858.
was unalterable in the air. For the mode of purifying this and the other salts used in the experiment, as well as for the precautions and corrections necessary in the experiment, the original memoir must be consulted*.

The method employed consisted in precipitating a known weight of chloride of barium by nitrate of silver, and then precipitating the barium as sulphate, and from the relation between the chloride of silver and the sulphate of barium thus formed, deducing the equivalent of barium. The result of these experiments gave the numbers 68·61, 68·59, and 68·55, or as the mean 68·58, which is almost the same as that formerly obtained, 68·57. And Marignac considers that the sources of error which cannot be eliminated are sufficiently large to justify the adoption of the number 68·5. The method used for the determination of the atomic weight of strontium was essentially the same. The results of experiments made with perfectly pure salts gave for the equivalent of strontium the numbers 43·77, 43·74, and 43·76. The mean of these, 43·76, is exactly the mean of the numbers 43·67 and 43·85 found by Berzelius and by Pelouze. The numbers hitherto adopted have been 43·5 and 44; but Marignac thinks it impossible that the errors of experiment would justify the adoption of either of these numbers. The equivalent of strontium is almost certainly 43·75; and if we wish to assume the existence of simple relations between equivalents, we must lower still further the unit which serves as their common measure; and having halved it for chlorine, and perhaps for barium, copper, and lead, we must again halve it for strontium. But if this common relation be no longer the actual equivalent of hydrogen, but a fraction of that equivalent, there is no reason why this fraction should be \( \frac{1}{2} \) rather than \( \frac{1}{4} \), or than any other smaller fraction, \( \frac{1}{10} \) for instance. That is to say, that this question of the existence of a unit, of which the weights of all chemical elements would be simple multiples, will always be incapable of solution by experiment.

The equivalent of lead was determined by Berzelius by the reduction of oxide of lead by hydrogen. He found the number 103·56. Marignac applied the method previously used for the determination of barium and strontium to this metal. He found the numbers 103·57, 103·49, 103·55, and 103·46; the mean of which, 103·52, would confirm Berzelius's numbers. We may accept 103·5 without exceeding the limits of errors of observation.

V. Hauert† has been engaged in determining the equivalents

* Bibl. Univ. de Genève, March 1858, p. 209. Chemical Gazette, April 15, May 1, 1858.
† Journal für Prakt. Chemie, February 1858.
of several metals, and has published the results of his determinations of cadmium and manganese. His method is based on the fact that the sulphates of zinc, cadmium, lead, copper, manganese, cobalt, and nickel, when heated in sulphuretted hydrogen gas, yield their respective sulphides of perfectly definite composition. The sulphates of all these metals, with the exception of that of lead, crystallize readily, and may therefore easily be obtained pure. They are also easily rendered anhydrous; and although they are then somewhat hygroscopic, the influence on the weighing may be avoided. The reduction is effected at a moderately low temperature, the results are uniform; there is no danger from leaving the sulphide too long in contact with the gas, as it is not altered thereby; there is no danger of mechanically carrying away any of the substance, provided the operation be not conducted too rapidly; and further, sulphuretted hydrogen is a gas which is readily obtained pure, and hence there is no fear of introducing an impurity from that source. By the reduction of the sulphate to sulphide, 4 equivalents of oxygen are carried off in the form of water. This loss amounts to 30 or 40 per cent., and hence in this important respect the determination is favourable. The determination is dependent on the purity of the substance; and the only difficulty in execution lies in the reduction, and the two weighings.

For the determination of cadmium, a very pure sulphate was prepared by frequent recrystallization: this was precipitated by sulphuretted hydrogen; the precipitate well washed out was dissolved in hydrochloric acid, precipitated by carbonate of ammonia, and after being well washed and dried, was ignited to convert it into oxide. This was then repeatedly washed with water to free it from accidentally adhering chloride of cadmium, and then dissolved in dilute sulphuric acid, and the sulphate repeatedly recrystallized. In order to free it from excess of acid, the sulphate was heated to dull redness each time before it was dissolved.

The reduction was effected in a glass tube heated by means of a Bunsen's burner, and the salt was placed in small porcelain trays. The reduction was commenced at a moderate temperature, which was gradually raised; the operation was continued some hours, although it was found that from 5 to 8 grammes were reduced in two hours. The reduced sulphide was allowed to cool in the current of sulphuretted hydrogen, and was then weighed. It was found that the sulphide was not at all hygroscopic; each time it was tested for undecomposed sulphate. The mean of nine experiments gave 55.9994 as the equivalent of cadmium, assuming the equivalent of sulphur to be 16. Hence 56 is doubtless the true number.
For the determination of the equivalent of manganese, sulphate of manganese was used, which had been prepared from a specimen of fine crystallized pyrolusite from Bohemia. The sulphate was obtained perfectly neutral and pure without any great difficulty. For its reduction, however, a somewhat higher temperature than that of a lamp was required, and the glass tube was accordingly changed for a porcelain tube, which was heated to redness in a Liebig's combustion furnace, and the gas was passed through under a slightly increased pressure, in order to bring it more fully in contact with the salt to be reduced. As in the former case, the reduction was commenced at a low temperature, which was then gradually increased. The sulphide of manganese obtained is considerably caked together, is of a dark green colour, and not in the slightest degree hygroscopic. The mean of nine very concordant experiments was 27.4906, which is sufficiently near 27.5 to allow the assumption of that number. The numbers obtained by Berzelius were respectively 27.5 and 27.61.

Subsequently, v. Hauer tried to determine the equivalent by reducing the red oxide of manganese to protoxide by means of hydrogen. Unfortunately, the loss of weight in this case is very small. This objection may be easily obviated by employing large quantities of proto-peroxide for reduction; but here another difficulty arises, from the fact that the proto-peroxide is an exceedingly hygroscopic substance. And as manganese is usually determined by converting the carbonate into proto-peroxide by ignition, this property of the latter body may give rise to errors if not specially guarded against.

A determination was made by converting the protoxide into proto-peroxide by igniting it in contact with atmospheric air. It gave in two experiments the numbers 27.486 and 27.527, or in mean 27.506, which confirms the one obtained from the experiments with the sulphate.

XXIV. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 150.]

Jan. 14, 1858.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the Electric-Conducting Power of the Metals." By Augustus Matthiessen, Ph.D.

The following values for the conducting power of the metals were determined in the Physical Laboratory at Heidelberg, under the direction of Professor Kirchhoff, by the same method as is described in the 'Philosophical Magazine,' Feb. 1857.
Conducting Power at Temp. in Celsius' degrees.

<table>
<thead>
<tr>
<th>Metal</th>
<th>Conducting Power</th>
<th>°C</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silver</td>
<td>100</td>
<td>0</td>
</tr>
<tr>
<td>Copper, No. 3.</td>
<td>77·43</td>
<td>18·8</td>
</tr>
<tr>
<td>Copper, No. 2.</td>
<td>72·06</td>
<td>22·6</td>
</tr>
<tr>
<td>Gold</td>
<td>55·19</td>
<td>21·8</td>
</tr>
<tr>
<td>Sodium</td>
<td>37·43</td>
<td>21·7</td>
</tr>
<tr>
<td>Aluminium</td>
<td>33·76</td>
<td>19·6</td>
</tr>
<tr>
<td>Copper, No. 1.</td>
<td>30·63</td>
<td>24·2</td>
</tr>
<tr>
<td>Zinc</td>
<td>27·39</td>
<td>17·6</td>
</tr>
<tr>
<td>Magnesium</td>
<td>25·47</td>
<td>17·0</td>
</tr>
<tr>
<td>Calcium</td>
<td>22·14</td>
<td>16·8</td>
</tr>
<tr>
<td>Cadmium</td>
<td>22·10</td>
<td>18·8</td>
</tr>
<tr>
<td>Potassium</td>
<td>20·85</td>
<td>20·4</td>
</tr>
<tr>
<td>Lithium</td>
<td>19·00</td>
<td>20·0</td>
</tr>
<tr>
<td>Iron</td>
<td>14·44</td>
<td>20·4</td>
</tr>
<tr>
<td>Palladium</td>
<td>12·64</td>
<td>17·2</td>
</tr>
<tr>
<td>Tin</td>
<td>11·45</td>
<td>21·0</td>
</tr>
<tr>
<td>Platinum</td>
<td>10·53</td>
<td>20·7</td>
</tr>
<tr>
<td>Lead</td>
<td>7·77</td>
<td>17·3</td>
</tr>
<tr>
<td>Argentine</td>
<td>7·67</td>
<td>18·7</td>
</tr>
<tr>
<td>Strontium</td>
<td>6·71</td>
<td>20·0</td>
</tr>
<tr>
<td>Antimony</td>
<td>4·29</td>
<td>18·7</td>
</tr>
<tr>
<td>Mercury</td>
<td>1·83</td>
<td>22·8</td>
</tr>
<tr>
<td>Bismuth</td>
<td>1·19</td>
<td>13·8</td>
</tr>
<tr>
<td>Alloy of Bismuth 32 parts</td>
<td>0·884</td>
<td>24·0</td>
</tr>
<tr>
<td>Antimony 1 part</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alloy of Bismuth 12 parts</td>
<td>0·519</td>
<td>22·0</td>
</tr>
<tr>
<td>Tin 1 part</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alloy of Antimony 2 parts, Zinc 1 part</td>
<td>0·413</td>
<td>25·0</td>
</tr>
<tr>
<td>Graphite, No. 1.</td>
<td>0·0693</td>
<td>22·0</td>
</tr>
<tr>
<td>Graphite, No. 2.</td>
<td>0·0436</td>
<td>22·0</td>
</tr>
<tr>
<td>Gas-coke</td>
<td>0·0386</td>
<td>25·0</td>
</tr>
<tr>
<td>Graphite, No. 3.</td>
<td>0·0395</td>
<td>22·0</td>
</tr>
<tr>
<td>Bunsen's Battery-Coke</td>
<td>0·00246</td>
<td>26·2</td>
</tr>
<tr>
<td>Tellurium</td>
<td>0·000777</td>
<td>19·6</td>
</tr>
<tr>
<td>Red Phosphorus</td>
<td>0·00000123</td>
<td>24·0</td>
</tr>
</tbody>
</table>

All the metals were the same as those used for my thermo-electric experiments, with the exception of cadmium, which was purified by my friend Mr. B. Jegel.

The alloys of bismuth-antimony, bismuth-tin, antimony and zinc were determined in order to ascertain whether, as they give, with other metals, such strong thermo-electric currents, they might be more advantageously employed for thermo-electric batteries than those constructed of bismuth and antimony.

Coppers No. 1, 2, 3 were wires of commerce. No. 1 contained small quantities of lead, tin, zinc, and nickel. The low conducting
power of No. 1 is owing, as Professor Bunsen thinks, to a small quantity of suboxide being dissolved up in it.

Graphite No. 1 is the so-called pure Ceylon; No. 3 purified German, and No. 2 a mixture of both. The specimens were purified by Brodie's patent and pressed by Mr. Cartmell, to whom I am indebted for the above.

The conducting power for gas-coke, graphite, and Bunsen's battery-coke increases by heat from 0° to 140° C.; it increases for each degree 0·00245, i. e. at 0° C. the conducting power = 100, and between the common temperature and a light red heat about 12 per cent. The following metals were chemically pure:—Silver, gold, zinc, cadmium, tin, lead, antimony, quicksilver, bismuth, tellurium. Those pressed were sodium, zinc, magnesium, calcium, cadmium, potassium, tin, lead, strontium, antimony, bismuth, tellurium, and the alloys of bismuth-antimony and bismuth-tin. The way in which these wires were made is described in the 'Philosophical Magazine' for February 1857.

"On the Thermo-electric Series." By Augustus Matthiessen, Ph.D.

Being enabled by the method described in the 'Philosophical Magazine' (Feb. 1857) to obtain wires of the metals of the alkalies and alkaline earths, I have determined their places, together with those of most of the other metals, in the thermo-electric series.

If A, B, C are different metals, and (AB), (BC), (CA) the electromotive powers of thermo-elements formed out of each two of these metals, whose alternate soldering points are at two different temperatures, so is (AB) + (BC) + (CA) = 0, and therefore

\[
(AB)=a-b,
(BC)=b-c,
(CA)=c-a,
\]

where the values \(a, b, c\) not only depend on the two temperatures, but also on the nature of each of the metals A, B, C. As the differences of the same constitute the electromotive powers, the value for either of these metals may be put = 0.

If the temperatures of the soldering points of a thermo-element only vary slightly, the electromotive powers may be said to be in ratio with the difference of the two temperatures, and under the same conditions the values \(a, b, c\) are also in ratio with the difference of the temperatures, and their relations to each other therefore independent of the same.

If now the value of the second metal relative to the above value of the first be taken equal to 1, the values of the others, in relation to these, become constants, and only depend on the nature of each metal; these values I will call the Thermo-electric Constants. The results obtained are given in the following Table, where the thermo-electric constant of chemically pure silver is taken = 0, and that of a certain commercial sort of copper = 1.

<table>
<thead>
<tr>
<th>Metal Description</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bismuth (commercial, pressed wire)</td>
<td>+35·81</td>
</tr>
<tr>
<td>Bismuth (pure, pressed wire)</td>
<td>+32·91</td>
</tr>
<tr>
<td>Alloy of 32 parts of bismuth and 1 part of antimony (cast)</td>
<td>+29·06</td>
</tr>
<tr>
<td>Substance</td>
<td>Quantity</td>
</tr>
<tr>
<td>-----------</td>
<td>----------</td>
</tr>
<tr>
<td>Bismuth (pure, cast)</td>
<td>+24·96</td>
</tr>
<tr>
<td>Bismuth (crystal, axial)</td>
<td>+24·59</td>
</tr>
<tr>
<td>Bismuth (crystal, equatorial)</td>
<td>+17·17</td>
</tr>
<tr>
<td>Cobalt No. 1 (a pressed specimen prepared by Professor Duflos, and from the Collection of the Heidelberg Chemical Laboratory)</td>
<td>+8·977</td>
</tr>
<tr>
<td>Potassium (the same as used for the determination of its electric conducting powers for different temperatures)</td>
<td>+5·492</td>
</tr>
<tr>
<td>Argentine (wire of commerce, hard)</td>
<td>+5·240</td>
</tr>
<tr>
<td>Nickel (commercial, free from cobalt, but containing iron, &amp;c.)</td>
<td>+5·020</td>
</tr>
<tr>
<td>Cobalt No. 2 (from the Collection of the Heidelberg Chemical Laboratory)</td>
<td>+3·748</td>
</tr>
<tr>
<td>Palladium (wire, hard, from Desmoutis, Chapuis and Co. of Paris)</td>
<td>+3·560</td>
</tr>
<tr>
<td>Sodium (the same as used for the determination of its electric conducting powers for different temperatures)</td>
<td>+3·094</td>
</tr>
<tr>
<td>Quicksilver (pure, fused in a glass tube)</td>
<td>+2·524</td>
</tr>
<tr>
<td>Aluminium (from Rousseau frères of Paris, wire-drawn, analysed by Dr. G. C. Caldwell, and found to contain Si 2·34, Fe 5·89, and Al 91·77)</td>
<td>+1·283</td>
</tr>
<tr>
<td>Magnesium (wire, pressed)</td>
<td>+1·175</td>
</tr>
<tr>
<td>Lead (pure, pressed wire)</td>
<td>+1·029</td>
</tr>
<tr>
<td>Tin (pure, pressed wire)</td>
<td>+1·000</td>
</tr>
<tr>
<td>Copper No. 1 (wire of commerce annealed, containing appreciable quantities of zinc, tin, lead and nickel)</td>
<td>+1·000</td>
</tr>
<tr>
<td>Copper No. 2 (wire of commerce annealed)</td>
<td>+0·922</td>
</tr>
<tr>
<td>Platinum (wire from Desmoutis, Chapuis and Co. of Paris)</td>
<td>+0·723</td>
</tr>
<tr>
<td>Gold (wire, hard drawn, purified by Dr. C. Meyboom)</td>
<td>+0·613</td>
</tr>
<tr>
<td>Iridium (from the Collection of the Heidelberg Chemical Laboratory)</td>
<td>+0·163</td>
</tr>
<tr>
<td>Antimony (wire, pressed specimens, purified by Dr. W. P. Dexter and Dr. G. C. Caldwell)</td>
<td>+0·036</td>
</tr>
<tr>
<td>Silver (pure, drawn, hard)</td>
<td>0·000</td>
</tr>
<tr>
<td>Gas-coke (from the Heidelberg Gas-Manufactory, the hard mass remaining in the retorts)</td>
<td>−0·057</td>
</tr>
<tr>
<td>Zinc (pure, pressed)</td>
<td>−0·208</td>
</tr>
<tr>
<td>Copper (galvanoplastically precipitated)</td>
<td>−0·244</td>
</tr>
<tr>
<td>Cadmium (a strip of foil from Prof. Böttger)</td>
<td>−0·332</td>
</tr>
<tr>
<td>Antimony (commercial, pressed wire)</td>
<td>−1·897</td>
</tr>
<tr>
<td>Strontium (pressed wire)</td>
<td>−2·028</td>
</tr>
<tr>
<td>Lithium (pressed wire)</td>
<td>−3·768</td>
</tr>
<tr>
<td>Arsenic (a piece, pure)</td>
<td>−3·828</td>
</tr>
<tr>
<td>Calcium (pressed wire)</td>
<td>−4·260</td>
</tr>
<tr>
<td>Iron (pianoforte wire No. 4)</td>
<td>−5·218</td>
</tr>
<tr>
<td>Antimony (axial)</td>
<td>−6·965</td>
</tr>
<tr>
<td>Antimony (equatorial)</td>
<td>−9·435</td>
</tr>
<tr>
<td>Red phosphorus (from Prof. Schröetter, from the Collection of the Heidelberg Chemical Laboratory)</td>
<td>−9·600</td>
</tr>
<tr>
<td>Antimony (purified as above)</td>
<td>−9·871</td>
</tr>
</tbody>
</table>
Mr. A. Cayley on the Theory of Matrices.

An alloy of 12 parts of bismuth and 1 part of tin ...... -13\,670
An alloy of 2 parts of antimony and 1 part of zinc ...... -22\,700
Tellurium (from M. Alexander Loewe, purified by
M. Holtzmann) .................................. -179\,80
Selenium (from the Collection of the Heidelberg Chemical
Laboratory) ................................... -290\,00

The method by which these determinations were made is the
following:—Two thermo-elements, whose warm and cold soldering
points had the same temperatures, were compared with each other;
these formed a circuit with the coil of a multiplicator, which sur-
rounded a magnet rod (of about a pound weight) to which was fast-
ened a piece of looking-glass, thereby allowing the deflections of
the magnet to be observed at a distance by means of a telescope and
scale, in the same manner as observations are made with the mag-
etometer. Two commutators were also brought into the circuit; the
one changed the direction of the current in the wire of the multipli-
cator, the other allowed the currents of the thermo-elements to pass
either so as to strengthen, or so as to oppose each other.

The foregoing experiments were carried out in the Physical Ca-
binet at Heidelberg, under the direction of Professor Kirchhoff, to
whose advice and assistance I am much indebted.

"A Memoir on the Theory of Matrices." By Arthur Cayley,
Esq., F.R.S.

The term matrix might be used in a more general sense, but in
the present memoir I consider only square and rectangular matrices,
and the term matrix used without qualification is to be understood
as meaning a square matrix; in this restricted sense, a set of quan-
tities arranged in the form of a square, e. g.

\[
\begin{pmatrix}
a & b & c \\
a' & b' & c' \\
a'' & b'' & c''
\end{pmatrix}
\]

is said to be a matrix. The notation of such a matrix arises naturally
from an abbreviated notation for a set of linear equations, viz. the
equations

\[
\begin{align*}
X &= ax + by + cz \\
Y &= a'x + b'y + c'z \\
Z &= a''x + b''y + c''z
\end{align*}
\]

may be more simply represented by

\[
(X, Y, Z) = \begin{pmatrix}
a & b & c \\
a' & b' & c' \\
a'' & b'' & c''
\end{pmatrix} \begin{pmatrix}
x, y, z
\end{pmatrix}
\]

and the consideration of such a system of equations leads to most
of the fundamental notions in the theory of matrices. It will be
seen that matrices (attending only to those of the same degree) com-
port themselves as single quantities; they may be added, multiplied,
or compounded together, &c.: the law of the addition of matrices is
precisely similar to that for the addition of ordinary algebraical quantities; as regards their multiplication (or composition), there is the peculiarity that matrices are not in general convertible; it is nevertheless possible to form the powers (positive or negative, integral or fractional) of a matrix, and thence to arrive at the notion of a rational and integral function, or generally of any algebraical function of a matrix. I obtain the remarkable theorem that any matrix whatever satisfies an algebraical equation of its own order, the coefficient of the highest power being unity, and those of the other powers functions of the terms of the matrix, the last coefficient being in fact the determinant. The rule for the formation of this equation may be stated in the following condensed form, which will be intelligible after a perusal of the memoir, viz. the determinant, formed out of the matrix diminished by the matrix considered as a single quantity involving the matrix unity, will be equal to zero. The theorem shows that every rational and integral function (or indeed every rational function) of a matrix may be considered as a rational and integral function, the degree of which is at most equal to that of the matrix, less unity; it even shows that in a sense, the same is true with respect to any algebraical function whatever of a matrix. One of the applications of the theorem is the finding of the general expression of the matrices which are convertible with a given matrix. The theory of rectangular matrices appears much less important than that of square matrices, and I have not entered into it further than by showing how some of the notions applicable to these may be extended to rectangular matrices.

January 21.—Dr. J. D. Hooker, V.P., in the Chair.

The following communication was read:—

"On the Physical Structure of the Old Red Sandstone of the County of Waterford, considered with relation to Cleavage, Joint Surfaces, and Faults." By the Rev. Samuel Haughton, Fellow of Trinity College, Dublin, and Professor of Geology.

After describing the general features of the district and giving his reasons for selecting it, the author proceeds to give a detailed account of the faults, joint surfaces, and cleavage planes, 345 in number, observed by him during the course of his survey.

The faults are nineteen in number and reducible to two pairs of rectangular systems. The bearings of these systems are E. 7° 30' N., and E. 54° 22' N. The other faults, which form nearly right angles with the preceding and may be called Conjugate Faults, have the following bearings, N. 3° 45' W. and N. 33° 24' W.

The author considers that the existence of two systems of conjugate faults indicates two distinct systems of upheaving force in the district; a supposition which is strongly confirmed by the fact that the average strike of the beds is E. 10° 46' N., a direction intermediate between those of the systems of faults. He then demonstrates from 345 observed planes, that the systems of joint and cleavage planes are also conjugate systems, reducible to four, of which two are identical with the two conjugate systems of faults already
The cleavage planes are distinguished from the joint planes by a peculiar flaggy or platy structure developed in the rock-mass, parallel to their direction. This structure the author thinks to be the result of pressure; and that it indicates that the cleavage planes are perpendicular to the lines of maximum force; he considers the cleavage planes to have been developed while the rock was yet soft. The joint planes, on the contrary, which are conjugate to the cleavage planes, are considered as perpendicular to the lines of minimum force of compression; they were formed by the shrinking of the rock mass, were subsequent to the cleavage planes, and formed when the rock was hard.

Having established the geometrical relations of the structural planes of the conglomerate, the author then deduces from them the mechanical forces which have been at work in bringing the district to its present condition and form. He believes that the method he has adopted in reference to the conglomerate of the county of Waterford is applicable to the physical structure of other districts; and that his results, if confirmed by corresponding results in other districts, of which he is confident, will prove to be a substantial addition to the arguments in favour of the mechanical theory of slaty cleavage.

Feb. 11.—Major-General Sabine, R.A., Treas. and V.P., in the Chair.

The following communication was read:

"An Account of some recent Researches near Cairo, undertaken with the view of throwing light upon the Geological History of the Alluvial Land of Egypt."—Part. II. By Leonard Horner, Esq., V.P.R.S.

In the first part of this Memoir, read on the 8th of February, 1855, the author stated the main object of the inquiry to have been, to endeavour, by probing the alluvial land in appropriate places, to discover the probable time that has elapsed since the lowest layer of Nile sediment was deposited, and thus to connect geological and historical time. This object, in the opinion of the author, can only be attained by means of shafts and borings of the soil in the immediate neighbourhood of monuments of a known age. The places he selected for these excavations were the vicinity of the Obelisk of Heliopolis, and the site of ancient Memphis. The general introductory matter, and the analyses of the various soils penetrated, together with a description of the researches at Heliopolis, are given in the first part of the memoir; but the author deferred his general conclusions, and all inferences as to the secular increase of the alluvial deposits, until he should have an opportunity of laying before the Society an account.

of the more extensive researches in the district of Memphis. That account, together with the author's general conclusions, form the subject of this second part.

The practical part of the whole inquiry has been conducted under the immediate direction of Hekckyan Bey, an Armenian engineer officer in the service of the Viceroy of Egypt; and a brief biographical account of him is given, showing his eminent scientific qualifications for such researches. The author had the advantage of obtaining the zealous cooperation of our Consul-General in Egypt, the Honourable Charles Augustus Murray, and his successor, the Honourable Frederick Bruce, on whose representations the late Viceroy Abbas Pacha, and the present, not only gave a ready assent to the undertaking, but, with a rare and most exemplary liberality, ordered that the expense should be defrayed by the Egyptian Government.

As at Heliopolis the Obelisk is all that remains above ground of that city, so, at Memphis, there is one solitary monument of its former greatness, a fallen colossal statue of the great king Ramesses II., the Sesostris of the Greeks. All testimony appears to concur in assigning the foundation of Memphis to Menes, the first king of the first dynasty, who, according to Lepsius, began his reign 3892 years B.C. The same authority assigns the dates of 1394 to 1328 B.C. for the reign of Ramesses II. The site of Memphis presented therefore a peculiarly fit situation for prosecuting the inquiry, by sinking pits to the greatest practicable depth near this colossal statue, and around it.

The surface of the ground, for some distance around the statue, being uneven, it became necessary, in order to ascertain the variable depth of water during an inundation, at the mouths of the pits, intended to be sunk in various parts of the area, that the level of the highest rise of the water over the ground at a given time should be determined. This was done for the inundation of 1851, and it proved to be somewhat above the 24th cubit mark of the Rhoda Nilometer, a height of water which covers the entire surface of the valley, leaving above it artificial elevations. The inequalities of the ground are such, that in any section, under the 24th cubit level, the surface varies from where it coincides with that level to nearly 20 feet in the deepest part; so that, while in one part of the district there might be a depth of nearly 20 feet of turbid water, in another it might be less than an inch; and consequently, the same period of time would be represented by very different degrees of thickness of the sediment.

Two pits were sunk close to the fallen colossal statue, sections of both of which are given. In the deepest, the shaft was continued to the depth of 24 feet 5 inches, when further progress was stopped by filtration water. This interruption to excavations occurred in every other pit that was sunk. From the bottom of the shaft, a boring tool was applied, and cores of soil were brought up from successive depths, the lowest being 41 feet 4 1/2 inches from the surface of the ground. The sections given of the two pits in this locality show, that the soil consists of varieties of loam and sand in irregularly alternating layers; and the Nile sediment from the lowest part of the boring was found, by a careful analysis, to be nearly identical in composition
with that deposited by the inundation of the present day. At a depth of 5 feet 8 inches from the surface of the ground they came upon the upper surface of the platform on which the colossus had stood, consisting of two courses of cyclopean masonry, together 5 feet 6 inches thick, resting on an artificial bed of sand, the sand resting on Nile sediment. Throughout the excavation various objects of art and some bones of domestic animals were met with, and the boring instrument brought up from the lowest depth a fragment of pottery.

The author next proceeds to describe, with references to detailed sections, seventeen pits and borings sunk in the area of Memphis, and also a series of seven pits opened in ground below the inundation level of 1851, in a line across the valley from the foot of the Libyan Hills on the west of the Nile, to the skirt of the Arabian Hills on the east of the river, embraced within an area of about five miles from west to east, and a mile from north to south.

In 1854 another series of pits and borings were sunk in the parallel of Heliopolis, above eight miles above the apex of the Delta, in ground below the inundation level of 1853, which was very nearly the same as that of 1851, the line including fifty-one pits in a distance of about sixteen miles, eight miles on the right, and eight miles on the left bank of the river; two of them near the river were carried to a depth of 50 feet, and one to a depth of 60 feet from the surface of the ground. This last reached to within 7½ inches of the mean level of the Mediterranean.

The author then reviews the chief facts made known by the ninety-five probings of the alluvial land above described, and gives the following results:

1. That the alluvium consists of two principal kinds, viz. an argillaceous earth or loam more or less mixed with fine sand, and of quartzose sand, which is probably brought from the adjacent deserts by violent winds;

2. That the Nile sediment found at the lowest depth reached is very similar in composition to that of the present day;

3. That in no instance did the boring instrument strike upon the solid rock, which may be presumed to form the basin between the Libyan and Arabian Hills, containing the alluvium accumulated through unknown ages;

4. That, except minute organisms discoverable only by a powerful microscope, few organic remains were found, and those met with were recent land shells and bones of domestic animals;

5. That there has not been found a trace of an extinct organic body;

6. That at the same level great varieties in the alluvium have been found in adjoining pits, even when the distances between them were very moderate;

7. That there is an absence of all lamination in the sediment. The author points out the causes that account for this,—chiefly the rapid drying of the soil, so soon as the inundation water has subsided, the operations of agriculture, and the violent winds that sweep over the valley forming vast clouds of dust;

8. That in many places the disintegrations of sun-burnt bricks have contributed largely to the soil;
9. That in nearly every part of the ground penetrated, artificial substances have been found, such as fragments and particles of burnt brick and pottery, and at the lowest depth reached.

The author then enters, at some length, into the circumstances which modify the deposition of the sediment in different parts of the valley, showing how the coarser and heavier matter held in suspension in the inundation water must be deposited in greatest amount in the higher parts of the river's course, in its bed, and near its hanks; that this must be further caused by the slight fall, which between Assouan and Cairo is less than 6½ inches in a mile, the Nile in its whole course from the first cataract to the sea not being used as water power; that the vast heat must cause an evaporation that lets fall the solid matter more abundantly in the southern latitudes; that the river from 42 miles below the first cataract is nowhere allowed to overflow the land, but is confined by embankments, so that the waters of irrigation are spread by canals, by which and by the irregularities of the ground eddies are formed. From all these causes affecting the distribution of the sediment over the land, the depth of the annual deposit by the inundation is very different in different parts of the valley, and consequently the same lapse of time may be represented by very different depths of the soil.

The author next treats of the rate of secular increase of the alluvial land. Before entering upon the results at which he arrives by these recent researches, he refers to the operations of the French engineers at the end of the last century, who state the mean of the rise of the land between Assouan and Cairo to be 5 inches in a century. From that conclusion, and especially from the application of it, the author dissents, and states his reasons at considerable length in the Appendix to his Memoir. He considers that in every situation where a calculation is to be made of the rate of secular increase, we must have a fixed point in time to start from; that is, the known age of a monument, the foundation of which rests upon Nile sediment, and upon the sides of which the latter has accumulated by subsequent inundations. If there have been no local causes to disturb the probability that the sediment above and below the foundation has accumulated at the same rate, we divide the amount above the foundation by the number of centuries known to have elapsed from the erection of the monument to the present time, and then apply the same chronometric scale to the greatest ascertained depth of sediment below the foundation. Estimated by this rule, the researches at Heliopolis gave the result of a rate of increase of 3·18 inches in a century. But a degree of uncertainty arises at this place, because of the city appearing to have been built upon a portion of land somewhat raised above the level of the rest of the skirt of the desert, and advancing into the low ground then inundated by the Nile; whereby it became doubtful whether a bed of sand penetrated was sedimentary or a part of the desert land.

In the excavations near the colossus of Ramesses II. at Memphis, there were 9 feet 4 inches of Nile sediment between 8 inches below the present surface of the ground and the lowest part of the platform on which the statue had stood, after making a due allow-
ance for the foundation of the platform having been below the then surface. It is assumed that the platform was laid in the middle of the reign of that king, that is, in the year 1361 B.C., which, added to A.D. 1854, when the observation was made, give 3215 years during which the above depth of sediment was accumulated; and supposing that no disturbing cause had interfered with the normal rate of deposition in this locality, and of which there is no evidence, we have thus a mean rate of increase within a small fraction of 3½ inches in a century. Below the platform, there were 32 feet of the total depth penetrated, but the lowest two feet consisted of sand, below which it is possible there may be no true Nile sediment in this locality, thus leaving 30 feet of the latter. If that amount has been deposited at the same rate of 3½ inches in a century, it gives for the lowest part deposited an age of 10,285 years before the middle of the reign of Ramesses II., 11,646 years B.C., and 13,500 years before A.D. 1854.

The author then observes, that these recent researches, taken in conjunction with those of a similar kind by the French engineers at the close of the last century, high in Upper Egypt, afford strong presumptive evidence that the whole of the land of Egypt between the bounding hills, from the first cataract to the sea, extending nearly 700 miles—that land which is associated in our minds with all that is most ancient in history or tradition—belongs entirely to the recent geological period. No trace of an extinct organism has been turned up to take the formation of the alluvial land of Egypt beyond that modern epoch from which we are used to carry back our geological reckonings.

The author concludes with some remarks on the evidence which these researches seem to afford of a very early existence of man in Egypt. In a large majority of the excavations and borings the sediment was found to contain at various depths, and frequently at the lowest, small fragments of burnt brick and of pottery. In the lowest part of the boring of the sediment at the colossal statue in Memphis, at a depth of 39 feet from the surface of the ground, consisting throughout of true Nile sediment, the instrument brought up a fragment of pottery. [This fragment was exhibited when the paper was read.] Having been found at a depth of 39 feet, it would seem to be a true record of the existence of man 13,371 years before A.D. 1854, reckoning by the before-mentioned rate of increase of 3½ inches in a century; 11,517 years before the Christian era; and 7625 years before the beginning assigned by Lepsius to the reign of Menes, the founder of Memphis; of man, moreover, in a state of civilization, so far, at least, as to be able to fashion clay into vessels, and to know how to harden them by the action of a strong heat.

Cambridge Philosophical Society.

[Continued from p. 158.]

Nov. 23, 1857.—A paper was read by Professor Thompson, "On the Sophista of Plato."

In this paper the genuineness of the Sophista was defended, and
some of its philosophical bearings pointed out. In answer to the doubts expressed by the Master of Trinity in a previous communication, it was shown that the Sophista, as well as the Politicus, which is a continuation of it, are repeatedly referred to in the works of Aristotle. In particular, Arist. Metaph. v. ii. 93 was appealed to as evidence that Aristotle had not only read the dialogue called Sophista, but believed it to have been written by his master.

The Dialogue was analysed, and shown to be a critique of the negative or Eristic systems of logic, derived from the Eleatics, which were taught by Euclides and Antisthenes, the founders of the Socratic sects of the Megarics and Cynics respectively. Many allusions, personal and otherwise, to Antisthenes were pointed out, as existing both in this dialogue and in the Theetetus, of which it is a professed continuation. The Theetetus was regarded as a critique of the contemporary psychology, and the Sophista as a confutation of the prevailing schemes of logic; and both were shown to contain exemplifications of the twin processes of Induction or Collection, and Division or Classification, which constitute, according to Plato in the Phaedrus, p. 265 E., the science or art of Dialectic.

It was also argued, in opposition to Schleiermacher, that the Materialistic doctrines confuted in Sophista, p. 246, represent those of Antisthenes, rather than the atomic theory of Democritus, or the empirical system of Aristippus.

The analysis of the simple Proposition (Soph. p. 262) was shown, by the testimony of Plutarch and others, to be Platonic, and the imperfect Idealists refuted in p. 276 were identified with the Megarics, and distinguished from the Platonists.

Passages were also quoted from the Politicus, showing the disciplinary and educational uses to which the method of Division was made subservient in the teaching of the Academy; and this teaching was further illustrated by a quotation of considerable length from a Comic Poet ap. Athen. lib. ii.

Incidentally, Porphyry and Abelard were appealed to in evidence that Plato’s Method of Division was known to the Neo-Platonists and the Schoolmen, and recognized by them as characteristic of his Dialectic.

December 7.—Professor Miller made a communication on the Planimeters of Wetli, Deecher, and Amsler, and communicated the following simple proof of the principle of Amsler’s, due to Mr. Adams.

Let O be the fixed point,

P the tracer,

Q the hinge,

W the centre of wheel,

M the middle point of PQ,

\[ OQ = a, \quad PQ = b, \quad MW = c. \]
The area of any closed figure whose boundary is traced out by $P$, is the algebraical sum of the elementary areas swept out by the broken line $OQP$ in its successive positions.

Let $\phi$ and $\psi$ be the angles which $OQ$, $QP$ at any time make respectively with their initial positions.

$s$ the arc which the wheel has turned through at the same time.

If now $OQP$ take up a consecutive position, and $\phi$, $\psi$, $s$ receive the small increments $\delta \phi$, $\delta \psi$, $\delta s$, we see that $\delta s = \text{motion of } W \text{ in direction perpendicular to } PQ$.

Hence motion of $M$ in the same direction $= \delta s + \delta \psi$, and therefore the elementary area traced out by $QP = b(\delta s + \delta \psi)$. Also elementary area traced out by $OQ = \frac{1}{2}a^2 \delta \phi$.

Hence the whole area swept out by $OQP$ in moving from its initial to any other position is

$$\frac{1}{2}a^2 \phi + bc \psi + bs.$$

If $OQP$ returns to its initial position without performing a complete revolution about $O$, the limits of $\phi$ and $\psi$ are 0, and the area of the figure traced out by $P$ is $bs$.

If $OQP$ has performed a complete revolution, the limits of $\phi$ and $\psi$ are $2\pi$, and the area traced out is

$$\pi(a^2 + 2bc) + bs.$$

A paper was also read by the Astronomer Royal, "On the substitution of Methods founded on Ordinary Geometry for Methods based on the General Doctrine of Proportions, in the treatment of some Geometrical Problems."

The doctrine of proportions laid down in the fifth book of Euclid is the only one applicable to all cases without exception, but it is cumbrous and difficult to remember. It is therefore natural to attempt, in special applications of the doctrine, to introduce the facilities which are special to each case. This has been done long since in the case of numbers, and this the author of this paper attempts in some cases in which geometrical lines only are the subject of consideration, by a new treatment of a theorem equivalent to Euclid's simple ex aequali and of the doctrine of similar triangles, referring to nothing more advanced than Euclid, Book II.

The author proves,—

1. If the rectangle contained under the sides $a$, $B$ be equal to the rectangle contained under the sides $b$, $A$; and if these rectangles be so applied together that the sides $a$ and $b$ shall be in a straight line and that the side $B$ shall meet the side $A$, the two rectangles will be the complements of the rectangles on the diameter of a rectangle.

2. If the rectangle contained under the lines $a$, $B$ is equal to the rectangle contained under the lines $b$, $A$; and if the rectangle under the lines $b$, $C$ is equal to the rectangle contained under the lines $c$, $B$; then will the rectangle contained under the lines $a$, $C$ be equal to the rectangle counted under the lines $c$, $A$.

(This is equivalent to the ordinary ex aequali theorem.)
If \( a:b::A:B \)
and
\( b:c::B:C, \)
then will \( a:c::A:C. \)

3. If two right-angled triangles are equiangular, and if \( a, A \) are their hypothenuses, and \( b, B \) homonymous sides, the rectangle contained under the lines \( a, B \) is equal to the rectangle contained under the lines \( b, A \).

(The equivalent theorem in proportions is
\[ a:b::A:B. \]

4. If \( a, c \) and \( A, C \) are homonymous sides of equiangular triangles, the rectangle contained under \( a, C \) will be equal to the rectangle contained under \( c, A \).

5. If \( b, c \) and \( B, C \) are homonymous sides including the right angles of two equiangular right-angled triangles, the rectangle contained under \( b, C \) will be equal to the rectangle contained under \( c, B \).

6. If the rectangle contained under the lines \( a, B \) is equal to the rectangle contained under the lines \( b, A \); the parallelogram contained under the lines \( a, B \) will be equal to the equiangular parallelogram contained under the lines \( b, A \).

(This is equivalent to the proposition,
If
\[ a:b::A:B \]
then
\[ a:b::A \cos \alpha : B \cos \alpha. \]

These propositions will suffice for the treatment of the first thirteen propositions of Euclid’s sixth book (Prop. I. excepted), and of all the theorems and problems apparently involving proportions of straight lines (not of areas, &c.) which usually present themselves. The author then proceeds, as an instance of their application, to prove by means of them the following theorem:

If pairs of tangents are drawn externally to each couple of three unequal circles, the three intersections of the tangents of each pair will be in one straight line.

Also a paper was read by Professor De Morgan, “On a Proof of the existence of a Root in every Algebraic Equation with an examination and extension of Cauchy’s Theorem of Imaginary Roots; and remarks on the proofs of the existence of Roots given by Argand and by Mourêy.”

The extension of Cauchy’s theorem is very easily found, when the proof is the first of those given by Sturm in Liouville’s Journal. The extended theorem is as follows:

Let \( \phi z \) be any function of \( z \), and let \( z=x+y \sqrt{-1} \). Let \( (x, y) \) be a point on any circuit which does not cut itself. Let this point describe the circuit in the positive direction of revolution; and, \( \phi(x+y \sqrt{-1}) \) being \( P+Q \cdot \sqrt{-1} \), let \( \frac{P}{Q} \) change sign \( k \) times as in
\[ +0-, \]
and \( l \) times as in \(-0+\). Let \( (x, y) \) be called a radical point when \( \phi(x+y \sqrt{-1}) = 0 \), or \( = \infty \). Let there be \( m \) radical points of the first kind within, and \( m' \) upon, the circuit; let there
be $n$ radical points of the second kind within, and $n'$ upon, the circuit. Then

$$k - l = 2m + m' - (2n + n').$$

Sturm's demonstration of the case where $m' = 0, n = 0, n' = 0$, which is Cauchy's theorem, assumes the existence of the roots of an algebraical expression. Mr. De Morgan's proof of the existence of these roots is as follows:—He shows, à priori, that in the sequence of signs which Cauchy's theorem requires to be examined, $k - l$ never undergoes any alteration except after 0 and $\infty$ have coincided, that is, where $P = 0$, $Q = 0$, simultaneously. It is then easily proved that change in $k - l$ happens in every algebraical equation.

The proofs given by Argand and Mourey were intended as illustrations of the power of the extension which is now called double algebra. Stript of this interpretation, they are purely algebraical, and Argand's proof is really that which was afterwards found by Cauchy. Argand's proof is more simple in form than Cauchy's.

---

**GEOLOGICAL SOCIETY.**

[Continued from p. 76.]

May 26, 1858.—Prof. Phillips, President, in the Chair.

The following communications were read:—

1. "On the Pleistocene Sea-bed of the Sussex Coast, being the Western Extension of the Raised Sea-beach of Brighton." By J. Prestwich, Esq., F.R.S., F.G.S.

The author first observed that the well-known raised beach at Brighton, described by Dr. Mantell, has been since extended by Sir R. Murchison to Hove, and by Mr. Dixon to Lancing and Broadwater near Worthing; and that Mr. Godwin-Austen has described some marine beds, which he ascribes to the same age, on the coast between Bognor and Bracklesham. He then proceeded to state the results of his examination of the country between Brighton and Havant. Traces of this old sea-beach are, in Mr. Prestwich's opinion, to be seen on the slightly-raised ground at the base of the chalk-hills east of Arundel, at a distance of three or four miles from the sea. It is much more distinct westward of Arundel, and can be followed to near Chichester. In a wood at a short distance north of the road from Arundel to Chichester, and about two miles from the former, these beach-deposits appear as a bed of sand about seven feet thick, with thin patches of shingle, and overlaid by subangular flint-gravel. On the brow of Avisford Hill the same bed of sand is seen to overlie the mottled clays; and in a sand-pit in the wood east of Slindon Common, similar pebbly sand, at least 16 feet thick, is seen to be overlaid by partially rounded flint-shingle, about three feet thick, and both to be covered by ferruginous sandy clay full of large angular flints, which occupies also a furrow excavated through the shingle into the sand.
A mass of brick-earth, 10 feet by 8, forms a core to the ferruginous clay in the furrow. It is to be remarked that the sand-bed is here about 100 feet above the sea-level; and also that it is intersected by a deep dell. The angular flint-gravel, underlaid by the sands, stretches across Slindon Common, and occurs at Broxgrove Common, and at intervals towards Goodwood. At Waterbeach, adjoining Goodwood, a sand-pit shows, in descending order, 1. ferruginous clay, full of angular flints, two to six feet; 2. chalk-rubble, about nine feet thick, containing small angular flints, and with a furrowed surface; 3. fine ash-coloured sand, slightly micaceous, with thin seams of concretionary sandstone, some few boulders of chalk, and friable shells.

The shingle-bed, seen at Avisford, is here replaced by chalk-rubble, like that overlying the old beach at Brighton; but no shells nor bones were found. Shells of the common *Mytilus* and the edible *Cardium* are found in the sand, but they are usually very friable; and in the holes made by *Pholades* on the large lumps of hard white chalk occurring in this sand, specimens of *Purpura lapillus* occur, whilst small *Balani* are attached to the surfaces of these chalk-boulders. A specimen of *Echinocyamus pusillus* was also met with. This sand-deposit, which the author believes to be identical with the old Brighton beach, he traces also westward of Goodwood to near Lavant and probably to Bourne Common.

2. "On the Sedimentary and other External Relations of the Palæozoic Fossils of the State of New York." By J. J. Bigsby, M.D., F.G.S.

The objects proposed in this inquiry were—to give more precision to facts as yet imperfectly ascertained, to discover new materials for the history of these earliest times, and to treat of new points of connexion between the Palæozoic Basins of New York and Wales. The first part of this memoir commenced with a few observations on the agencies by which the palæozoic sediments or sea-bottoms were laid down, namely, 1st the constant and superficial, or Neptunian, and 2nd the modifying or occasional and subterranean (Plutonic); and then proceeded to describe their mineral character. In the second part, the distribution and immediate relations of palæozoic animal life in the State of New York were considered; and in the third part the recurrence or vertical range of fossils was treated of in detail. Lastly, the results arrived at by the author were given as reflections presenting themselves on a survey of the ancient and vastly prolonged processes that laid down the palæozoic strata of New York. The unity of design and predetermination of the complete idea manifest in these geological phenomena,—the evidences of direct creation and of occasional migrations,—the conditions of contemporaneity,—the relations of geographic and epochal centres of life,—the laws of recurrence of animal forms, or their reappearance in new epochs, and of the disappearance of faunæ,—and lastly the points of similarity and of dissimilarity between the palæozoic rocks of Wales and New York were, in the concluding portion of the memoir, fully dwelt upon.
Prof. Harkness on Jointings, and on the Dolomites near Cork. 235

June 9.—L. Horner, Esq., Vice-President, in the Chair.

The following communications were read:—

1. "On Jointings, and on the Dolomites near Cork." By Professor Harkness, F.R.S., F.G.S.

The strata in the neighbourhood of Cork consist of Devonian rocks, and the lower portion of the Carboniferous series. In the former a regular system of north and south joints occur, cutting through the strata at nearly right angles to their strike. The carboniferous rocks are also intersected by joints having a similar direction; but in these latter rocks other joints occur, one system of which approaches to the horizontal, and the other is inclined sometimes towards the east, at other times towards the west. This threefold system of jointings, which make their appearance in the carboniferous rocks, is not equally prevalent in all the members of this series.

The limestones in which they occur in the greatest perfection are such as have the greatest amount of carbonate of lime, and are of the purest character.

In some siliceous limestones, which are associated with the more perfectly jointed rocks, the series of jointings is confined to such as prevail in the Devonian strata, these having only the north and south system of divisional planes. Respecting the north and south series of joints, the author is disposed to regard them as resulting from pressure, caused by the movements which have given to the strata in the south of Ireland an arrangement in the form of rolls, having an east and west strike,—a course at right angles to the direction of the principal joints.

In connexion with these north and south joints, both in the limestones of the carboniferous series and also in the underlying carboniferous slates, the author has noticed distortion of fossils, and this distortion consists of an elongation between the series of joints, the fossils being pulled as it were out towards the sides of these planes, or at right angles to the strike of the joints.

Prof. Haughton has already noticed the elongation of the fossils in the direction of the strike of cleavage, a mode of elongation at variance with that described by Prof. Phillips and the late Mr. D. Sharpe.

As this direction (of distortion), however, occurs among strata in the north of Ireland devoid of cleavage, the author is disposed to regard it as resulting rather from jointing than from cleavage; and he regards it as evidence that pressure has exerted considerable influence in producing joints. Concerning the other two systems of joints, which are peculiar to the purer limestone, these have a disposition to split the masses of limestone into rhomboids; and these divisional planes, the author considers, result from the same cause as the principal joints; but, owing to the pure state of the limestone, lines of division akin to mineral cleavage have intersected these strata, and given to them their complex series of jointings.

Dolomites also occur in the limestones of the district around Cork. They usually are met with in a dyke-like form, and in general are found conforming to the strike of the perpendicular
north and south joints, or, in other words, intersecting the strata at right angles.

In some instances, however, they have an east and west course, and are found associated, in this condition, with the siliceous limestones which possess only the principal system of joints.

These dolomites, from their mode of occurrence, have had their origin subsequent to the production of the joints, and are often found thinner below than above. From the circumstances of their occurrence, the author is disposed to regard them as metamorphosed ordinary limestones; and the metamorphic action he is disposed to attribute to sea-water, which, containing sulphate of magnesia, has found access, by means of the joints, into the limestone masses; and this, at an elevated temperature, combined with pressure, has produced these changes, by the double decomposition of sulphate of magnesia and carbonate of lime, and, by this means, given rise to the production of the dolomites occurring in this neighbourhood.

2. "On an Experiment in melting and cooling some of the Rowley Rag." By W. Hawkes, Esq.

About 31 cwt. of basalt was melted in a large double reverberatory furnace, and after a slow cooling during thirteen days, it presented an upper stratum of stony vesicular matter, about 1 inch thick, next a layer of black glass, from 2 to 8 inches deep on that side of the mass which was exposed to the air from the door of the furnace (elsewhere, immediately under the vesicular layer was solid stone, interspersed here and there with air-bubbles). Mr. Hawkes added some observations relating to the results of experiments which he had made to ascertain the temperature of melted cast iron, and of melted basalt.


The author described the occurrence of ores of iron within a district, of about thirty miles in length, between Ilfracombe and the Bridgewater Flats, in slaty rocks belonging to the "greywacke" of De la Beche's "Report on Cornwall," or to the "Plymouth group" of the Devonian System, as described by Sedgwick and Murchison. They are situated partly to the north, but chiefly to the south, of the irregular bands of limestone which pass from Combe Martin eastward by Simonsbath to Cutcombe and Treborough. The really stratified deposits of iron-ore are bands of argillaceous nodules, alternating with shales, similarly to those of the coal-measures; and these have been found in abundance on the flank of Hangman Hill near Combe Martin, and in the North Forest of Exmoor; but, in general, their angle of inclination is too great to allow of their being worked in competition with the analogous ores of the coal-fields.

The lodes containing iron-ore, extensively worked some centuries ago, and which have recently been explored in the Brendon Hills, and tested on the property of F. W. Knight, Esq., M.P., are so nearly conformable to the beds of slate, striking about E. 10° S., with a dip of from 45° to 65° southward, that they might be easily taken for stratified deposits.
In several places, as at Cornham Ford, the Roman lode, and the little Woolcombe lode, haematite has been found at or near the surface, containing 98.4 per cent. of peroxide of iron, in masses which present the structure of sparry iron or carbonate. Most of the lodes, however, especially those of the Deer-park on Exmoor, and Raleigh’s Cross, consist of brown or hydrous peroxide, generally occurring in large cavernous masses, with the interior of the cells often lined with brilliant but minute crystals of Goethite (pure hydrous peroxide), yet exhibiting the rhombohedral structure of sparry ore. Analysis shows this ore, especially from certain of the Exmoor veins, to contain 71.3 per cent. of peroxide of iron, together with a considerable per-centage of oxide of manganese.

Lastly, several of the veins prove in depth to consist of sparry ore (siderite or carbonate of iron), containing a notable per-centage of carbonate of manganese. This has especially been shown at Goosemoor, Huel Eliza, Hangley Cleave, and on Kentisbury Down. The width of these veins varies from a few inches to above 20 feet, and at the greatest depth hitherto reached, 140 feet, there is no deterioration in size. Quartz-veinstone and fragments of slate occasionally fill part of the lodes; and this fact, coupled with the interruptions of “slides” and “heaves,” has led to some misplaced disappointment on the part of those who made their calculations without taking into account the capricious nature of true veins.

Hence it appears,—1st, that the vein-fissures have been formed in this district almost solely in planes conformable to those of stratification, and that, after having been filled with carbonate of iron, quartz, &c., they have been more or less disturbed.

2ndly. That the sparry ore has been converted, often to a great depth, into Goethite or pure hydrous peroxide, the consequent decrease of bulk giving rise to the cellular cavities lined with crystals.

3rdly. That some smaller portions have been converted into haematite, having probably passed through the intermediate stage of brown peroxide.

4thly. The pebbles of haematite at the base of the New Red in the adjoining district appear to be derived from these lodes, and would indicate their having been filled with ore prior to the great spread of deposits of red and brown iron-ore which took place in S.W. England and South Wales soon after the deposition of the coal-measures.

The interest attached to these pseudomorphous changes, and to the scale on which they have taken place, is heightened by comparison with the ores of Siegen, of Sommorstro near Bilboa, and those brought by Dr. Livingstone from Central Africa.


The author described some microscopic arborescent crystallizations of native copper occurring with ruby copper in small cavities in a brown limestone, which contains yellow bisulphuret of copper. Harder sulphurous ore occurs in the bed below, and carbonates and oxides of copper in the bed above. The arborescent copper is pro-
bably the result of the decomposition of copper-ores, and has been formed subsequently to the crystals of lime in the limestone, which are often tipped with an octahedron of ruby oxide, or overlaid with spikes of the native copper.

5. "On the Slate-rocks and Trap-veins of Easdale and Oban." By Professor James Nicol, F.G.S.

The author first described the mineral characters of the clay-slate of the Islands of Easdale and Seil, and noticed the iron-pyrites and the veins of calc-spar and of quartz occurring in the slate, as well as the gritty and calcareous strata that are detected as interstratified with the more highly cleaved beds of the slate; also some soft carbonaceous beds and some hard calcareous nodules, imbedded in the slate. In one of these nodular masses, at a depth of 140 feet from the surface, a small quantity of bituminous coal was stated to have been lately found. Fucoidal or annelidal markings occasionally appear in the slates. Prof. Nicol then described the original but obscure stratification of the Easdale slate, which, with care, is seen to present beds disposed in undulations, and, where most clearly seen, dip at a high angle to the west or north-west. Near Oban, and along the Sound of Kerrera, the stratification is more distinct, and the strata are very much contorted. The slaty cleavage of the district was then treated of in detail. Its average strike varies 20° from that of the beds; and the direction of strata and cleavage-planes have evidently a connexion with the more ancient and deep-seated causes which have produced the most striking features in the configuration of the country; they are nearly parallel to the shores of Loch Linnhe, to the Island of Lismore, and to the great depressions occupied by Loch Etive, Loch Awe, and Loch Fyne. The trap-veins of Easdale and Oban were next described, especially a pitch-stone on Seil, and were shown to be of three periods. At least two sets of the veins appear to be of a date subsequent to the Jurassic period; and the author remarked, as an instance of the very recent date of many of the great convulsions by which the present physical outline of Scotland has been produced, that the mountain-cliff, 1200 to 1500 feet high, forming the coast of Morne, between Ardtornish and Loch Linnhe, consists in half of the old gneiss, in half of lias-strata and recent trap, brought side by side with each other along an enormous fault, and now smoothed down into one uniform mass. In conclusion, Prof. Nicol stated that he considered the Easdale slate to be of Lower Silurian age, but younger than the slates of Birnam and Dunkeld.

XXV. Intelligence and Miscellaneous Articles.

RESISTANCE OF SHIPS.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the course of last year there were communicated to me in confidence the results of a great body of experiments on the engine power required to propel steam-ships of various sizes and figures at
Intelligence and Miscellaneous Articles.

various speeds. From those results I deduced a general formula for the resistance of ships having such figures as usually occur in steamers, which on the 23rd of December, 1857, I communicated to the owner of the experimental data; and he has since applied it to practice with complete success.

As the experimental data were given to me in confidence, I am for the present bound in honour not to disclose the formula which I deduced from them; but as I am desirous not to delay longer the placing it upon record, I have recourse to the old fashion of sending it to you in the form of an anagram, in which the letters that occur in its verbal statement are arranged in alphabetical order, and the number of times that each letter occurs is expressed by figures.

20 A. 4 B. 6 C. 9 D. 33 E. 8 F. 4 G. 16 H. 10 I.
5 L. 3 M. 15 N. 14 O. 4 P. 3 Q. 14 R. 13 S. 25 T.
4 U. 2 V. 2 W. 1 X. 4 Y. (219 letters in all.)

I hope I may soon be released from my present obligation to secrecy.

I am, Gentlemen,

Your most obedient Servant,

Glasgow, August 26, 1858.

W. J. Macquorn Rankine.

OBSERVATIONS ON THE NATIVE PHOSPHATES OF COPPER.

BY PROF. BERGEMANN.

I have been led by my late investigations of Ehelite to make some experiments upon other compounds of oxide of copper with phosphoric acid and water, in order to ascertain whether these might not also contain vanadic acid. And although I have not again found this acid, the experiments yielded unexpected results, inasmuch as I detected arsenic acid in all the minerals belonging to the phosphate of copper group, and often in considerable quantities. I was surprised at finding this acid, for all the researches on these minerals mention no trace of it.

The analysis of Hungarian Libethenite performed upon small selected crystals, for which I am indebted to the kindness of my friend Dr. Krantz, gave the following results:—

<table>
<thead>
<tr>
<th>Component</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Oxide of copper</td>
<td>66.29</td>
</tr>
<tr>
<td>Phosphoric acid</td>
<td>26.46</td>
</tr>
<tr>
<td>Arsenic acid</td>
<td>2.30</td>
</tr>
<tr>
<td>Water</td>
<td>4.04</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>99.09</strong></td>
</tr>
</tbody>
</table>

The relation of the oxygen is therefore 4:5:1, and Libethenite is, as is well known, isomorphous with olivenite.

Phosphochalcite from Linz on the Rhine consists of—

<table>
<thead>
<tr>
<th>Component</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Oxide of copper</td>
<td>69.97</td>
</tr>
<tr>
<td>Phosphoric acid</td>
<td>19.89</td>
</tr>
<tr>
<td>Arsenic acid</td>
<td>1.78</td>
</tr>
<tr>
<td>Water</td>
<td>8.21</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>99.85</strong></td>
</tr>
</tbody>
</table>

Oxygen.

13.26
14.92  15.71
0.79
3.58

Oxygen.

14.1
11.14  11.75
0.65
7.11
Hence from the relation $6:5:3$, phosphochalcite would be isomorphous with strahlerz.

It would appear from its general diffusion, that the arsenic acid in these ores plays the same part as the phosphoric acid in those consisting of arseniate of copper. I have found arsenic in Siberian Libethenite, in phosphochalcite of Tagilsk and Hungary, and also in Tagilite, and doubt not that other members of this group also contain it. Even Ehlite contains a trace, which has not been mentioned in the published paper. The quantity of arsenic acid appears to vary in different ores; for instance, in Siberian Libethenite a greater quantity appears to replace phosphoric acid than in Hungarian.

It is in any case surprising that arsenic acid, which is generally so readily detected, should have remained so long undiscovered, more especially in the blowpipe examination of these minerals. If these be treated in the ordinary manner upon charcoal with the reducing part of the flame, neither the odour nor the incrustation is at first perceptible; but by continuing the reducing action, both appear, much more quickly, however, if any reducing agent be added, or if the test be made in the manner described by Plattner for the detection of small quantities of antimonic acid, especially of the combination of this substance with oxide of copper.

In the present case arsenic acid is most simply detected by bringing the solution of a small quantity of the mineral into a Marsh's apparatus. Arsenical mirrors may thus be prepared in sufficient quantities to obtain the most decisive and indubitable results with them.—Poggendorff's *Annalen*, vol. civ. p. 190.

---

**ON THE EUPHOTIDES OF MT. ROSE.** BY T. STERRY HUNT.

In the American Journal of Science for May 1858, there appeared an extract from a letter of mine to Mr. J. D. Dana, describing the results of some observations on the Euphotide of Mt. Rose. In this note it was stated that the smaragdite of this euphotide contains vanadium, and that this element had also been detected with nickel in a chromiferous serpentine from Gaspé. It was not until this note had been printed in the American Journal that I discovered my error, which is however corrected in the list of errata of the same Number of that Journal. The colouring matter of the smaragdite appears to be chromic oxide, and neither in it nor in the serpentine of Gaspé have I been able to verify the presence of vanadium. As the note in question is copied in this Magazine for July 1858, p. 553, I take this means of correcting the mistake.

As I have before stated, the true euphotide is a mixture of smaragdite with Saussurite, the white mineral is a compact zoisite or lime-alumina epidote, having a density of 3·3—3·4, and a hardness of 7·0. In some specimens of the rock, however, cleavable grains and masses of a felspar, apparently labradorite, are seen imbedded in the compact Saussurite, showing the passage of the euphotide into a diablastic dolerite, with which many lithologists have confounded it. In other specimens the Saussurite becomes intermingled with small bladed crystals of bluish-gray kyanite, exhibiting a passage into the so-called *dithene rock*.

Montreal, Canada, Aug. 1, 1858.
On the Distribution of Heat over Islands, and especially over the British Isles. By Henry Hennessy, F.R.S., M.R.I.A., Professor of Natural Philosophy in the Catholic University of Ireland*.

No element among the conditions of terrestrial climate is so important, none has engaged so much attention, nor has any other been systematically observed for so long a period, as temperature; yet the time is comparatively recent when philosophers commenced to consider the laws of its distribution over the earth’s surface in a truly scientific spirit. In 1779 appeared the mathematical inquiries of Lambert, in which an attempt was made to estimate the difference between the heat received by the earth from the sun and the heat which it loses by radiation. Mayer had about the same time deduced his well-known law from theoretical grounds, and by considering solar radiation alone as the source of terrestrial heat. Towards the close of the last century our countryman, Richard Kirwan, attempted for the first time to compare Mayer’s law with existing observations, and thus to arrive at general views regarding the climate of our planet. Humboldt followed up this step by one of still more importance when he published his essay on Isothermal Lines. Laying aside speculative considerations, he presented the results of actual observation in a way at once novel and luminous. Having found the mean annual temperatures of a great number of stations, he compared them together and marked on a map the places which had equal temperatures. The points of equal temperature having been joined by curves, the forms of the isothermal lines thus produced present a graphical picture of the

* From the Atlantis for July, 1858: revised and communicated by the Author.

distribution of heat over the earth. This elegant method of representation has since been applied to other elements of terrestrial physics, such as atmospheric pressure, magnetic intensity and declination, and the distribution of the tides.

While the science of terrestrial temperature was thus progressing by systematic induction from observed facts, some of its fundamental principles were examined and consolidated by the mathematical labours of Fourier, Poisson, and Laplace. Fourier especially has established with surpassing clearness the relations subsisting between the thermal conditions of the earth's surface, the warmth of its interior, and the temperature of the planetary spaces. His works also contain some highly suggestive views as to the influence of the physical properties of the superficial portions of the earth's crust on local and general climates.

No one has more ably extended, or more effectively applied, Humboldt's graphical method of representing the distribution of terrestrial temperature than Professor Dove. Having tabulated a vast number of observations made in different parts of the world, he has not only been able to improve the representation of mean annual temperature, but has furnished us with maps of the isothermals of each month*.

A glance over the maps of mean annual, mean summer, mean winter, or mean monthly temperature, informs us of the general fact, that the temperature of a place depends on other circumstances besides its latitude. Mayer's law could not be true unless the isothermals were all parallel to the equator,—a relation which they are far from fulfilling. The same remark applies to the improved forms of Mayer's law, which have been subsequently suggested by different eminent scientific men†. Had the earth's surface been of uniform texture, and stripped of every kind of fluid covering, the temperature of every point would depend upon its latitude, and places having the same latitude would possess the same temperature. But three-fourths of the heat-absorbing and heat-radiating surface is water; and this, from its mobility

* These maps appeared originally in the Berlin Transactions, but they are best known in these countries through the English edition prepared for the members of the British Association by General Sabine.

† Except a formula of Sir David Brewster's, in which he makes the mean temperature of a place depend upon its distance from two points of minimum temperature, the one in Asia, the other in America. This was undoubtedly an improvement; but it did not justify the remark of an eminent writer, that the coordinates of latitude and longitude should altogether be discarded in connexion with climate. The connexion of the former is obvious, though complicated; the connexion of the latter is less important, and it manifestly depends on the prevalent directions of oceanic and aerial currents, the influence of which on climate is universally acknowledged. See Forbes, Report of the British Association, vol. i. p. 215, second edition.
and other properties, greatly modifies the distribution of the warmth which its particles receive from the sun. The surfaces of the seas and oceans are traversed by currents, whose temperatures influence the climate of any land which they approach, and whose directions depend on varied and complicated conditions. The resulting effect upon the temperature of the land must correspond in some measure to such complications.

But even if the ocean were free from currents, it might still influence the climate of the land if it possessed a different mean temperature. That it does actually possess a higher mean temperature at its surface than the mean temperature of the air over the land, seems to have been definitely proved within a very recent period. The facts adduced by Admiral Duperrey, by which he was led to infer that the mean temperature of the liquid coating which surrounds three-fourths of our planet is higher than that of even the lower strata of its gaseous envelope, continue to receive additional confirmation in proportion to the number, sagacity, and activity of observers. Lieutenant Maury, whose labours have so greatly enlarged our knowledge of the physical conditions of the ocean, has especially contributed to establish the truth of the above conclusion. From whatever cause this superioriety of oceanic temperature may arise, it imparts additional interest to the problem of the influence of the sea on the climate of the land.

This question has been already treated by Humboldt in his essay on the causes of the inflexions of isothermal lines; but here I propose to examine it in a more general manner, and by following an order precisely the reverse of that which he has adopted.

In this way I have succeeded in establishing a general law relating to the distribution of isothermal lines, which does not appear to have been previously noticed, and which comprehends as particular cases such as had been already observed.

Let us conceive an island situated in either hemisphere of the globe, and let it be completely surrounded with water possessing nearly the same temperature all around the coast. The temperature of any place on the island will depend upon constant and fluctuating causes. The former are the temperature of space, and the extremely small but steady flux of heat from the interior of the earth through its outer crust. The latter are the heat it directly gains from the sun, what it loses by radiation, what it receives from warm and loses from cold currents of the atmosphere, what it obtains by the condensation of moisture and gives back by evaporation.

The four last sources of gain and loss are manifestly connected

with the conditions of the ocean in which the island is situated. If the surface of the ocean is warmer than the air over the island, the latter will gain in temperature by the interchange of currents of air over both. If we abstract all other causes, it is obvious that a point on the island would in this case be warmer, the closer it happened to be to the sea; in other words, its temperature would be a function of its distance from the coast. The isothermal lines of the island would be a series of nearly concentric curves, having some relation in their shapes to the outline of the coast. If the influence of the amount of heat gained by sunshine above what is lost by radiation be now considered, it appears in general that the positions and shapes of the isothermals will be changed.

This change may be represented by transporting the centres of the isothermals towards the nearest pole of the earth.

For if \( H \) represent the effective amount of heat gained by a point in the island, its expression will be made up of two principal terms, of which the first, as we have just seen, must be a function of the distance \( c \) from the coast. The second would obviously be a function of the latitude \( \lambda \), whether we take into account the absorption of the sun’s rays in passing through the atmosphere or not. In the latter and more simple case \( f(\lambda) \), can be found in terms of the latitude of the place, the sun’s longitude, the inclination of the ecliptic to the equator, and the excentricity of the earth’s orbit. I have treated the problem of isothermal lines with the form of \( f(\lambda) \) so found, and have arrived at the same conclusion as that which is here deduced, in a paper read before the Royal Irish Academy.

If we take into account the resistance of the atmosphere to the passage of sunshine through it, whatever knowledge we already possess shows that the loss of heat from this cause will increase with the obliquity of the sun’s rays, and therefore it will be such a function \( \phi(\lambda) \) as to possess the property of increasing with \( \lambda \), and its minimum value will be \( \phi(0) \).

We may therefore write

\[
H = F(c) + f(\lambda) - \phi(\lambda),
\]
or simply,

\[
H = F(c) + f(\lambda),
\]

with the conditions that \( F(c) \) continuously increases as \( c \) diminishes, down to \( c = 0 \); and that \( f(\lambda) \) continuously increases as \( \lambda \) diminishes, down to \( \lambda = 0 \); so that the maximum value of \( H \) would be

\[
F(0) + f(0).
\]

If another point whose distance from the coast is \( c_1 \), and lati-
of Heat over Islands.

tude \( \lambda_1 \), be situated on the same isothermal line, we must have

\[
H = F(c_1) + f(\lambda_1);
\]

whence

\[
F(c) - F(c_1) = f(\lambda_1) - f(\lambda).
\]

It follows from the foregoing conditions that this equation cannot subsist unless we have the inequalities

\[
\lambda < \lambda_1 \text{ and } c_1 < c,
\]

or

\[
\lambda > \lambda_1 \text{ and } c_1 > c.
\]

Hence that part of the isothermal which has the greatest latitude must be nearer to the coast than any other part, and that which has the smallest latitude must be the most remote from the coast. Isothermals which had been previously near the coast would now no longer be closed curves, at least within the island, and thus several might terminate on the coast as irregular arcs, with their convex sides turned towards the equator. If the dimensions of the island in the direction of the meridian were very great, the isothermals might all terminate on the coast.

If predominating currents of wind should blow from any point of the compass, it is likewise evident that they will further influence the position of the isothermals in a manner that can be represented by shifting them away from, or by moving them towards, the point of the wind, according as it happens to be a warm or a cold current.

As the surfaces of islands are usually not flat, but covered with eminences and depressions, and as the temperature of any point depends on its elevation above the sea, as well as on the other elements of its position, the transportation of the isothermals in an island would necessarily be accompanied by some change in their shapes, and thus, after transposition, they would not in general have the same detailed relations to the coast-line as in their concentric condition. As every piece of land, whether designated as a continent or as an island, is in reality surrounded by water, these views are capable of very general application. But, in the case of continents, as the oceans surrounding them do not possess the same temperature at different parts of their coasts, the function \( F(c) \) should receive different values for different places. In the case of islands of limited extent, we may, however, compare these views with observations without much difficulty.

It has been long recognized that the warm current, known to mariners as the Gulf-stream, bathes the shores of these islands as well as the greater part of the western coast of Europe. Very recently decisive evidence has been afforded of its calorific effects,
not merely upon our western seaboards, but all around the entire coast-line of the British islands. They are thus situated precisely under such conditions as naturally lead us to expect to find the distribution of heat over their surfaces such as would be indicated by groups of isothermal lines conforming to the law here adduced.

There are, in addition, some physical peculiarities in the structure of Ireland, which probably enhance the influence of the ocean upon the relative thermal conditions of its maritime and its inland portions. It is nearly surrounded at its coasts by ranges of mountains or lofty hills; and its interior consists chiefly of flat and low-lying plains. Eight such littoral ranges of elevated ground can be distinctly traced on a map of Ireland, each separated from its neighbour by some great outlet for the drainage waters of the interior. These elevated masses may be traced, 1, on the north-west coast in Donegal; 2, on the west in Mayo and Galway; 3, along the south-west coast in Kerry and Cork; 4, towards the South in Waterford; 5, on the east coast in Wicklow and Wexford; 6, on the north-east coast in Down; 7, towards the north-north-east in Antrim; and 8, near the north coast in Londonderry. The first of these groups trends principally from N.E. to S.W. The second from N. to S., with some nearly perpendicular offshoots. The fourth from E. to W.; the fifth from S.S.W. to N.N.E.; the sixth from S.W. to N.E.; the seventh is a rather irregular group of hills; the eighth trends from E. to W. It thus appears that the general direction of each of these groups is nearly parallel to the general direction of the adjoining coast. The only extensive line of coast which is not backed by a hilly or mountain barrier is that lying between the Wicklow and Mourne mountains.

Although it would be difficult to estimate the precise influence of these elevated masses in obstructing interchanges between the air surrounding the coast and that of the interior, they doubtlessly must produce some effect. It is at least obvious that moist and warm currents, encountering such masses, would lose with a portion of their moisture some of their heat, and would, on reaching the inland plains, be observed by the inhabitants colder as well as drier than the same winds had been noticed by those residing on the coast.

The considerable opening from the interior towards the sea, which has been just referred to, happens to be situated in the direction of those winds which blow least frequently in Ireland, and which at the same time possess the least moisture and warmth.

Although observations on temperature have hitherto been made at very few stations in Ireland, it is still possible to com-
pare what has been done with the principles here put forward. During the year 1851, a series of meteorological observations were conducted under the management of a committee of the Royal Irish Academy; and the stations, although not numerous, were fortunately so distributed as to enable us to combine their results in such a way as to clearly illustrate the thermal conditions of the island. The most important results are contained in the Rev. Dr. Lloyd’s valuable memoir on the Meteorology of Ireland.*

As to temperature, the observations had a twofold application: 1st, those referring to the temperature of the sea, and, 2ndly, those relating to the temperature of the air. A comparison of both classes of observations establishes the fact of an excess of temperature of the sea over the air, in 1851, amounting to 3°8 Fahrenheit. The mean temperature at Portrush, the most northern station, during the same year, was 49°1; and that at Castletownsend, the most southern, 52°. The excess of temperature of the sea over that of the air appears thus greater than the greatest difference of temperature which could arise between two stations as a consequence of their difference of latitude.

On comparing the mean annual temperatures of the sixteen stations where observations were made in 1851, I immediately saw that the following approximately isothermal groups could be formed.

<table>
<thead>
<tr>
<th>Mean temperature of isothermal group</th>
<th>Station</th>
<th>Temperature</th>
<th>Difference from isothermal</th>
<th>Remarks</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>Armagh</td>
<td>48°6</td>
<td>+0°2</td>
<td>Inland stations.</td>
</tr>
<tr>
<td></td>
<td>Markree</td>
<td>48°2</td>
<td>-0°2</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Athy.</td>
<td>48°4</td>
<td>0°0</td>
<td></td>
</tr>
<tr>
<td>49°05</td>
<td>Portrush</td>
<td>49°1</td>
<td>+0°05</td>
<td>North coast.</td>
</tr>
<tr>
<td></td>
<td>Buncrana</td>
<td>49°0</td>
<td>-0°05</td>
<td></td>
</tr>
<tr>
<td>50°26</td>
<td>Killough</td>
<td>50°2</td>
<td>-0°06</td>
<td>E. and N.E. coast.</td>
</tr>
<tr>
<td></td>
<td>Dublin</td>
<td>50°3</td>
<td>+0°04</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Courtown</td>
<td>50°3</td>
<td>+0°04</td>
<td></td>
</tr>
<tr>
<td>50°85</td>
<td>Kilrush</td>
<td>50°9</td>
<td>+0°05</td>
<td>Estuary of the Shannon.</td>
</tr>
<tr>
<td></td>
<td>Killybegs</td>
<td>50°8</td>
<td>-0°05</td>
<td>Donegal.</td>
</tr>
<tr>
<td>51°45</td>
<td>Dunmore</td>
<td>51°5</td>
<td>+0°05</td>
<td>Co. Waterford.</td>
</tr>
<tr>
<td></td>
<td>Westport</td>
<td>51°4</td>
<td>-0°05</td>
<td>Mayo.</td>
</tr>
<tr>
<td>52°2</td>
<td>Cahirciveen</td>
<td>52°1</td>
<td>-0°1</td>
<td>S.S.W. coast.</td>
</tr>
<tr>
<td></td>
<td>Castletownsend</td>
<td>52°3</td>
<td>+0°1</td>
<td></td>
</tr>
</tbody>
</table>

The remaining two stations, Donaghadee and Portarlington,

could not be included in any of these groups, as their temperatures were respectively 49°·6 and 47°·3.

Before I had made these combinations, Dr. Lloyd had remarked the singular difference between the temperature of the inland stations and those on the coast. In the small map which accompanies his memoir, the isothermal lines are deduced from the coast observations alone; and the author thus seems to indicate that these lines are intended to show the isothermals of the air over the sea surrounding Ireland, but not the distribution of temperature within the island itself. From the comparatively regular forms of such lines on the surface of the ocean, Dr. Lloyd assumed that over a small space they might be regarded as approximately straight; and he was thus enabled to apply the method of least squares for the precise determination of their positions.

As I could not assume any definite form for the interior isothermals, I could not use the method of least squares, and have had to lay them down by the ordinary method of tracing curve lines, each of which runs through a group of stations of equal temperature.

On looking at the map, it is apparent that, having first traced the isothermal of 52° which runs along the south-west coast near Cahirciveen and Castletownsend, the isothermal of the next lowest temperature must fall completely inside it, for two different isothermal lines cannot intersect. But the line of 51° runs from Mayo to Waterford, and is much longer than the line of 52°. The isothermal of 50° must in like manner be included within this, and it terminates on the north-east and north-west coast respectively. Thus we have three open isothermals, until we come to that of 49°, which runs along the north coast near Portrush and Buncrana. But as this must be included within the preceding, it would, if produced at each extremity, form a re-entrant curve. The isothermal of 48° passes within that of 49°, and through places not far from the inland stations, Armagh, Athy, and Markree*. The station of least temperature, Portarlington, should be situated within this line; and it actually does lie between Markree and Athy, although much nearer to the latter. Any other places possessing the same mean temperature as Portarlington must be situated within the isothermal of 48°; and an isothermal passing through Portarlington would be a closed curve. Donaghadee, on the north-east coast, lies, as should be expected, outside the isothermal of 49°, and inside the prolongation of the isothermal of 50°.

All these results completely harmonize with the views I have put forward; and I feel perfectly assured that further observa-

* See the Table of mean annual temperature for Ireland at the end.
tions will only confirm the truth of my general conclusions. In Ireland we thus perceive that the coldest district lies in the north-east portion of the midland counties—probably somewhere between the counties of Cavan and Tyrone, and that the warmest region is situated along the coasts of Kerry and Cork.

*Isothermals of the British Isles*.  

I now proceed to examine how far the distribution of heat over the island of Great Britain conforms to the general laws that have been shown to regulate it in Ireland. Here there are some remarkable conditions in the physical configuration of the island which should be kept in view in considering its thermal

* The figures denote the mean annual temperature, in degrees Fahrenheit, corresponding to each isothermal line.
conditions. The western coast alone is backed by any considerable masses of elevated land; and the interior is generally far from being so flat as the inland portions of Ireland. Its position with regard to the warmer and more prevailing winds, as well as the magnitude of the Bristol Channel, indicates that this great arm of the sea may exercise a remarkable influence on districts in the valley of the Severn which might otherwise possess a climate similar to places more strictly inland. Lastly, the much greater length of the island in the direction of the meridians, compared to its mean breadth in the direction of the parallels, would seem to point to a very different distribution of temperature from that which exists in Ireland.

The observations on the temperature of the sea recorded in Dr. Lloyd’s memoir show that, in the Irish Sea and St. George’s Channel, as well as in the Atlantic Ocean, the mean temperature of the surface-water exceeds that of the air. A similar result has been found by observations made on the surface of the German Ocean at Scarborough, although the excess of the water temperature above that of the atmosphere in this case is not so remarkable as in the former. It is to be hoped that Captain Woodall, who announced this interesting fact at the meeting of the British Association held in 1856 at Cheltenham, will publish the observations which have led him to a result so important.

As it thus appears to be placed beyond the possibility of reasonable doubt, that the shores of Great Britain, like those of Ireland, are washed by heat-bearing currents, I was prepared, notwithstanding the diversity of configuration of the two islands, to find some approach towards the re-entrant shape in the isotherms of the former. In order to construct these lines, I made use of the valuable Tables calculated by Professor Dove, and printed in the Report of the British Association for 1847, and of the quarterly returns of the state of the weather in England, published by Mr. Glaisher. I have also availed myself of some detached results which are not contained in either of these collections. In Mr. Glaisher’s returns will be found the names of the observers to whose patience and devotion to science we owe many of these interesting facts.

Having calculated the mean annual temperatures for more than fifty places not already in Dove’s Table, and corrected a few of his results by the aid of subsequent observations, I combined them in approximately isothermal groups, precisely as I had already done in the case of Ireland. The isothermal lines laid down in the Map which accompanies this essay, have been drawn by the aid of such indications, most weight being attached to the results of the best observations. These lines are not intended to represent with perfect accuracy the mean annual dis-
tribution of temperature, because in some extensive districts
observations are entirely wanting, although made in great num-
bers in certain localities. Although these isothermals may thus
require some ulterior modifications, I have no reason to doubt
their faithfulness in presenting a general view of the distribution
of temperature. The influence of distance from the coast, as
well as of latitude, is very distinctly visible; and the arrange-
ment of the isothermals evidently conforms to the same laws as
those of Ireland.

The mean temperatures of stations where observations have
been made have been recorded usually in connexion with certain
coordinates of each station, namely, its latitude, longitude, and
elevation above the level of the sea.

While the influence of latitude and longitude is now more
justly estimated, that of elevation above the sea is sometimes
liable to be overrated. The decrease of temperature with height
has generally been calculated from observations made on the
sides and summits of mountains, or by comparison of the thermal
conditions of successively overlying portions of the atmosphere
during the voyages of balloons. But a much slower rate of
decrease must take place in the temperature of the air which
touches gradually-rising and widely-spread surfaces, than along
nearly vertical lines or rapidly-ascending planes. The manner
in which the air becomes heated by contact with the ground is
sufficient to indicate the truth of this conclusion. Thus, although
some of the best determinations from observations made in bal-
loons give a decrease of 1° Fahr. for 276 feet, the decrease of
temperature on the sides of mountains has been estimated at 1°
in 355 feet, and at 1° in 433 feet on the surfaces of elevated
plains*.

The mean elevation of the greater number even of the inland
stations of Great Britain whose temperature has been deter-
mined is probably less than 200 feet; and almost all such
stations are situated on gently undulating or low-lying grounds.
The highest station in Ireland is only 230 feet above the level
of the sea, and it is situated in the great central plain of the
island.

As the heights of most of the inland and coast stations in
Ireland have been determined with more than ordinary care by
actual levelling, we may estimate with remarkable precision the
elevation corresponding to a decrease of temperature of one
degree, on the supposition that vertical height above the sea,
and not horizontal distance from its surface, produces the low
temperature of the interior as compared with that of the regions

* 195 metres and 235 metres for 1° C.—Kämtz, Meteorologie, p. 215,
French edition.
bordering on the coast. For this purpose we should manifestly compare a group of inland with a group of coast stations nearly on the same parallel of latitude. The following are thus selected from the Table of mean annual temperature of Ireland:—

**Inland Stations.**

<table>
<thead>
<tr>
<th></th>
<th>Latitude</th>
<th>Temperature</th>
<th>Elevation in feet.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Armagh</td>
<td>54°21'</td>
<td>47°-8</td>
<td>211</td>
</tr>
<tr>
<td>Markree</td>
<td>54 12</td>
<td>47°-8</td>
<td>132</td>
</tr>
<tr>
<td>Portarlington</td>
<td>53 9</td>
<td>47°-0</td>
<td>230</td>
</tr>
<tr>
<td>Limerick</td>
<td>52 40</td>
<td>49°-4</td>
<td>92</td>
</tr>
<tr>
<td><strong>Means</strong></td>
<td>53 35</td>
<td>48°-0</td>
<td>166</td>
</tr>
</tbody>
</table>

**Coast Stations.**

<table>
<thead>
<tr>
<th></th>
<th>Latitude</th>
<th>Temperature</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Killybegs</td>
<td>54°34'</td>
<td>50°-5</td>
<td>20</td>
</tr>
<tr>
<td>Killough</td>
<td>54 13</td>
<td>49°-9</td>
<td>23</td>
</tr>
<tr>
<td>Dublin</td>
<td>53 21</td>
<td>50°-0</td>
<td>19</td>
</tr>
<tr>
<td>Kilrush</td>
<td>52 38</td>
<td>50°-8</td>
<td>45</td>
</tr>
<tr>
<td><strong>Means</strong></td>
<td>53 41</td>
<td>50°-3</td>
<td>27</td>
</tr>
</tbody>
</table>

The mean latitude of the coast stations is 6° greater than that of the inland stations, and is thus slightly unfavourable to an excess of temperature; yet the mean temperature of the former exceeds that of the latter by 2°-3. The mean difference in height is 189 feet; consequently, if the greater warmth of the coast were due to difference of elevation alone, the rate of decrease of atmospheric temperature in ascending over ground lying at an almost insensible mean inclination to the horizon, would be 1° for 60 feet. A supposition which leads to a result so discordant with the best observations must be abandoned as altogether untenable.

Hitherto, distance from the sea has but rarely entered into temperature tables, and then only for a limited number of stations. It now appears to possess claims to be definitively considered as a fourth coordinate, and in the case of most of the stations in the British islands it is undoubtedly the most important after latitude. In the accompanying Tables a column has been accordingly appended, in which the value of this element is given in English miles. The distances are all taken from the maps published by the Society for the Diffusion of Useful Knowledge. When a distance is taken from the mouth of a river or estuary, it is generally not counted from the nearest point of the coast, but from some point about midway between the opposite shores of the inlet.

The mean temperatures, marked D, are taken from Dove's Table; those marked n, I have calculated entirely from other sources; those unmarked are combinations of such results together
with those contained in Professor Dove's Tables. In the Table which exhibits the mean annual temperature of twenty-three stations in Ireland, the results marked L are given on Dr. Lloyd's authority. He estimated these numbers from the observations of 1851, by subtracting 0°.3 from each, because in Dublin the temperature of the year was in excess of the mean of twelve years by that quantity. The mean temperatures so deduced are probably much closer to their true values than the uncorrected results of 1851. This may be noticed with reference to stations where observations were made during other years, such as Armagh and Markree, where the temperature is regularly recorded at the astronomical observatories of Dr. Robinson and Mr. Cooper.

The observations at Cove were made by Dr. Scott; at Cork, by Mr. Humphreys and his predecessors at the Royal Institution, also at the barracks; at Limerick and Kilrush, in connexion with the Ordnance Survey; at Derry, by Mr. Patterson; at Waterford, by Dr. Cavet *; at Belfast, under the superintendence of Professor Stevelly at the College.

The isothermals of the northern portions of the two great continents which constitute the old and new worlds, exhibit, at least during the winter, very decided conformity to the same laws as those which are so manifest in the British Isles.

On examining Professor Dove's large map of the isothermals of January and July (No. 3 in General Sabine's edition), it appears that during the former month the isothermals which traverse North America run very nearly parallel to its western coast; then bending, they run nearly parallel to the northern shores of the Gulf of Mexico; afterwards they ascend towards the north-east, and cut the eastern coast of the United States, Canada, and Labrador at very oblique inclinations. In Europe, those which approach the sea appear in general to be parallel to the coast, as in Scandinavia and on the western and southern coasts of France. In Asia, some of the isothermals approach in shape a rude outline of that great division of our continent, and many appear to cut the northern coast of Siberia almost at right angles to the parallels of latitude, precisely in the same manner as some of those in the British islands, which are not re-entrant curves.

These results indicate the existence of the two poles of minimum temperature suggested by Sir David Brewster †; but a more general conclusion may be drawn from the entire of the preceding reflections,—namely, that there are nearly as many poles of minimum temperature upon the globe as there are islands and continents distributed over its surface.

* Wilde, in the Reports of the Census Commissioners. Tables of Deaths, vol. i.
† See note †, p. 242.
Table of the Mean Annual Temperature of Great Britain*.

<table>
<thead>
<tr>
<th>Station</th>
<th>No. of years</th>
<th>Mean annual temperature,</th>
<th>Latitude, North.</th>
<th>Longitude, West.</th>
<th>Elevation above sea.</th>
<th>Distance from sea.</th>
<th>Sea from which the distance is calculated</th>
</tr>
</thead>
<tbody>
<tr>
<td>D Calenik</td>
<td>5</td>
<td>52.0 ± 0.50</td>
<td>5 10</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel, South coast of Cornwall.</td>
</tr>
<tr>
<td>n Ventnor (Isle of Wight)</td>
<td>6</td>
<td>52.5 ± 0.50</td>
<td>1 13</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>Helston</td>
<td>13</td>
<td>52.1 ± 0.50</td>
<td>5 18</td>
<td></td>
<td>1</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>D Sidmouth</td>
<td>3</td>
<td>52.1 ± 0.50</td>
<td>3 13</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>Plymouth</td>
<td>7</td>
<td>52.0 ± 0.50</td>
<td>4 7</td>
<td>75</td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>D Penzance</td>
<td>21</td>
<td>51.8 ± 0.50</td>
<td>5 33</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>D Gosport</td>
<td>16</td>
<td>51.8 ± 0.50</td>
<td>1 7</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>Falmouth</td>
<td>9</td>
<td>51.6 ± 0.50</td>
<td>5 6</td>
<td></td>
<td>2</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>D Southwick</td>
<td>11</td>
<td>51.5 ± 52.30</td>
<td>-1 25</td>
<td></td>
<td>10</td>
<td>0</td>
<td>German Ocean.</td>
</tr>
<tr>
<td>Swansea</td>
<td>6</td>
<td>51.2 ± 51.36</td>
<td>3 53</td>
<td></td>
<td>0</td>
<td>0</td>
<td>St. George's Channel.</td>
</tr>
<tr>
<td>n Maidstone</td>
<td>2</td>
<td>51.2 ± 51.16</td>
<td>-3 80</td>
<td>16</td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>n Torquay</td>
<td>6</td>
<td>51.0 ± 50.25</td>
<td>3 30</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>Uckfield</td>
<td>2</td>
<td>50.9 ± 50.58</td>
<td>0 5</td>
<td>150</td>
<td>14</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>n Gloucester</td>
<td>1</td>
<td>50.9 ± 51.52</td>
<td>2 14</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>Truro</td>
<td>7</td>
<td>50.8 ± 50.16</td>
<td>3 28</td>
<td>53</td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>Bristol</td>
<td>13</td>
<td>50.7 ± 51.27</td>
<td>2 36</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>n Ryde (I.of Wight)</td>
<td>6</td>
<td>50.7 ± 50.43</td>
<td>1 11</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>M Lyme Regis</td>
<td>13</td>
<td>50.7 ± 50.43</td>
<td>2 56</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>Liverpool</td>
<td>28</td>
<td>50.6 ± 53.25</td>
<td>2 59</td>
<td></td>
<td>0</td>
<td>0</td>
<td>Liverpool Bay.</td>
</tr>
<tr>
<td>London</td>
<td>61</td>
<td>50.5 ± 51.30</td>
<td>0 5</td>
<td></td>
<td>38</td>
<td>0</td>
<td>Mouth of Thames.</td>
</tr>
<tr>
<td>n Hastings</td>
<td>2</td>
<td>50.2 ± 50.53</td>
<td>0 35</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>n Teignmouth</td>
<td>4</td>
<td>50.1 ± 50.33</td>
<td>3 55</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>n Penarrow</td>
<td>4</td>
<td>50.0 ± 50.44</td>
<td>4 32</td>
<td>220</td>
<td>14</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>n Exeter</td>
<td>11</td>
<td>50.0 ± 51.12</td>
<td>4 39</td>
<td></td>
<td>0</td>
<td>0</td>
<td>Irish Sea.</td>
</tr>
<tr>
<td>n Douglas (I.of Man)</td>
<td>11</td>
<td>50.0 ± 51.12</td>
<td>4 39</td>
<td></td>
<td>0</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>D Chiswick</td>
<td>16</td>
<td>49.9 ± 51.29</td>
<td>0 18</td>
<td></td>
<td>42</td>
<td>0</td>
<td>Mouth of Thames.</td>
</tr>
<tr>
<td>n Southampton</td>
<td>5</td>
<td>49.8 ± 50.54</td>
<td>1 24</td>
<td></td>
<td>9</td>
<td>0</td>
<td>Mouth of Thames.</td>
</tr>
<tr>
<td>Cheltenham</td>
<td>16</td>
<td>49.8 ± 51.54</td>
<td>2 4</td>
<td></td>
<td>34</td>
<td>0</td>
<td>British Channel, mouth of Southampton Water.</td>
</tr>
<tr>
<td>n Newport (Isle of Wight)</td>
<td>5</td>
<td>49.5 ± 50.42</td>
<td>1 19</td>
<td></td>
<td>4</td>
<td>0</td>
<td>British Channel, mouth of Southampton Water.</td>
</tr>
<tr>
<td>Chichester</td>
<td>5</td>
<td>49.5 ± 50.52</td>
<td>0 45</td>
<td></td>
<td>5</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>n Lewisham</td>
<td>6</td>
<td>49.5 ± 51.26</td>
<td>0 2</td>
<td></td>
<td>34</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>D Cobham</td>
<td>1</td>
<td>49.5 ± 51.20</td>
<td>0 23</td>
<td></td>
<td>36</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>D Bushey Heath</td>
<td>8½</td>
<td>49.3 ± 51.38</td>
<td>0 22</td>
<td></td>
<td>50</td>
<td>0</td>
<td>Mouth of Thames.</td>
</tr>
<tr>
<td>D Tottenham</td>
<td>25</td>
<td>49.2 ± 51.36</td>
<td>0 5</td>
<td></td>
<td>32</td>
<td>0</td>
<td>Mouth of Thames.</td>
</tr>
<tr>
<td>D Bolton</td>
<td>10</td>
<td>49.1 ± 53.35</td>
<td>2 24</td>
<td></td>
<td>25</td>
<td>0</td>
<td>Mouth of Thames.</td>
</tr>
<tr>
<td>n Worthing</td>
<td>6</td>
<td>49.0 ± 50.48</td>
<td>0 22</td>
<td></td>
<td>30</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>n Greenwich</td>
<td>10</td>
<td>49.0 ± 51.20</td>
<td>0 22</td>
<td>156</td>
<td>0</td>
<td>0</td>
<td>Mouth of Thames.</td>
</tr>
<tr>
<td>n Worcester</td>
<td>3</td>
<td>49.0 ± 52.12</td>
<td>2 18</td>
<td>125</td>
<td>50</td>
<td>0</td>
<td>British Channel.</td>
</tr>
<tr>
<td>Boston</td>
<td>21</td>
<td>49.0 ± 52.48</td>
<td>0 5</td>
<td></td>
<td>7</td>
<td>0</td>
<td>The Wash beyond the Shallows.</td>
</tr>
<tr>
<td>Whitehaven</td>
<td>16</td>
<td>49.0 ± 54.33</td>
<td>3 33</td>
<td></td>
<td>0</td>
<td>0</td>
<td>The Irish Sea.</td>
</tr>
<tr>
<td>n Hull</td>
<td>10</td>
<td>49.0 ± 53.33</td>
<td>0 20</td>
<td></td>
<td>12</td>
<td>0</td>
<td>Mouth of the Humber.</td>
</tr>
<tr>
<td>n Bicester</td>
<td>5</td>
<td>48.9 ± 51.52</td>
<td>1 10</td>
<td></td>
<td>72</td>
<td>0</td>
<td>British Channel.</td>
</tr>
</tbody>
</table>

* The temperatures are given in degrees of Fahrenheit’s scale. The longitudes are counted from the meridian of Greenwich; those with the sign — prefixed are cast, and those without any sign are west.
<table>
<thead>
<tr>
<th>Station</th>
<th>No. of years</th>
<th>Mean annual temperature</th>
<th>Latitude, North.</th>
<th>Longitude, West.</th>
<th>Elevation above the sea.</th>
<th>Distance from sea (miles)</th>
<th>Sea from which the distance is counted</th>
</tr>
</thead>
<tbody>
<tr>
<td>D Crumpsal</td>
<td>8</td>
<td>48°53'32&quot;</td>
<td>2°14'</td>
<td>66</td>
<td>35</td>
<td>Irish Sea at the mouth of the Ribble.</td>
<td></td>
</tr>
<tr>
<td>n Keyingham</td>
<td>18</td>
<td>48°53'33&quot;</td>
<td>0°5'</td>
<td></td>
<td>7</td>
<td>Mouth of the Humber.</td>
<td></td>
</tr>
<tr>
<td>Manchester</td>
<td>49</td>
<td>48°53'29&quot;</td>
<td>2°14'</td>
<td></td>
<td>32</td>
<td>Liverpool Bay.</td>
<td></td>
</tr>
<tr>
<td>n Enfield</td>
<td>5</td>
<td>48°51'41&quot;</td>
<td>0°5'</td>
<td></td>
<td>39</td>
<td>Mouth of the Thames.</td>
<td></td>
</tr>
<tr>
<td>n Hartwell</td>
<td>7</td>
<td>48°51'49&quot;</td>
<td>0°50'</td>
<td></td>
<td>70</td>
<td>Mouth of the Southampton Water.</td>
<td></td>
</tr>
<tr>
<td>n Bedford</td>
<td>7</td>
<td>48°52'8&quot;</td>
<td>0°30'</td>
<td></td>
<td>64</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>D Lyndon</td>
<td>28</td>
<td>48°52'32&quot;</td>
<td>-3°510</td>
<td></td>
<td>32</td>
<td>Mouth of the Blackwater in the German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Royston</td>
<td>6</td>
<td>48°65'2&quot;</td>
<td>0°1&quot;</td>
<td></td>
<td>47</td>
<td>Month of Frith of Forth, German Ocean.</td>
<td></td>
</tr>
<tr>
<td>D St. Andrew's</td>
<td>8</td>
<td>48°56'21&quot;</td>
<td>2°48'</td>
<td>70</td>
<td>65</td>
<td>British Channel at Southampton Water.</td>
<td></td>
</tr>
<tr>
<td>n Oxford</td>
<td>9</td>
<td>48°51'46&quot;</td>
<td>1°16'</td>
<td></td>
<td>0</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Aberdeen</td>
<td>10</td>
<td>48°47'9&quot;</td>
<td>2°5'</td>
<td>50</td>
<td>0</td>
<td>Frith of Forth.</td>
<td></td>
</tr>
<tr>
<td>D Leith</td>
<td>2</td>
<td>48°45'59&quot;</td>
<td>3°10'</td>
<td></td>
<td>68</td>
<td>British Channel at Southampton Water.</td>
<td></td>
</tr>
<tr>
<td>D Hawkhill</td>
<td>3</td>
<td>48°45'58&quot;</td>
<td>3°10'</td>
<td></td>
<td>64</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Thame</td>
<td>1</td>
<td>48°35'45&quot;</td>
<td>0°58'</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Cardington (near Bedford)</td>
<td>7</td>
<td>48°35'2&quot;</td>
<td>0°30'</td>
<td></td>
<td>17</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Norwich</td>
<td>7</td>
<td>48°25'37&quot;</td>
<td>-1°18'</td>
<td>32</td>
<td>12</td>
<td>Cardigan Bay; hills intervening.</td>
<td></td>
</tr>
<tr>
<td>n Lampeter</td>
<td>3</td>
<td>48°25'2&quot;</td>
<td>4°43°</td>
<td>420</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Bywell</td>
<td>2</td>
<td>48°25'47&quot;</td>
<td>1°54'</td>
<td></td>
<td>20</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Bute</td>
<td>7</td>
<td>48°15'48&quot;</td>
<td>5°2'</td>
<td></td>
<td>2</td>
<td>Frith of Clyde.</td>
<td></td>
</tr>
<tr>
<td>n Rosehill (near Oxford)</td>
<td>7</td>
<td>48°15'46&quot;</td>
<td>1°16'</td>
<td>270</td>
<td>65</td>
<td>British Channel at Southampton Water.</td>
<td></td>
</tr>
<tr>
<td>n Hartwell Rectory</td>
<td>6</td>
<td>48°15'49&quot;</td>
<td>0°51'</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Ackworth</td>
<td>18</td>
<td>48°15'39&quot;</td>
<td>1°20'</td>
<td></td>
<td>70</td>
<td>Mouth of Southampton Water.</td>
<td></td>
</tr>
<tr>
<td>D Anatomical Gardens [St. Andrew's?]</td>
<td>7</td>
<td>48°05'24&quot;</td>
<td>2°48'</td>
<td></td>
<td>52</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Holkham</td>
<td>7</td>
<td>48°05'25&quot;</td>
<td>-0°46'</td>
<td></td>
<td>0</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Hawarden</td>
<td>7</td>
<td>48°05'10&quot;</td>
<td>3°1200</td>
<td></td>
<td>15</td>
<td>Mouth of the Dee.</td>
<td></td>
</tr>
<tr>
<td>n Grantham</td>
<td>7</td>
<td>47°9'52&quot;</td>
<td>0°38'</td>
<td></td>
<td>30</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>York</td>
<td>10</td>
<td>47°9'53&quot;</td>
<td>1°5'</td>
<td></td>
<td>27</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Berkhamstead</td>
<td>3</td>
<td>47°8'51&quot;</td>
<td>0°35'</td>
<td></td>
<td>61</td>
<td>Mouth of the Thames.</td>
<td></td>
</tr>
<tr>
<td>n Llandudno</td>
<td>4</td>
<td>47°8'51&quot;</td>
<td>0°42'</td>
<td></td>
<td>62</td>
<td>Mouth of the Thames.</td>
<td></td>
</tr>
<tr>
<td>n Nottingham</td>
<td>7</td>
<td>47°8'52&quot;</td>
<td>14°181</td>
<td></td>
<td>60</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Wakefield</td>
<td>7</td>
<td>47°8'53&quot;</td>
<td>1°29'</td>
<td></td>
<td>59</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Malvern</td>
<td>1</td>
<td>47°7'52&quot;</td>
<td>2°19'</td>
<td></td>
<td>40</td>
<td>Bristol Channel.</td>
<td></td>
</tr>
<tr>
<td>n Newcastle</td>
<td>1</td>
<td>47°7'54&quot;</td>
<td>1°36'</td>
<td></td>
<td>9</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Eign (Hereford)</td>
<td>1</td>
<td>47°6'52&quot;</td>
<td>2°42'</td>
<td></td>
<td>34</td>
<td>Bristol Channel.</td>
<td></td>
</tr>
<tr>
<td>n Alderley Edge</td>
<td>1</td>
<td>47°6'53&quot;</td>
<td>2°17'</td>
<td></td>
<td>34</td>
<td>Liverpool Bay.</td>
<td></td>
</tr>
<tr>
<td>D New Malton</td>
<td>8</td>
<td>47°6'54&quot;</td>
<td>0°47'</td>
<td>85</td>
<td>18</td>
<td>German Ocean.</td>
<td></td>
</tr>
<tr>
<td>n Clifton</td>
<td>5</td>
<td>47°5'51&quot;</td>
<td>2°26'</td>
<td></td>
<td>8</td>
<td>Bristol Channel.</td>
<td></td>
</tr>
<tr>
<td>Station</td>
<td>No. of years</td>
<td>Mean annual temperature</td>
<td>Longitude, North.</td>
<td>Latitude, West.</td>
<td>Elevation above sea.</td>
<td>Distance from sea.</td>
<td>Sea from which the distance is counted</td>
</tr>
<tr>
<td>------------------</td>
<td>--------------</td>
<td>-------------------------</td>
<td>-------------------</td>
<td>-----------------</td>
<td>---------------------</td>
<td>-------------------</td>
<td>--------------------------------------</td>
</tr>
<tr>
<td>n Gainsborough</td>
<td>5</td>
<td>47.5 53 24</td>
<td>0 47</td>
<td>34</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Colinton</td>
<td>5</td>
<td>47.4 55 55</td>
<td>3 16</td>
<td>53</td>
<td>Frith of Forth.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Clunie</td>
<td>16</td>
<td>47.3 57 12</td>
<td>2 35</td>
<td>20</td>
<td>Solway Frith.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Keswick</td>
<td>4.5</td>
<td>47.3 54 33</td>
<td>3 9</td>
<td>18</td>
<td>Irish Sea; mountains intervening.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>u Glasgow</td>
<td>3</td>
<td>47.3 55 51</td>
<td>4 14</td>
<td>25</td>
<td>Frith of Clyde.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>u Derby</td>
<td>9</td>
<td>47.2 52 58</td>
<td>1 30 160</td>
<td>68</td>
<td>German Ocean, near the Wash.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>u Edinburgh</td>
<td>18</td>
<td>47.2 55 58</td>
<td>3 11</td>
<td>2</td>
<td>Frith of Forth.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Scarborough</td>
<td>3</td>
<td>47.1 54 17</td>
<td>0 23</td>
<td>0</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Warrington</td>
<td>3</td>
<td>47.0 53 24</td>
<td>2 36</td>
<td>17</td>
<td>Liverpool Bay.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Kendal</td>
<td>13</td>
<td>47.0 54 17</td>
<td>2 46</td>
<td>13</td>
<td>Morcambe Bay.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Elgin</td>
<td>4</td>
<td>47.0 57 38</td>
<td>3 16</td>
<td>4</td>
<td>North Sea.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Carlisle</td>
<td>24</td>
<td>47.0 54 54</td>
<td>2 58</td>
<td>16</td>
<td>Irish Sea.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Wick</td>
<td>2</td>
<td>46.9 58 29</td>
<td>3 5</td>
<td>1</td>
<td>North Sea.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Knutsford</td>
<td>10</td>
<td>46.9 53 20</td>
<td>2 20</td>
<td>26</td>
<td>Liverpool Bay.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Kincaun's Castle</td>
<td>22</td>
<td>46.9 56 23</td>
<td>3 19 140</td>
<td>22</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Belvoir Castle</td>
<td>3</td>
<td>46.8 52 54</td>
<td>0 46</td>
<td>40</td>
<td>German Ocean, at the Wash.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>u Carbeth</td>
<td>4</td>
<td>46.8 56 6</td>
<td>4 22 480</td>
<td>26</td>
<td>Frith of Clyde.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Knechworth</td>
<td>3</td>
<td>46.7 51 49</td>
<td>0 13</td>
<td>51</td>
<td>Mouth of the Thames.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Lancaster</td>
<td>7</td>
<td>46.4 54 3</td>
<td>2 48</td>
<td>6</td>
<td>Morecambe Bay.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Durham</td>
<td>3</td>
<td>46.4 54 46</td>
<td>1 37 352</td>
<td>10</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Stonyhurst</td>
<td>7</td>
<td>46.3 53 52</td>
<td>2 23</td>
<td>22</td>
<td>Morecambe Bay.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Dunino</td>
<td>4</td>
<td>46.3 56 16</td>
<td>2 49 250</td>
<td>4</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Sandwich(Orkney)</td>
<td>5.5</td>
<td>46.2 59 5</td>
<td>3 17 100</td>
<td>0</td>
<td>Atlantic Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>u Allenheads</td>
<td>2</td>
<td>46.2 54 49</td>
<td>2 16</td>
<td>46</td>
<td>Solway Frith.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Makerston</td>
<td>2</td>
<td>46.1 55 35</td>
<td>2 31 211</td>
<td>26</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>u North Shields</td>
<td>6</td>
<td>46.0 55 0</td>
<td>0 25</td>
<td>1</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Inverness</td>
<td>2</td>
<td>45.8 57 30</td>
<td>4 12</td>
<td>13</td>
<td>Moray Frith.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Anstruther</td>
<td>1</td>
<td>45.7 56 15</td>
<td>2 41</td>
<td>0</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Applegarth</td>
<td>19</td>
<td>45.6 55 13</td>
<td>3 12 170</td>
<td>12</td>
<td>Solway Frith.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D High Wycombe</td>
<td>4</td>
<td>45.5 51 36</td>
<td>0 35</td>
<td>60</td>
<td>English Channel.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>u Arbrought</td>
<td>4</td>
<td>45.5 56 34</td>
<td>2 34</td>
<td>0</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Dunfermline</td>
<td>20</td>
<td>45.2 56 5</td>
<td>3 26</td>
<td>4</td>
<td>Frith of Forth.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Alford</td>
<td>10</td>
<td>45.1 57 13</td>
<td>2 45 420</td>
<td>28</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>u Laurencekirk</td>
<td>1</td>
<td>44.6 56 50</td>
<td>2 30 140</td>
<td>54</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Bonally</td>
<td>5</td>
<td>44.2 55 56</td>
<td>3 16 1100</td>
<td>6</td>
<td>Frith of Forth.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D Leadhills</td>
<td>10</td>
<td>44.1 53 25</td>
<td>3 48 1280</td>
<td>33</td>
<td>Irish Channel.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>D St. Bathan's</td>
<td>1</td>
<td>43.9 55 52</td>
<td>2 23 420</td>
<td>64</td>
<td>German Ocean.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>n Kingussie</td>
<td>1</td>
<td>42.8 57 4</td>
<td>4 5 750</td>
<td>40</td>
<td>Moray Frith.</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table of the Mean Annual Temperature of Ireland.

<table>
<thead>
<tr>
<th>Station</th>
<th>No. of years</th>
<th>Mean annual temperature</th>
<th>Elevation above the sea, feet</th>
<th>Distance from sea, miles</th>
</tr>
</thead>
<tbody>
<tr>
<td>L Calhircven</td>
<td>1</td>
<td>52.0±31 56</td>
<td>10 12</td>
<td>52</td>
</tr>
<tr>
<td>L Castletownsend</td>
<td>1</td>
<td>51.8±31 33</td>
<td>9 9</td>
<td>18</td>
</tr>
<tr>
<td>Cove (Queenstown)</td>
<td>15</td>
<td>51.5±31 50</td>
<td>8 19</td>
<td>18</td>
</tr>
<tr>
<td>Cork*</td>
<td>30</td>
<td>51.5±31 51</td>
<td>8 20</td>
<td>18</td>
</tr>
<tr>
<td>L Westport</td>
<td>1</td>
<td>51.4±33 50</td>
<td>9 37</td>
<td>17</td>
</tr>
<tr>
<td>L Dunmore</td>
<td>1</td>
<td>51.3±32 38</td>
<td>9 30</td>
<td>45</td>
</tr>
<tr>
<td>Kilrush</td>
<td>2</td>
<td>50.8±32 38</td>
<td>9 20</td>
<td>66</td>
</tr>
<tr>
<td>L Kellybogs</td>
<td>1</td>
<td>50.5±34 34</td>
<td>8 27</td>
<td>10</td>
</tr>
<tr>
<td>L Courtown</td>
<td>1</td>
<td>50.0±32 39</td>
<td>6 13</td>
<td>40</td>
</tr>
<tr>
<td>L Dublin</td>
<td>12</td>
<td>50.0±33 21</td>
<td>6 15</td>
<td>19</td>
</tr>
<tr>
<td>L Killough</td>
<td>1</td>
<td>49.9±34 13</td>
<td>5 40</td>
<td>23</td>
</tr>
<tr>
<td>L Limerick</td>
<td>3</td>
<td>49.4±35 40</td>
<td>8 38</td>
<td>92</td>
</tr>
<tr>
<td>L Donaghadee</td>
<td>1</td>
<td>49.3±35 38</td>
<td>5 33</td>
<td>16</td>
</tr>
<tr>
<td>L Athy</td>
<td>14</td>
<td>47.8±34 21</td>
<td>6 39</td>
<td>30</td>
</tr>
<tr>
<td>Armagh</td>
<td>1</td>
<td>47.8±34 21</td>
<td>6 39</td>
<td>121</td>
</tr>
<tr>
<td>Antrim</td>
<td>13</td>
<td>47.8±34 43</td>
<td>6 8</td>
<td>132</td>
</tr>
<tr>
<td>Markree</td>
<td>14</td>
<td>47.8±34 14</td>
<td>8 28</td>
<td>45</td>
</tr>
<tr>
<td>L Portarlington</td>
<td>1</td>
<td>47.0±33 9</td>
<td>7 12</td>
<td>230</td>
</tr>
</tbody>
</table>

XXVII. On a New Base obtained by the Action of Ammonia on the Terbromide of Allyle. By Dr. MAXWELL SIMPSON†.

The terbromide of allyle, a body recently obtained by M. Wurtz by treating the iodide of allyle with an excess of bromine, is readily acted upon by ammonia, and forms with it a heavy oil, which has all the characters of a base, and is of remarkable constitution. For this new compound I propose the name dibromallylammonia. As the preparation of the hydrochlorate of the base must precede the preparation of the base itself, I propose to commence with the explanation of the process for obtaining the former body.

Formation of the Hydrochlorate of Dibromallylammonia.

To prepare this body, one volume of the terbromide is mixed with about six volumes of a solution of ammonia in weak alcohol; and

* The observations of every kind made at Cork embraced probably a longer period.
† Communicated by the Author.

the mixture is exposed in sealed tubes to the temperature of a water-bath for about ten or twelve hours. Decomposition commences almost immediately, and a large quantity of a white salt is precipitated, which I afterwards ascertained by analysis to be pure bromide of ammonium. At the expiration of the above-mentioned time the tubes were opened, and the bromide separated by filtration. On adding a large quantity of water to the filtrate it became turbid, and deposited after some time a heavy oil, which is the base in question, contaminated with some undecomposed terbromide. This was well washed with water, dissolved in dilute hydrochloric acid and filtered. The filtered liquor was then exposed to the temperature of 100°C till all the water had been driven off, and it ceased to evolve acid vapours. After the expulsion of the water the salt melted, but became solid again on cooling. To purify the salt it was now dissolved in water, filtered to remove a trace of oil, and evaporated to dryness again. It was then washed with ether, in which it is but little soluble, and finally dried over sulphuric acid in the vacuum of an air-pump. The salt thus prepared proved on analysis to be a species of hydrochlorate of ammonia, in which two equivalents of hydrogen are replaced by two equivalents of the monobasic radical. C₆H₄Br, thus:—

\[
\begin{align*}
\text{Hydrochlorate of} & \quad \text{Hydrochlorate of} \\
\text{ammonia.} & \quad \text{dibromallylammonia.}
\end{align*}
\]

Its formation may be thus explained:—

\[
\text{C}_6\text{H}_5\text{Br}_3 + \text{NH}_3 = \text{C}_6\text{H}_4\text{Br}_2 + \text{NH}_3, \text{HBr},
\]

and

\[
2(\text{C}_6\text{H}_4\text{Br}_2) + 3\text{NH}_3 = \text{N} \left\{ \begin{array}{c} \text{C}_6\text{H}_4\text{Br} \\ \text{H} \end{array} \right\} + 2(\text{NH}_3, \text{HBr}).
\]

The base dissolved in the hydrochloric acid gives the salt. The analysis of the salt gave the following numbers:—

I. 0·3911 grm. of salt gave 0·3505 grm. carbonic acid and 0·1285 water.

II. 0·4053 grm. of salt gave 0·3540 grm. carbonic acid and 0·1375 water.

III. 0·6905 grm. of salt gave 27·5 cubic centims. of moist nitrogen, barometer 760 millims., thermometer 12°C.

IV. 0·8585 grm. of salt, burnt with soda-lime, gave 0·3016 grm. platinum.

V. 0·4958 grm. of salt gave 0·2536 grm. chloride of silver.
the Action of Ammonia on the Terbromide of Allyle. 259

VI. 0·4160 grm. of salt, burnt with quicklime, gave 0·7208 grm. mixed chloride and bromide of silver.

Per-centage composition:

<table>
<thead>
<tr>
<th></th>
<th>I.</th>
<th>II.</th>
<th>III.</th>
<th>IV.</th>
<th>V.</th>
<th>VI.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbon</td>
<td>24·40</td>
<td>23·80</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hydrogen</td>
<td>3·64</td>
<td>3·76</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Nitrogen</td>
<td>...</td>
<td>4·77</td>
<td>4·98</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Chlorine</td>
<td>...</td>
<td>...</td>
<td>12·65</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bromine</td>
<td>...</td>
<td>...</td>
<td>52·80</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

These numbers agree with the formula given above, as will be seen by the following per-centage Table, in which the theoretical and experimental numbers are placed in juxtaposition:

<table>
<thead>
<tr>
<th></th>
<th>Theory.</th>
<th>Mean of experiment.</th>
</tr>
</thead>
<tbody>
<tr>
<td>12</td>
<td>Carbon</td>
<td>72·00</td>
</tr>
<tr>
<td>10</td>
<td>Hydrogen</td>
<td>10·00</td>
</tr>
<tr>
<td>1</td>
<td>Nitrogen</td>
<td>14·00</td>
</tr>
<tr>
<td>1</td>
<td>Chlorine</td>
<td>35·50</td>
</tr>
<tr>
<td>2</td>
<td>Bromine</td>
<td>160·00</td>
</tr>
</tbody>
</table>

The hydrochlorate of dibromallylammonia is of a light buff colour, extremely soluble in water and alcohol, slightly soluble in æther. It has a sweet, pungent, aromatic taste, which is highly characteristic. Long exposure to the temperature of 100° C. causes it to become dark in colour, probably from slight decomposition; nevertheless it is by no means an unstable body, as it requires a very high temperature to effect the destruction of the molecule. At the temperature of 160° C. it partially sublimes. On the addition of nitrate of silver to a solution of this salt in water, double decomposition ensues, and all the chlorine is precipitated in the form of chloride of silver: the bromine, however, is not precipitated, but remains in solution. This proves clearly that these two bodies occupy totally different positions in the molecule.

Bichloride of Platinum and Hydrochlorate of Dibromallylammonia.

I have been enabled to confirm the above formula, and to ascertain the atomic weight of the new body, by the analysis of the double salt which it forms with bichloride of platinum. This salt is readily prepared of great beauty and in a state of perfect

* The deficiency of bromine arose from the salt not having been perfectly dry. I found it very difficult to effect its complete desiccation in the air-pump.
purity, by mixing cold concentrated solutions of the organic salt and of bichloride of platinum. A bright orange-yellow precipitate soon separates, which must be well washed with absolute alcohol, in which it is almost insoluble, in order to remove the excess of bichloride. Specimens of the salt prepared at different times, and dried at 100° C., gave on analysis the following numbers:—

I. 0.5946 grm. of salt gave 0.3430 grm. carbonic acid and 0.1253 water.

II. 0.3611 grm. of salt gave 0.0771 grm. platinum.

III. 0.3620 grm. of salt gave 0.0766 grm. platinum.

IV. 0.2243 grm. of salt gave 0.0172 grm. platinum.

These numbers agree perfectly with the formula

$$N \left\{ \begin{array}{c} C_6 H^4 Br \\ C_6 H^4 Br \\ H \end{array} \right\} HCl + Pt Cl_2,$$

as will be seen from the following per-cent age Table:—

<table>
<thead>
<tr>
<th></th>
<th>Theory.</th>
<th>Experiment.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>I. 11. III. IV.</td>
</tr>
<tr>
<td>12 equivs. Carbon</td>
<td>72.00</td>
<td>15.61 15.73</td>
</tr>
<tr>
<td>10 &quot; Hydrogen</td>
<td>10.00</td>
<td>2.17 2.34</td>
</tr>
<tr>
<td>1 &quot; Nitrogen</td>
<td>14.00</td>
<td>3.03</td>
</tr>
<tr>
<td>3 &quot; Chlorine</td>
<td>106.50</td>
<td>23.10</td>
</tr>
<tr>
<td>2 &quot; Bromine</td>
<td>160.00</td>
<td>34.69</td>
</tr>
<tr>
<td>1 &quot; Platinum</td>
<td>98.70</td>
<td>21.40 ... 21.35 21.16 21.05</td>
</tr>
</tbody>
</table>

|       | 461.2 | 100.00 |

**Formation of Dibromallylammonia.**

When dilute potash is added to a solution of the hydrochlorate of dibromallylammonia, its base is set free, and precipitates after some time in the form of a heavy oil. This body I submitted to analysis, having previously washed it with water to remove adhering potash, and dried it in vacuo over sulphuric acid. It cannot be distilled without decomposition. The analysis gave the following results:—

I. 0.3432 grm. of base gave 0.3642 grm. carbonic acid and 0.1174 water.

II. 0.3588 grm. of base gave 0.3815 grm. carbonic acid and 0.1078 water.

III. 0.3206 grm. of base gave 0.3430 grm. carbonic acid and 0.1114 water.

These numbers correspond with the formula

$$N \left\{ \begin{array}{c} C_6 H^4 Br \\ C_6 H^4 Br \\ H \end{array} \right\},$$
as shown in the following Table:

<table>
<thead>
<tr>
<th></th>
<th>Theory</th>
<th>Experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>I.</td>
<td>II.</td>
</tr>
<tr>
<td>12 equivs Carbon</td>
<td>72·00</td>
<td>28·24</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>9</td>
<td>9·00</td>
<td>3·53</td>
</tr>
<tr>
<td>Hydrogen</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>14·00</td>
<td>5·49</td>
</tr>
<tr>
<td>Nitrogen</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>160·00</td>
<td>62·74</td>
</tr>
<tr>
<td>Bromine</td>
<td></td>
<td></td>
</tr>
<tr>
<td>255·00</td>
<td>100·00</td>
<td></td>
</tr>
</tbody>
</table>

The specimens analysed were the products of different preparations, and the quantity employed in the last analysis was precipitated from its salt by ammonia.

Dibromallylammonia is very sparingly soluble in water, but completely soluble in alcohol, and in sulphuric, muriatic, nitric, and acetic acids. Its taste is similar to that of the hydrochlorate. It fumes when brought into contact with hydrochloric acid, and behaves like an alkali towards litmus-paper. It is, however, a weak base, being incapable of precipitating copper or silver from their salts. It does not appear to have any great tendency to form crystallizable salts. The sulphate may be readily prepared by dissolving the base in dilute sulphuric acid, and removing the excess of acid by means of carbonate of baryta. On filtering and evaporating the solution, a gummy mass is obtained which has but little disposition to crystallize. Alcoholic solutions of this base and of chloride of mercury give, when mixed, an abundant white precipitate, which has probably a constitution analogous to that of the double chloride and amide of mercury, investigated by Sir Robert Kane.

**Action of Iodide of Æthyle on Dibromallylammonia, Æthylodibromallylammonia.**

In order to confirm the formula of the new base, and to gain additional insight into its constitution, I thought it desirable to ascertain whether or not it contained another equivalent of hydrogen capable of being replaced by an organic radical. With this view I resolved to try the action of iodide of æthyle upon it. The experiment was conducted in the following manner:—Two grammes of the base were mixed with a large excess of iodide of æthyle, and the mixture was introduced into a long tube, which was afterwards hermetically sealed. The base dissolved completely in the iodide; on heating the tube, however, in a water-bath, a thick oil, in which some crystals could be detected, soon separated and fell to the bottom of the tube. After twenty hours' heating, the tube was opened and the excess of iodide distilled off as completely as possible by means of a water-bath.
The residue, which was the hydriodate of the ethylated base, was then dissolved in warm water, in which it is sparingly soluble, and potash added to the solution. The ethylated base being set free, precipitated in the form of a heavy oil, which was well washed with water, dried over sulphuric acid in the vacuum of an air-pump and analysed. The aqueous solution from which the oil had separated, neutralized by nitric acid, gave, on the addition of nitrate of silver, an abundant precipitate of iodide of silver. For this base I propose the name ethyldibromallyl-ammonia.

The following results were obtained on analysis:

0.2432 grm. of base gave 0.3051 grm. carbonic acid and 0.1067 water.

The per-centagie composition calculated from these numbers agrees well with the formula \[ \text{N}\left\{\frac{C^6H^4Br}{C^6H^4Br} + \frac{C^4H^5}{C^4H^5}\right\} \], as shown by the following Table:

<table>
<thead>
<tr>
<th></th>
<th>Theory</th>
<th>Experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td>16 equivs.</td>
<td></td>
<td>34.21</td>
</tr>
<tr>
<td>Carbon</td>
<td>96.00</td>
<td>33.92</td>
</tr>
<tr>
<td>13</td>
<td>13.00</td>
<td>4.59</td>
</tr>
<tr>
<td>Hydrogen</td>
<td></td>
<td>4.87</td>
</tr>
<tr>
<td>1</td>
<td>14.00</td>
<td>4.94</td>
</tr>
<tr>
<td>Nitrogen</td>
<td></td>
<td>56.55</td>
</tr>
<tr>
<td>2</td>
<td>160.00</td>
<td>100.00</td>
</tr>
<tr>
<td>Bromine</td>
<td></td>
<td></td>
</tr>
<tr>
<td>283.00</td>
<td>1.00</td>
<td></td>
</tr>
</tbody>
</table>

The formation of this base may be thus explained:

\[ \text{N}\left\{\frac{C^6H^4Br}{C^6H^4Br} + \frac{C^4H^5}{C^4H^5}\right\} = \text{N}\left\{\frac{C^6H^4Br}{C^6H^4Br} + \frac{C^4H^5}{C^4H^5}\right\} \]

and

\[ \text{N}\left\{\frac{C^6H^4Br}{C^6H^4Br} + \frac{C^4H^5}{C^4H^5}\right\} \]

This body has an extremely bitter, pungent taste, and an odour somewhat resembling that of nutmegs. It is insoluble in water, but soluble in acids. It behaves like an alkali towards litmus-paper, and is a stronger base than that from which it is derived, as shown by its having the power of precipitating oxide of copper from its salts. I would gladly have tried to ethylate this base again, but was prevented by want of material.

With the view of ascertaining whether or not dibromallylammonia could be obtained from the brominated bromide of propylene \( (C^6H^5BrBr) \) derived from propylene gas, I exposed this body also to the action of an alcoholic solution of ammonia, under exactly the same conditions, as in the case of the tertbromide of
allyle. Crystals of bromide of ammonium were formed, but by no means in so great quantity as when the terbromide was employed, although I heated the tube containing the reacting bodies for a much longer time. On opening the tube and adding water to the alcoholic solution, a heavy oil separated, nearly equal in quantity to that originally employed. This oil proved to be quite insoluble in hydrochloric acid. Its boiling-point I found to be greatly inferior to that of the brominated bromide of propylene, the greater part of it passing over between 107° and 120° C. Nevertheless analysis showed that it did not differ much in composition from that body.

The following numbers were obtained on analysis:—

I. 0·3066 grm. of oil gave 0·1579 grm. carbonic acid and 0·0481 water.

II. 0·4081 grm. of oil gave 0·8052 grm. bromide of silver.

These numbers agree tolerably with the empirical formula $\text{C}_1^2 \text{H}_9 \text{Br}_5$, or $\left\{ \begin{array}{ll} 
\text{C}_6 \text{H}_4 \text{Br}_2 \\
\text{C}_6 \text{H}_5 \text{Br}_3 
\end{array} \right\}$ as the following Table shows:—

<table>
<thead>
<tr>
<th></th>
<th>Experiment.</th>
<th>Theory.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbon</td>
<td>14·96</td>
<td>14·04</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>1·87</td>
<td>1·74</td>
</tr>
<tr>
<td>Bromine</td>
<td>83·17</td>
<td>83·94</td>
</tr>
<tr>
<td></td>
<td>100·00</td>
<td></td>
</tr>
</tbody>
</table>

XXVIII. On the Constitution of Thermophyllite. By Augustus Beauchamp Northcote, F.C.S., Demonstrator to the Professor of Chemistry at Oxford*.

Among several new and rare minerals recently given by M. Nordenskiöld to Dr. Percy, was a specimen of the substance which M. Nordenskiöld has named Thermophyllite. No record of the composition of this mineral being in existence, I am indebted to Dr. Percy’s kindness in furnishing me with the requisite specimen for what I believe to be the first statement of its composition.

The mineral came from Hoponsuo; it is disseminated in aggregated masses of a brownish-grey colour and semi-translucent, through a rock which consists essentially of the same constituents as itself. In some parts, where disposed in thin laminae (the faces of the only perfect cleavage which it presents), it possesses a decidedly micaceous appearance. Notwithstanding this, however, attempts to ascertain its crystalline form have failed, and

* Communicated by the Author.
that even in the experienced hands of Professor Miller of Cambridge, who has kindly investigated this point for me. Every fracture, with the exception of the one above-mentioned cleavage, he found to be extremely irregular: the single face obtained was so curved as to render it impossible to obtain any distinct reflexion from it; and no natural faces were observable. Its hardness is between 1·5 and 2·0, and its specific gravity 2·61, as determined by M. Nordenskiöld’s observation. When heated before the blowpipe, this mineral swells up more than pyrophyllite, and assumes fantastic forms; it loses only 0·3 per cent. of its water when exposed to a heat of 100° C., and is scarcely acted on by hydrochloric acid, a small quantity of iron being almost all that is removed. Its analysis was therefore effected by fusion with an alkaline carbonate, and with the hydrate of barium for the determination of the sodium. The constituent elements were as follows:—sodium, magnesium, aluminium, iron, hydrogen, oxygen, silicon; and the atomic weights employed were, for sodium, 23; magnesium, 12; aluminium, 13·7; iron, 28; hydrogen, 1; oxygen, 8; and silicon, 22. Nordenskiöld has given yttrium as one of its components, an observation which I have been unable to verify. The following determinations were in the present instance made:

<table>
<thead>
<tr>
<th></th>
<th>I.</th>
<th>II.</th>
<th>Mean.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Soda</td>
<td>2·98</td>
<td>2·70</td>
<td>2·84</td>
</tr>
<tr>
<td>Magnesia</td>
<td>37·39</td>
<td>37·45</td>
<td>37·42</td>
</tr>
<tr>
<td>Water not expelled at 100° C.</td>
<td>10·73</td>
<td>10·43</td>
<td>10·58</td>
</tr>
<tr>
<td>Water expelled at 100° C.</td>
<td>0·32</td>
<td>0·28</td>
<td>0·30</td>
</tr>
<tr>
<td>Alumina</td>
<td>5·51</td>
<td>5·44</td>
<td>5·49</td>
</tr>
<tr>
<td>Silica</td>
<td>41·52</td>
<td>41·44</td>
<td>41·48</td>
</tr>
<tr>
<td>Protoxide of iron</td>
<td>1·71</td>
<td>1·47</td>
<td>1·59</td>
</tr>
</tbody>
</table>

The composition thus arrived at, when calculated to 100 parts with and without the water expelled at 100° C., is slightly altered as follows—the loss of water is so slight as to be obviously immaterial to the composition of the mineral:

<table>
<thead>
<tr>
<th></th>
<th>Including the 0·3 per cent. of water expelled at 100° C.</th>
<th>Excluding the 0·3 per cent. of water expelled at 100° C.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Soda</td>
<td>2·84</td>
<td>2·85</td>
</tr>
<tr>
<td>Magnesia</td>
<td>37·53</td>
<td>37·65</td>
</tr>
<tr>
<td>Alumina</td>
<td>5·50</td>
<td>5·52</td>
</tr>
<tr>
<td>Protoxide of iron.</td>
<td>1·59</td>
<td>1·60</td>
</tr>
<tr>
<td>Water</td>
<td>10·92</td>
<td>10·65</td>
</tr>
<tr>
<td>Silica</td>
<td>41·62</td>
<td>41·73</td>
</tr>
<tr>
<td></td>
<td>100·00</td>
<td>100·00</td>
</tr>
</tbody>
</table>
These results correspond closely with the following formula:

\[
49\text{MgO} \cdot \text{HO} + \begin{cases} 
37\text{MgO} \cdot \text{SiO}^3 \\
4\text{NaO} \cdot \text{SiO}^3 \\
\text{FeO} \cdot \text{SiO}^3 \\
\text{FeO} \cdot \text{Al}^2 \text{O}^3 \\
2\text{HO} \\
\text{FeO} \cdot \text{Al}^2 \text{O}^3 \\
4\text{HO} \cdot \text{Al}^2 \text{O}^3
\end{cases}
\]

which has the following percentage composition:

<table>
<thead>
<tr>
<th>Component</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Soda</td>
<td>2.70</td>
</tr>
<tr>
<td>Magnesia</td>
<td>37.40</td>
</tr>
<tr>
<td>Alumina</td>
<td>5.58</td>
</tr>
<tr>
<td>Protoxide of iron</td>
<td>1.56</td>
</tr>
<tr>
<td>Water</td>
<td>10.76</td>
</tr>
<tr>
<td>Silica</td>
<td>42.00</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100.00</strong></td>
</tr>
</tbody>
</table>

This formula may appear cumbersome; but if it be allowed that a true substitution of magnesia can take place by soda, which mineralogists frequently assume; and by water, which is certainly not more improbable; and that silicic acid (\( \text{SiO}^3 \)) may be replaced by the triatomic aluminic acid which exists in gibbsite (\( 3\text{HO} \cdot \text{Al}^2 \text{O}^3 \)), and by the monatomic aluminic acid which is found in diaspore (\( \text{HO} \cdot \text{Al}^3 \text{O}^3 \)); then it will be found that this mineral thermophyllite is essentially

\[
\text{MgO} \cdot \text{HO} + \text{MgO} \cdot \text{SiO}^3
\]

the silicate of magnesia being partly replaced, at least in this specimen, by the silicates of soda and protoxide of iron, by the monobasic aluminic acid of water, \( \text{HO} \cdot \text{Al}^2 \text{O}^3 \) (diaspore), and by the tribasic aluminic acid of iron and water, \( \text{FeO} \cdot \text{Al}^2 \text{O}^3 \), a body which, although not known, I believe, in the separate state, has its existence rendered highly probable by the occurrence of the analogous aluminic acid of potash, \( \text{K} \cdot \text{Al}^2 \text{O}^3 \), which has been obtained artificially in crystals. The simple fact of any given salt not having been found as a mineral is not a sufficient warrant for the assumption that it does not so exist, especially when it is in striking analogy with other known compounds, of the existence of which no question can be raised.

It would be interesting to examine other specimens of this mineral, to ascertain whether the typical formula

\[
\text{MgO} \cdot \text{HO} + \text{MgO} \cdot \text{SiO}^3
\]

ever exists, or whether other replacements exist which render that view of its composition admissible.
XXIX. On Equally Attracting Bodies. By Dr. T. A. Hirst.

[Concluded from p. 177.]

23. The theorem of art. 21 furnishes at once a method of discovering any number of pairs of equally attracting surfaces; for the equation (18) of art. 20 may be thus written:

\[
\frac{dv}{d\theta} = -\frac{1}{\sin \theta} \frac{dv_1}{d\phi} = f(\theta, \phi), \quad \ldots \quad (19)
\]

where \(f(\theta, \phi)\) is some function of \(\theta, \phi\) which we will suppose to be given. The general integrals of these two partial differential equations of the first order and degree will represent two systems of surfaces, such that the normal vector-planes of any surface of the one system will be perpendicular to the corresponding normal vector-planes of every surface of the second system. We may therefore select a surface from each of the two systems for our surfaces \(\rho\) and \((\rho_1)\), and thus arrive by the theorem of art. 21 at a pair of equally attracting surfaces \((r)\) and \((r_1)\).

Let us enter a little further into the details of this method. It is well known that the integration of the partial differential equations (19) depends upon that of the ordinary differential equations

\[
\begin{align*}
\frac{d\theta}{\sin \theta} + f(\theta, \phi) &= 0, \\
\frac{d\phi}{\sin \theta} - f(\theta, \phi) d\phi &= 0,
\end{align*}
\]

in such a manner, that if the integrals of the latter be represented, respectively, by

\[
F(\theta, \phi) = \text{const.}, \\
F_1(\theta, \phi) = \text{const.},
\]

the complete integrals of (19) may be written, respectively, thus,

\[
\begin{align*}
v &= 2 \log [\Psi(F)], \\
v_1 &= 2 \log [\Psi_1(F_1)],
\end{align*}
\]

where \(\Psi\) and \(\Psi_1\) are symbols of arbitrary functions. Thence by art. 21 the general equations of a pair of equally attracting surfaces will be

\[
2u = v + v_1 = 2 \log [\Psi \cdot \Psi_1],
\]

\[
2u_1 = v - v_1 = 2 \log [\Psi : \Psi_1];
\]

or, including at once the inverse surfaces \(r', r'_1\) to \(r, r_1\), the
general equations of a system of four equally attracting surfaces will be

\[
\frac{r}{c} = \frac{c'}{r'} = \Psi \left[ \frac{F(\theta, \phi)}{\sin \theta} \right] \cdot \Psi_1 \left[ \frac{F_1(\theta, \phi)}{\sin \theta} \right],
\]

(21)

\[
\frac{r_1}{c_1} = \frac{c_1'}{r_1'} = \frac{\Psi \left[ \frac{F(\theta, \phi)}{\sin \theta} \right]}{\Psi_1 \left[ \frac{F_1(\theta, \phi)}{\sin \theta} \right]}.
\]

24. As an example of the above method, let us assume \( f(\theta, \phi) \) to be a product of two functions; one \( \Theta \) of \( \theta \) alone, and another \( \Phi \) of \( \phi \) alone. Under this hypothesis the variables in (20) are at once separable, and we have

\[
F(\theta, \phi) = \int \frac{\Theta}{\sin \theta} d\theta + \int \frac{d\phi}{\Phi},
\]

\[
F_1(\theta, \phi) = \int \frac{d\theta}{\Theta \cdot \sin \theta} - \int \Phi \cdot d\phi;
\]

so that the four equally attracting surfaces of (21) are represented by the equations

\[
\frac{r}{c} = \frac{c'}{r'} = \Psi \left[ \int \frac{\Theta}{\sin \theta} d\theta + \int \frac{d\phi}{\Phi} \right] \cdot \Psi_1 \left[ \int \frac{d\theta}{\Theta \cdot \sin \theta} - \int \Phi \cdot d\phi \right],
\]

(22)

\[
\frac{r_1}{c_1} = \frac{c_1'}{r_1'} = \frac{\Psi \left[ \int \frac{\Theta}{\sin \theta} d\theta + \int \frac{d\phi}{\Phi} \right]}{\Psi_1 \left[ \int \frac{d\theta}{\Theta \cdot \sin \theta} - \int \Phi \cdot d\phi \right]}.
\]

25. To descend to particular cases of these formulæ, let, in the first place,

\[
\Theta = \Phi = 1 = f(\theta, \phi);
\]

the equations (22) will then become

\[
\frac{r}{c} = \frac{c'}{r'} = \Psi \left[ \log \left( \tan \frac{\theta}{2} \right) + \phi \right] \cdot \Psi_1 \left[ \log \left( \tan \frac{\theta}{2} \right) - \phi \right],
\]

\[
\frac{r_1}{c_1} = \frac{c_1'}{r_1'} = \frac{\Psi \left[ \log \left( \tan \frac{\theta}{2} \right) + \phi \right]}{\Psi_1 \left[ \log \left( \tan \frac{\theta}{2} \right) - \phi \right]},
\]

whose form is somewhat similar to that of the equations in art. 22. Here, however, the surfaces \((r)\) and \((r_1)\) have not in general their corresponding normal vector-planes perpendicular to each other; in fact, it may be shown that it is only when

\[
\Psi(\xi) = \Psi_1(\xi) = e^{\frac{m}{2} \xi},
\]

where \( m \) is an arbitrary constant, that \((r)\) and \((r_1)\) possess this
property, and then indeed they are identical with the two surfaces already considered in art. 16.

26. A second particular case, of more interest than the last, arises from the hypothesis

\[ \Theta = \cos \theta, \text{ and } \Phi = \tan \phi, \]

by means of which the equations (22), art. 24, become

\[ \frac{r}{c} = \frac{c'}{r} = \Psi [\log (\sin \theta \cdot \sin \phi)], \quad \Psi_1 [\log (\tan \theta \cdot \cos \phi)], \]

\[ \frac{r_1}{c_1} = \frac{c'_1}{r'_1} = \Psi_1 [\log (\sin \theta \cdot \sin \phi)] \quad \text{and} \quad \Psi_1 [\log (\tan \theta \cdot \cos \phi)]. \]

If we transform to rectangular coordinates by means of the formulæ

\[ x = r \cos \theta, \]
\[ y = r \sin \theta \cdot \cos \phi, \]
\[ z = r \sin \theta \cdot \sin \phi, \]

and replace the functional symbols \( \Psi \log, \Psi_1 \log \) by the equally available symbols \( \Psi, \Psi_1 \), the above equations assume the simpler forms,

\[ \frac{r}{c} = \frac{c'}{r} = \Psi \left( \frac{z}{r} \right) \cdot \Psi_1 \left( \frac{y}{x} \right), \]

\[ \frac{r_1}{c_1} = \frac{c'_1}{r'_1} = \Psi_1 \left( \frac{z}{r} \right) \cdot \Psi_1 \left( \frac{y}{x} \right). \]

(23)

27. Instead of examining the nature of the surfaces here represented, we will particularize still further by assuming

\[ \Psi(\xi) = \frac{1}{\xi}. \]

If, at the same time, we confine our attention to the two surfaces \((r)\) and \((r_1)\), the formulæ (23) take the forms

\[ z = c \Psi_1 \left( \frac{y}{x} \right), \]

\[ z = \frac{c_1}{\Psi_1 \left( \frac{y}{x} \right)}, \]

(24)

and the surfaces represented are both conoidal, inasmuch as they are generated by the motion of a line which always cuts the \(z\)-axis perpendicularly.

28. One of the most interesting cases of such equally attract-
Dr. Hirst on Equally Attracting Bodies.

ing conoidal surfaces corresponds to the hypothesis

$$\Psi_1 \left( \frac{y}{x} \right) = \frac{ax + by}{a_1 x + b_1 y}$$

where \(a, b, a_1, b_1\) are arbitrary constants, for then the equations (24) become

\[
\begin{align*}
\varepsilon &= c \frac{ax + by}{a_1 x + b_1 y}, \quad \ldots \ldots \ldots (\text{II}) \\
\varepsilon &= c_1 \frac{a_1 x + b_1 y}{ax + by}, \quad \ldots \ldots \ldots (\text{II}_1)
\end{align*}
\]

and represent two equilateral hyperbolic paraboloids having a common generator—the \(z\)-axis—passing through their vertices and the attracted point.

Any plane through the \(z\)-axis, \(e. g.\)

\[
\frac{ax + by}{a_1 x + b_1 y} = \frac{\alpha}{\beta^2}
\]

where \(\alpha\) and \(\beta\) are constants, touches the paraboloids (II) and (II\(_1\)) at points on the \(z\)-axis whose distances from the origin are

\[
\varepsilon = c \frac{\alpha}{\beta^2}, \quad \text{and} \quad \varepsilon_1 = c_1 \frac{\beta}{\alpha},
\]

respectively, so that the product

\[
\varepsilon \cdot \varepsilon_1 = c \cdot c_1 = \text{const. ;}
\]

that is to say, these points of contact form a system of points in involution, the origin or attracted point being the centre of the system. The two surfaces, therefore, are so situated with respect to each other, that the asymptotic plane, through the common director, of either surface touches the other in the attracted point; and they have, moreover, two generators in common which pass through the double points of the system in involution, and are situated at the distance \(\sqrt{\varepsilon \cdot \varepsilon_1}\) on each side of the attracted point. These coincident generators are, of course, real or imaginary according as \(c\) and \(c_1\) have like or unlike signs.

The result of the foregoing investigation may be thus expressed:—When any two equilateral hyperbolic paraboloids have in common a director through their vertices, and at the same time are so situated that the points of contact of planes passing through this director constitute a system of points in involution, the centre of this system will be attracted equally by corresponding portions of both surfaces, provided the densities at corresponding points are the same.

29. In order that the accuracy of this theorem may be perfectly evident, we ought to remark that (II) or (II\(_1\)) is the most general form of the equation of an equilateral hyperbolic para-
boloid, whose director through the vertex is the $z$-axis. It will also be noticed that the mutual position of the two hyperboloids is completely defined by saying that the asymptotic plane, through the common director, to each surface must touch the other in the attracted point. In fact it is well known that the pencil of tangent planes, whose axis is the common director, is homographic at once with the system of their points of contact with the surface $H$, and that formed by their points of contact with the surface $H_1$. Hence, whatever the position of the two surfaces, the points of contact of these planes constitute two homographic systems of points in the common director. But when their position is such as above defined, that is to say, when the points in each system which correspond, respectively, to the infinitely distant points in the other system coincide, it is well known that the two homographic systems of points form a system in involution whose centre is at those coincident points.

30. The theorem of art. 28, as above modified, leads easily to the solution of the following problem:—Given any two equilateral hyperbolic paraboloids $(H)$ and $(H_1)$, and a point $O$ in the director which passes through the vertex of the first $(H)$, it is required to place the second surface $(H_1)$ so that corresponding portions of both surfaces may attract $O$ equally.

The problem is solved by performing the following three operations on $(H_1)$:—First. Make one of the directors through the vertex of $(H_1)$ coincide with the director of $(H)$ in which the point $O$ is situated. Secondly. Make $(H_1)$ turn around the common director until its asymptotic plane through that director touches $(H)$ in the point $O$. Thirdly. Make $(H_1)$ slide along the common director without rotating, until the point of the same in which $(H_1)$ is touched by the asymptotic plane of $(H)$ coincides with the point $O$.

The magnitude of the rotation $\lambda$ in the second, and of the displacement $\gamma$ in the third operation, may be thus found. Let the equation of $(H_1)$ at the end of the first operation be

$$z = \frac{\alpha x + \beta y}{\alpha_1 x + \beta_1 y},$$

replacing $x, y, z$ by $x \cos \lambda + y \sin \lambda, y \cos \lambda - x \sin \theta, z - \gamma$, respectively, this equation becomes

$$z = \frac{[(\alpha + \alpha_1 \gamma) \cos \lambda - (\beta + \beta_1 \gamma) \sin \lambda] x + [(\alpha + \alpha_1 \gamma) \sin \lambda + (\beta + \beta_1 \gamma) \cos \lambda] y}{(\alpha_1 \cos \lambda - \beta_1 \sin \lambda) x + (\alpha_1 \sin \lambda + \beta_1 \cos \lambda) y},$$

and is that of $(H_1)$ after effecting the second and third operations. But in order that $(H_1)$ may, in this new position, attract in the same manner as $(H)$, the last equation must have the form of $(H_1)$ in art. 28; whence, to determine $\lambda$ and $\gamma$, we
Dr. Hirst on Equally Attracting Bodies.

have the conditions

\[
\frac{(\alpha + \alpha_1 \gamma) \cos \lambda - (\beta + \beta_1 \gamma) \sin \lambda}{(\alpha + \alpha_1 \gamma) \sin \lambda + (\beta + \beta_1 \gamma) \cos \lambda} = \frac{a_1}{b_1},
\]

\[
\frac{\alpha_1 \cos \lambda - \beta_1 \sin \lambda}{\alpha_1 \sin \lambda + \beta_1 \cos \lambda} = \frac{a}{b}
\]

from which we deduce

\[
\tan \lambda = \frac{a_1 b - \beta_1 a}{a_1 a + \beta_1 b} = \frac{(\alpha + \alpha_1 \gamma) b_1 - (\beta + \beta_1 \gamma) a_1}{(\alpha + \alpha_1 \gamma) a_1 + (\beta + \beta_1 \gamma) b_1};
\]

the magnitude of \( \lambda \), therefore, is determined. That of \( \gamma \), after a few easy reductions, is found to be

\[
-\gamma = \frac{(a b_1 - a_1 b)(a a_1 + \beta \beta_1) + (a a_1 + b_1)(\alpha \beta - \alpha_1 \beta)}{(a b_1 - a_1 b)(a^2 + \beta^2)}.
\]

31. It would not be difficult to establish the theorem of art. 28 by proving, directly, that the surfaces (H) and (H₁) cut each ray of a pencil, whose centre is the attracted point, at equal angles; we omit this demonstration, however, as well as the establishment of a similar theorem for all the conoidal surfaces of art. 27, in order to notice briefly the two surfaces inverse to the paraboloids (H) and (H₁), and which attract in the same manner. Their equations might, of course, be just as readily found as were those of (H) and (H₁); we prefer here, however, to confine ourselves to the generation of the surfaces in question, as suggested by the theory of inverse surfaces.

If in the axis of a pencil of planes we take a system of points homographic with that pencil, we know that an equilateral hyperbolic paraboloid is generated by a right line moving so as always to cut the axis perpendicularly in the point which corresponds to the plane of the pencil in which such line is situated. The inverse of this generating line, with respect to a fixed point O of inversion in the axis of the pencil, is a circle passing through O, and having its centre in the axis; further, the centres of all such circles clearly form a system of points homographic with the first system and, therefore, with the pencil of planes. So that having again taken a suitable system of points in the axis of, and homographic with the pencil of planes, the inverse of the above equilateral hyperbolic paraboloid would be generated by a circle of variable magnitude moving so as always to pass through the fixed point O, and to have its centre at that point of the axis which corresponds to the plane in which the circle is situated. The surface, generated in the same manner, which attracts O equally is so placed that its generating circles all pass through O, and have their centres in the same
line; in fact, the centres of generating circles, in the same plane, form a system in involution whose centre is O.

It is scarcely necessary to remark that there are other ways of generating the inverse of an equilateral hyperbolic paraboloid; for example, since the latter surface may be generated by a line moving along two directors so as constantly to remain perpendicular to one of them, the inverse surface will be generated by a circle moving so as always to pass through a fixed point, to have its centre in a fixed line through that point, and to have as director any other circle whatever passing through the same fixed point.

32. As a last example of equally attracting conoidal surfaces represented by the equations (24), art. 27, we may mention the case where one is a so-called skew helicoid. As is well known, the curvilinear director of this surface is a helix on the surface of a common cylinder, the rectilineal director, to which the generator is always perpendicular, being the axis of the cylinder—in our case the $z$-axis. The equation of such a surface is easily found to be

$$z = ka \tan^{-1}\left(\frac{y}{x}\right), \quad \ldots \ldots \ldots \ldots \quad (S)$$

where $a$ is the radius of the circular base of the cylinder, and $k$ the tangent of the angle at which the helix cuts the plane of that base. By art. 27, therefore, the equation of the equally attracting conoidal surface is

$$z = \frac{c_1}{\tan^{-1}\left(\frac{y}{x}\right)}. \quad \ldots \ldots \ldots \ldots \quad (S_1)$$

The director here is an equilateral hyperbola traced upon the surface of the cylinder; one of its asymptotes is the generator of the cylinder through the point where the helix cuts the base, the other is the base itself, as is at once evident from the fact that the ordinates of the surfaces (S) and (S$_1$) have reciprocal values.

33. The particular cases of the general equations of art. 23 are, of course, inexhaustible; we will limit ourselves to one more which, from its simplicity, possesses some interest. By setting

$$\Psi\left(\frac{z}{r}\right) = \sqrt{1 - \frac{z^2}{r^2}} = \frac{\sqrt{x^2 + y^2}}{r},$$

$$\Psi_1\left(\frac{y}{x}\right) = \sqrt{1 + \frac{y^2}{x^2}} = \frac{\sqrt{x^2 + y^2}}{x},$$

and selecting the formulae for $r'$ and $r_1'$, given in (23) of art. 26,
we obtain the equally attracting surfaces

\[ c' = \frac{x^2 + y^2}{x}, \ldots \ldots \quad (C) \]

\[ c'_1 = x. \ldots \ldots \ldots \ldots \quad (F) \]

Whence we learn that, the densities at corresponding points being equal, a point is attracted in the same manner by corresponding portions of an indefinite plane, of its inverse sphere (see II. art. 7), and of any cylinder circumscribing that sphere in such a manner as to have its generators parallel to the plane. To these three equally attracted surfaces may be added, too, the inverse of the cylinder—a surface easily constructed by means of the theorem III. of art. 7.

34. By the method of art. 23, of which numerous examples have been given, every hypothesis with respect to the forms of the functions \( \Psi \) and \( \Psi_1 \) leads, it is true, to a system of four equally attracting surfaces; but if we propose to find all the surfaces which attract in the same manner as any given one, this method ceases to be applicable. By the theorem of art. 3, however, and by the equation (6) of art. 11, the general integral of the partial differential equation

\[
\left( \frac{du}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{du}{d\phi} \right)^2 = \tan^2 \psi, \quad (6)
\]

where \( \tan \psi \) is to be regarded as a given function of \( \theta \) and \( \phi \), furnishes a complete solution of the more general problem in question.

35. The integration of (6) presents no difficulties beyond those of ordinary quadrature and elimination when \( \tan \psi \) is a given function \( \Theta \) of \( \theta \) alone. In fact, the condition of integrability, as well as the equation

\[
\left( \frac{du}{d\theta} \right)^2 + \frac{1}{\sin^2 \theta} \left( \frac{du}{d\phi} \right)^2 = \Theta^2, \quad \ldots \quad (25)
\]

will be satisfied in such cases when

\[
\begin{align*}
du \\
d\phi \\
d\theta
\end{align*} = \begin{align*}
\alpha = \text{const.} \\
\sqrt{\Theta^2 - \frac{\alpha^2}{\sin^2 \theta}} \end{align*} \quad \ldots \ldots \quad (26)
\]

so that a solution of (25) is

\[ u = F(\theta, \alpha) + \alpha \phi, \]

where

\[ F(\theta, \alpha) = \int d\theta \sqrt{\Theta^2 - \frac{a^2}{\sin^2 \theta}}. \]  

(27)

We may remark, once for all, that in the integration here indicated it will not be necessary to add an arbitrary constant, since the same may be regarded as already involved in \( u \) (see equation (5), art. 11).

The geometrical peculiarity of the above solution is, that any cone of revolution about the \( x \)-axis, having the origin for vertex, cuts the surface in a curve which is everywhere equally inclined to the generators of the cone, so that on unfolding the latter into a plane, the curve in question would become a logarithmic spiral having the origin for pole.

It will be readily seen, too, that the complete integral of (25) will result from the elimination of \( \alpha \) between the equations

\[
\begin{align*}
  u &= F(\theta, \alpha) + \alpha \phi + f(\alpha), \\
  \frac{du}{d\alpha} &= \frac{dF}{d\alpha} + \phi + f'(\alpha) = 0,
\end{align*}
\]

(28)

where \( f(\alpha) \) is an arbitrary function of \( \alpha \), and, as usual,

\[ f'(\alpha) = \frac{df(\alpha)}{d\alpha}. \]

In fact, if we differentiate the first of these equations under the hypothesis that \( \alpha \) is a function of \( \theta \) and \( \phi \) as determined by the second, we shall, in consequence of this second, re-obtain the values (26), which we know to satisfy the equation (25).

According to the method (Monge’s) here employed, each of the required surfaces is considered as the envelope of the space traversed by the first of the surfaces (28) when the latter changes its form and position in consequence of the variation of the parameter \( \alpha \). When \( \alpha \) is regarded as having a definite, though arbitrary value, the two equations (28) represent the so-called characteristic of the required surfaces,—a curve of double curvature which may clearly be represented equally well by many other pairs of equations, and amongst the rest by the pair

\[
\begin{align*}
  u &= F(\theta, \alpha) - \alpha \frac{dF}{d\alpha} + f(\alpha) - \alpha f'(\alpha), \\
  0 &= \frac{dF}{d\alpha} + \phi + f'(\alpha),
\end{align*}
\]

(29)

of which the former is the equation of a surface of revolution about the \( x \)-axis, and the latter that of a cone having the origin for vertex.

36. By means of these equations (29), it is easy to verify a remark already made in the first paragraph of art. 19, to the
effect that every surface of the class under consideration may be
turned in any manner around the x-axis without ceasing to be
one of the equally attracting surfaces of the group to which it
belongs. In fact, if \( c \) be an ordinary constant, the value of the
quantity \( f(\alpha) - \alpha f'(\alpha) \) in (29) will remain unaltered when \( f(\alpha) \) is
replaced by \( f(\alpha) + c\alpha \), and at the same time \( f'(\alpha) \) will be changed
into \( f'(\alpha) + c \). So that if, for any particular form of \( f(\alpha) \),
\[
u = \psi(\theta, \phi)\]
be the result of eliminating \( \alpha \) from (29),
\[
u_1 = \psi(\theta, \phi + c)\]
will be the result of such elimination when \( f(\alpha) \) is changed into
\( f(\alpha) + c\alpha \). But the equally attracting surfaces \( u \) and \( u_1 \) clearly
differ in position only, and both can be made to coincide by
turning either around the x-axis through an angle \( c \).

37. We proceed to treat briefly a few particular cases of the
equation (29), art 35. If, in the first place, \( \Theta \) be a constant
\( = \lambda \), the equation (27) becomes
\[
F(\theta, \alpha) = \int \frac{d\theta}{\sin \theta} \sqrt{\lambda^2 \sin^2 \theta - \alpha^2};
\]
whence, on integrating and subsequently differentiating accord-
ing to \( \alpha \), we find
\[
F(\theta, \alpha) = -\lambda \sin^{-1} \left( \frac{\lambda \cos \theta}{\sqrt{\lambda^2 - \alpha^2}} \right) - \alpha \tan^{-1} \left( \frac{\sqrt{\lambda^2 \sin^2 \theta - \alpha^2}}{\alpha \cos \theta} \right),
\]
\[
\frac{dF}{d\alpha} = -\tan^{-1} \left( \frac{\sqrt{\lambda^2 \sin^2 \theta - \alpha^2}}{\alpha \cos \theta} \right);
\]
so that the formulae (29) give
\[
\begin{align*}
\nu &= -\lambda \sin^{-1} \left( \frac{\lambda \cos \theta}{\sqrt{\lambda^2 - \alpha^2}} \right) + f(\alpha) - \alpha f'(\alpha), \\
0 &= -\tan^{-1} \left( \frac{\sqrt{\lambda^2 \sin^2 \theta - \alpha^2}}{\alpha \cos \theta} \right) + \phi + f'(\alpha).
\end{align*}
\]
These equations, from which \( \alpha \) is to be eliminated as soon as
\( f \) is particularized, represent the class of surfaces alluded to in
art. 8; each surface cuts every ray of a pencil, whose centre is
the origin, at a constant angle \( \beta \), whose cotangent is \( \lambda \).

According to art. 36, the hypotheses \( f(\alpha) = \Lambda + B\alpha \), and
\( f(\alpha) = \Lambda = \text{const} \) lead to essentially like results; indeed, without
loss of generality, we may even suppose this constant \( \Lambda = 0 \), so
that the second of equations (30) becomes
\[
\frac{\sqrt{\lambda^2 \sin^2 \theta - \alpha^2}}{\alpha \cos \theta} = \tan \phi,
\]
whence we deduce

\[ \lambda^2 - \alpha^2 = \frac{\lambda^2 \cos^2 \theta}{1 - \sin^2 \theta \cdot \sin^2 \phi}. \]

This value, substituted in the first of equations (30), gives

\[ -u = u' = \lambda \sin^{-1}(\sqrt{1 - \sin^2 \theta \cdot \sin^2 \phi}). \]

The nature of the surface here represented will be at once evident on introducing the angle \( \theta_1 \) between the radius vector and the \( z \)-axis; for since \( \sin \theta \cdot \sin \phi = \cos \theta_1 \), the above equation becomes simply

\[ u' = \lambda \theta_1, \quad \text{or} \quad r' = e^{\lambda \theta_1}, \]

and represents a surface of revolution around the \( z \)-axis generated by a logarithmic spiral whose pole is at the origin. The surface inverse to \( r' \) is of a similar nature, as already remarked in art. 8.

38. We omit several other cases, where \( \alpha \) may be eliminated without difficulty from the equations (30), in order to notice with equal brevity the group of surfaces alluded to in art. 19, as being the only one, of the class under consideration, which contains pairs of equally attracting surfaces whose corresponding normal vector-planes are perpendicular to each other. Making \( \alpha = m \lambda \) in equation (16) of art. 19, we have now

\[ \tan^2 \frac{\theta}{2} \]

\[ \Theta^2 = a^2 \frac{\tan^2 \frac{\theta}{2}}{\sin^2 \theta}; \]

so that, by (27),

\[ F(\theta, \alpha) = \int \frac{d\theta}{\sin \theta} \sqrt{a^2 \tan^2 \frac{\theta}{2} - \alpha^2}. \]

The integration here indicated presents no difficulty, and when effected, gives

\[ F(\theta, \alpha) = \frac{1}{m} \sqrt{a^2 \tan^2 \frac{\theta}{2} - \alpha^2} - \frac{\alpha}{m} \tan^{-1} \left( \frac{\sqrt{a^2 \tan^2 \frac{\theta}{2} - \alpha^2}}{\alpha} \right); \]

whence, by differentiation,

\[ \frac{dF}{d\alpha} = -\frac{1}{m} \tan^{-1} \left( \frac{\sqrt{a^2 \tan^2 \frac{\theta}{2} - \alpha^2}}{\alpha} \right). \]

Accordingly, by the formul\( \text{e} \) (29), the group of equally attracting surfaces under consideration is represented by the equations
\[ u = \frac{1}{m} \sqrt{a^2 \tan^2 \frac{\theta}{2} - \alpha^2 + f(\alpha) - af'(\alpha),} \]

\[ 0 = -\frac{i}{m} \tan^{-1} \left( \frac{\sqrt{a^2 \tan^2 \frac{\theta}{2} - \alpha^2}}{\alpha} \right) + \phi + f'(\alpha), \]

from which, as before, \( \alpha \) is to be eliminated as soon as a particular form for \( f \) has been chosen.

The hypothesis \( f(\alpha) = f'(\alpha) = 0 \) leads at once to the first of the pair of surfaces given at the end of art. 19; the second surface of the pair, whose corresponding normal vector-planes are perpendicular to each other, results from the hypothesis \( f(\alpha) = \alpha \cdot \frac{\pi}{2} \), and consequently, by art. 36, differs only in position from the first surface.

39. We will treat at somewhat greater length the more interesting case of the group of surfaces which attract in the same manner as a given plane perpendicular to the \( x \)-axis. In this case it is evident that \( \Theta = \tan \theta \), and therefore

\[ F(\theta, \alpha) = \int \frac{d\theta}{\sin \theta} \sqrt{\sin^2 \theta \cdot \tan^2 \theta - \alpha^2}; \]

whence, effecting the integration,

\[ F(\theta, \alpha) = \log \left[ \frac{\alpha}{\sqrt{\alpha^2 + 4}} \cdot \frac{\sqrt{\sin^4 \theta - \alpha^2 \cos^2 \theta + 1 + \cos^2 \theta}}{\cos \theta} \right] + \frac{\alpha}{2} \sin^{-1} \left[ \frac{1 + \cos^2 \theta}{\sin^2 \theta} \right], \]

and differentiating,

\[ \frac{dF}{d\alpha} = \frac{2(\alpha^2 + 2)}{\alpha(\alpha^2 + 4)} + \frac{1}{2} \sin^{-1} \left[ \frac{\alpha}{\sqrt{\alpha^2 + 4}} \cdot \frac{1 + \cos^2 \theta}{\sin^2 \theta} \right]. \]

Consequently it follows from (29), that every surface which attracts in the same manner as the given plane will be represented by an equation which results from the elimination of \( \alpha \) between the equations

\[ u = \log \left[ \frac{\alpha}{\sqrt{\alpha^2 + 4}} \cdot \frac{\sqrt{\sin^4 \theta - \alpha^2 \cos^2 \theta + 1 + \cos^2 \theta}}{\cos \theta} \right] + f(\alpha) - af'(\alpha) - \frac{2(\alpha^2 + 2)}{\alpha^2 + 4}, \]

\[ 0 = \frac{1}{2} \sin^{-1} \left[ \frac{\alpha}{\sqrt{\alpha^2 + 4}} \cdot \frac{1 + \cos^2 \theta}{\sin^2 \theta} \right] + \phi + f'(\alpha) + \frac{2(\alpha^2 + 2)}{\alpha(\alpha^2 + 4)} \]

\[ \{32\} \]

40. In order to examine the nature of the characteristic represented by these equations, we will give to \( u \), for the moment,
the value \( \log \frac{c}{r} \); so that, for these surfaces \((r)\), the equations \((32)\) may be written in the form

\[
\frac{b}{r} = \sqrt[\sin^4 \theta - \alpha^2 \cos^2 \theta + 1 + \cos^2 \theta} \cos \theta \left\{ \frac{\alpha}{\sqrt{\alpha^2 + 4}} \cdot \frac{1 + \cos^2 \theta}{\sin^2 \theta} = \sin 2(\phi - \beta), \right. \] \tag{32a}

where \(b\) and \(\beta\) are constants dependent upon \(\alpha\), itself conceived to have a constant value. By means of the latter, the former of these equations may be replaced by

\[
\frac{b}{r} \cos \theta - 1 - \cos^2 \theta = \pm \frac{\alpha}{2} \sin^2 \theta \cos 2(\phi - \beta),
\]

where for brevity we have made \(\alpha_1 = \sqrt{\alpha^2 + 4}\), so that \(\alpha_1 > 2\).

Passing to rectangular coordinates by means of the formulæ

\[\begin{align*}
& r \cos \theta = x, \quad r \sin \theta \cos (\phi - \beta) = y, \quad r \sin \theta \sin (\phi - \beta) = z,
\end{align*}\]

it will be found that the characteristic under consideration is the intersection of one of the two surfaces,

\[
4 \left( x - \frac{b}{4} \right)^2 + (\alpha_1 + 2)y^2 - (\alpha_1 - 2)z^2 = \frac{b^2}{4}, \quad \ldots \quad (\text{H})
\]

\[
4 \left( x - \frac{b}{4} \right)^2 - (\alpha_1 - 2)y^2 + (\alpha_1 + 2)z^2 = \frac{b^2}{4}, \quad \ldots \quad (\text{H}_1)
\]

with the surface

\[2ax^2 + ay^2 - 2axyz + az^2 = 0. \quad \ldots \quad (33)\]

Here \((\text{H})\) and \((\text{H}_1)\) are hyperboloids of one sheet, each of which has its principal axes parallel to those of the coordinates, a vertex at the origin, and its centre in the \(x\)-axis. The plane \((yz)\), in fact, touches \((\text{H})\) along two generators whose equations are

\[
\frac{y}{z} = \pm \frac{\sqrt{\alpha_1 - 2}}{\sqrt{\alpha_1 + 2}} = \pm \frac{\alpha_1 - 2}{\alpha},
\]

and \((\text{H}_1)\) along the two generators

\[
\frac{y}{z} = \pm \frac{\alpha_1 + 2}{\alpha}.
\]

The surface \((33)\) is clearly a cone of the second order with its vertex at the origin, and symmetrically placed with respect to the plane \((yz)\); the latter, in fact, cuts the cone in the two generators

\[
\frac{y}{z} = \frac{\alpha_1 \pm 2}{\alpha}, \quad \ldots \quad \ldots \quad \ldots \quad (\text{L})
\]
Dr. Hirst on Equally Attracting Bodies.

one of which, it will be observed, coincides with a generator of

\((H)\), whilst the other coincides with a generator of \((H_1)\).

It can be easily shown, however, that if, by squaring, we cause

the radical sign to disappear, the two equations \((32)\) will then

represent a surface composed of the inverse surfaces \((r)\) and \((r')\).

The characteristic of this more general surface will be the inter-

section of the cone \((33)\) with both hyperboloids \((H)\) and \((H_1)\);

in other words, this characteristic will consist of two right lines

\((L)\) in the plane \((yz)\), and of two curves of double curvature of

the third order, all of which pass through the origin.*

41. With respect to particular cases of the surfaces represented

by \((32)\), we remark, in the first place, that the hypothesis \(a = 0\)

leads to the plane and its inverse sphere,

\[
\frac{r}{c} = \frac{e^l}{r'} = \frac{1}{\cos \theta^2}
\]

as may be best seen by treating, directly, the original differential

equation \((25)\).

42. As a second hypothesis, let

\[
2f''(\alpha) + \frac{4(\alpha^2 + 2)}{\alpha(\alpha^2 + 4)} = \sin^{-1}\left(\frac{a \alpha}{\sqrt{\alpha^2 + 4}}\right) = \cos^{-1}\left(\frac{\sqrt{b^2 \alpha^2 + 4}}{\sqrt{\alpha^2 + 4}}\right),
\]

where \(a\) and \(b\) are any constants less than unity, and fulfilling

the relation

\[
a^2 + b^2 = 1.
\]

In accordance with this hypothesis, we easily find for \(f(\alpha)\) the value

\[
f(\alpha) = \frac{a}{2} \sin^{-1}\left(\frac{a \alpha}{\sqrt{\alpha^2 + 4}}\right) - \log \left[\alpha \left(\sqrt{b^2 \alpha^2 + 4} - 2a\right)\right].
\]

By substituting these values of \(f(\alpha)\) and \(f''(\alpha)\), reducing, and

neglecting constants which do not affect the generality of the solution, the equations \((32)\) become

\[
\begin{aligned}
0 &= \log \left[\frac{\sqrt{\sin^2 \theta - \alpha^2 \cos^2 \theta + 1 + \cos^2 \theta}}{\cos \theta (\sqrt{b^2 \alpha^2 + 4} - 2a)}\right], \\
0 &= a \left(1 + \frac{\cos^2 \theta}{\sin^2 \theta} + a \alpha \cos 2\phi + \sqrt{b^2 \alpha^2 + 4} \sin 2\phi\right)
\end{aligned}
\]

from which \(\alpha\) may be eliminated without difficulty. In fact,

* A valuable memoir, by M. Chasles, on these curves of double curva-

ture of the third order will be found in Liouville’s Journal \(2^{nde}\) sér. vol. ii.).

We may remark, too, that in our case both these curves are situated upon

a surface of revolution of the third order, the generating curve of which

has the equation

\[
(x^2 + y^2)(\alpha^2 x - 2c) = ex(2x - c).
\]
from the last equation we deduce, immediately,

\[ \frac{a}{\sin^2 \theta \cdot \sin 2\phi} = \frac{\pm \sin^2 \theta \cdot \sin 2\phi}{\sqrt{U}} \]  

where, for brevity, we have made

\[ U = (1 + \cos^2 \theta + a \sin^2 \theta \cos 2\phi)^2 - b^2 \sin^4 \theta \sin^2 2\phi. \]

From (35) we deduce, too,

\[ \frac{dU}{\sin^2 \theta - a^2 \cos^2 \theta} = \frac{\sin^2 \theta}{\sqrt{U}} \left[ (1 + \cos^2 \theta) \cos 2\phi + a \sin^2 \theta \right], \]

where the upper and lower sign must correspond, respectively, with those in (35), in order that the result of differentiating \( \frac{dU}{d\phi} = a \) according to \( \theta \), may be identical with that obtained by differentiating

\[ \frac{du}{d\theta} = \sqrt{\frac{\tan^2 \theta - a^2 \cos^2 \theta}{\sin^2 \theta \cdot \cos \theta}} \frac{1}{\sqrt{\sin^4 \theta - a^2 \cos^2 \theta}} \]

according to \( \phi \). Further, by (35),

\[ \frac{\sqrt{b^2 a^2 + 4}}{2} = \frac{1 + \cos^2 \theta + a \sin^2 \theta \cos 2\phi}{\sqrt{U}}, \]

where, again, the upper or lower sign must be employed according as the upper or lower signs in (35) and (36) are adopted, otherwise the second equation in (34) would not be identically satisfied. Substituting these values (36) and (37) in the first of the equations (34), we find

\[ r = \frac{c}{r} = \frac{\sin^2 \theta [(1 + \cos^2 \theta) \cos 2\phi + a \sin^2 \theta] \pm (1 + \cos^2 \theta) \sqrt{U}}{\cos \theta [1 + \cos^2 \theta + a \sin^2 \theta \cos 2\phi + a \sqrt{U}]}, \]

which, by an easy but somewhat tedious calculation, is reducible to either of the following forms:

\[ \frac{r}{c} = \frac{c'}{r'} = \frac{a(1 + \cos^2 \theta) + \sin^2 \theta \cos 2\phi \pm \sqrt{U}}{\cos \theta} \]

\[ = \frac{-4b^2 \cos \theta}{a(1 + \cos^2 \theta) + \sin^2 \theta \cos 2\phi \pm \sqrt{U}}, \]

where, as usual, the upper and lower signs respectively correspond.

43. In order clearly to distinguish between the four surfaces represented in (38), let the pair of inverse surfaces \((r)\) and \((r')\) correspond to the upper signs, and \((r_1)\) and \((r'_1)\), also inverse to each other, to the lower signs. This being the case, the identical expressions in (38) show, at once, that \((r)\) and \((-r_1), (r')\)
and \((-r')\) are also two pairs of inverse surfaces, or, as we may express it, \((r)\) and \((r_1)\), \((r')\) and \((r'_1)\) constitute two pairs of surfaces negatively inverse to each other, so that the surfaces of each pair attract equally, but in opposite directions. If, then, by squaring we cause the double signs in (38) to disappear, we shall obtain the equation of a surface composed of \((r)\) and \((r_1)\)—or of \((r')\) and \((r'_1)\)—which by its attraction will keep the pole in equilibrium, and one-half of which will attract in the same manner as the given plane, whilst the other will attract equally but in an opposite direction. In this case, too, the two compound inverse surfaces \((r, r')\) and \((r_1, r'_1)\) will be of the same kind, and differ only in position; in other words, both may be represented by the single equation

\[
[ar'(1 + \cos^2 \theta) + r' \sin^2 \theta \cos 2\phi - c' \cos \theta]^2 = r'^2U,
\]

which by simplifying, dividing throughout by \(-\frac{1}{4} \cos \theta\), setting \(c\) in place of \(\frac{1}{2}c\), and omitting the accent of \(r\), becomes

\[
b^2\cos \theta + cr\left[a(1 + \cos^2 \theta) + \sin^2 \theta \cos 2\phi\right] - c^2 \cos \theta = 0. \quad (39)
\]

In rectangular coordinates the surface here represented has the equation

\[
\frac{b^2y^2}{[c(1-a)-b^2x]} - \frac{b^2z^2}{[c(1+a)+b^2x]} = 1. \quad (41)
\]

44. This surface of the third degree, which we have thus found to attract the origin in the same manner as any plane perpendicular to the \(x\)-axis, is altogether contained between the two planes

\[
x = \frac{c}{b^2} (1-a),
\]

\[
x = -\frac{c}{b^2} (1+a);
\]

the former of which cuts the \(x\)-axis, say on the right of the origin \(O\) in a point \(C\), and touches the surface along a line \(CZ_0\) parallel to the \(z\)-axis; whilst the latter cuts the \(x\)-axis in a point \(C_1\) on the left of \(O\), and touches the surface along a line \(C_1Y_1\) parallel to the \(y\)-axis. Every plane parallel to, and intermediate between the foregoing cuts the surface in a hyperbola whose centre is in the \(x\)-axis, and whose vertices are in the plane \((xy)\) or in the plane \((xz)\), according as the plane of the section is on the right or left of the origin. In the plane \((yz)\) itself, this hyperbola degenerates into two lines \(OA, OB\), equally inclined to
the coordinate axes, and having the equations

\[ \frac{y}{z} = \pm \frac{\sqrt{1-a}}{\sqrt{1+a}}. \]

When \( z = 0 \), the equation (40) resolves itself into

\[ [c(1+a) + b^2x] [b^2(x^2 + y^2) - c(1-a)x] = 0; \]

and when \( y = 0 \), into

\[ [c(1-a) + b^2x] [b^2(x^2 + z^2) + c(1+a)x] = 0; \]

that is to say, the plane \((xy)\) cuts the surface in the line \( C_1 Y_0 \), and in a circle \((C)\), described upon \( OC \) as diameter, whilst the plane \((xz)\) cuts the surface in the line \( CZ_0 \), and in a circle \((C_1)\), described upon \( OC_1 \) as diameter.

Again, planes through the \( x\)-axis and the lines \( OA, OB \) cut the surface in two curves \((0), (0_1)^{(0)}\), whose common polar equation is found, from (39), by setting \( \tan^2 \phi = \frac{1-a}{1+a} \), i.e. \( \cos 2\phi = a \), to be

\[ b^2r^2 \cos \theta + 2acr - c^2 \cos \theta = 0; \]

which equation, expressed in rectangular coordinates \( x, y \) in the plane of either curve, becomes

\[ (x^2 + y^2)(b^2x + 2ac) = c^2x. \]

Each section \((O), (O_1)\), therefore, is a curve of the third degree passing through the circular points at infinity and having two coincident foci in the origin \( O \). Each curve, too, consists of an oval and a serpentine branch, each of which is symmetrically placed with respect to the \( x\)-axis, and the negative inverse of the other with respect to a circle, around the origin as centre, with the radius \( \frac{c}{b} \). The oval, moreover, is on the right of the plane \((yz)\), lies altogether within this circle \( \left( \frac{c}{b} \right) \), and passes through the points \( O, C \); whilst the serpentine branch is altogether on the left of the plane \((yz)\), and without the circle \( \left( \frac{c}{b} \right) \); it passes through the point \( C_1 \), and has for asymptote the line

\[ b^2x + 2ac = 0. \]

The surface under consideration, therefore, may be conceived to be generated by a hyperbola, of variable form, moving along the circular cubics \((O), (O_1)\), as directors, in such a manner that its plane remains constantly parallel to the plane \((yz)\), and its vertices constantly upon one of the circles \((C), (C_1)\).

435. The equation (41) represents as many surfaces, attracting in the same manner as the given plane, as there are different values of \( a \) and \( b \) compatible with the condition \( a^2 + b^2 = 1 \) (art.
When \( a = 0, \ b = 1 \), this equation (41) takes the simpler form
\[
\frac{y^2}{(c-x)x} - \frac{z^2}{(c+x)x} = 1;
\]
and, with reference to the generation of this simpler surface, the circular cubics \( (O) \), \( (O_1) \) become equal circles around the origin as centre with radii equal to \( c \), their planes become perpendicular to each other, and bisect the angles between the planes of the circles \( (C) \) and \( (C_1) \), which latter have now radii equal to each other and to \( \frac{c}{2} \).

46. When \( b = 0 \) and \( a = 1 \), the equation (40) assumes the still simpler form
\[
x^2 + y^2 - \frac{c}{2} x = 0,
\]
and represents the same right cylinder already obtained, by a quite different method, in art. 33.

47. When \( \tan \psi \) has the form \( \frac{f(\phi)}{\sin \theta} \), where \( f(\phi) \) is any given function of \( \phi \) alone, the fundamental equation (6) of Art. 34 may be treated in a manner precisely similar to that followed in the 35th and following articles. Instead, however, of extending the present memoir to greater length by entering into further details, we will only remark, in conclusion, that it will be sometimes found more convenient to treat this equation (6) in the ordinary manner, by setting \( p = \frac{du}{d\theta} \) and \( q = \frac{du}{d\phi} \), by means of which it becomes
\[
p^2 + \frac{q^2}{\sin^2 \theta} = \tan^2 \psi = t^2, \ldots \ldots \ldots \ (42)
\]
where \( t \) is a given function of \( \theta \) and \( \phi \).

This being an identity, we may differentiate, separately, according to \( \theta \) and \( \phi \), and thus obtain the equations
\[
p \frac{dp}{d\theta} + \frac{q}{\sin^2 \theta} \frac{dq}{d\theta} - \frac{\cos \theta}{\sin^3 \theta} q^2 = t \frac{dt}{d\theta},
\]
\[
p \frac{dp}{d\phi} + \frac{q}{\sin^2 \theta} \frac{dq}{d\phi} = t \frac{dt}{d\phi}.
\]
By means of the general relation
\[
\frac{dp}{d\phi} = \frac{dq}{d\theta},
\]
and the original equation (42), the two last assume the forms
\[ p \frac{dp}{d\theta} + \sqrt{t^2 - p^2} \cdot \frac{dp}{d\phi} = t \frac{dt}{d\theta} + (t^2 - p^2) \tan \theta, \]

\[ \frac{\sqrt{t^2 \sin^2 \theta - q^2}}{\sin \theta} \cdot \frac{dq}{d\theta} + \frac{q}{\sin^2 \theta} \frac{dq}{d\phi} = t \frac{dt}{d\phi}. \]

For any given form of \( t \), therefore, the problem reduces itself to the integration of either of these linear, partial, differential equations, for, of \( p \) and \( q \), one being known the other is immediately deduced, and

\[ u = \int (p \, d\theta + q \, d\phi). \]

Here, however, \( p \) and \( q \) involve arbitrary functions of \( \theta \) and \( \phi \), which must in general be particularized before the final integration can be effected.

Paris, July 1858.

XXX. On the Gaseous Emanations which accompany the Boracic Acid in the Lagoons of Tuscany. By C. SAINTE-CLaire DEVILLE and F. LE BLANC.—First Memoir*.

Amongst the geological phenomena which give rise to evolutions of gases and vapours thrown into the atmosphere at a high temperature, there are scarcely any which merit in a higher degree to fix our attention than those emanations which, in Tuscany, annually bring to the surface of the soil enormous quantities of boracic acid.

These phenomena have already been described by several naturalists and geologists, especially by Targioni-Tozzetti, Coquand, &c.; and in a chemical point of view they have been the object of investigation on the part of M. Payen, and latterly of M. Schmidt, Professor at Dorpat.

In the locality, as is well known, the name of saffioni is given to impetuous currents of gas and aqueous vapour at a high temperature, forcing a passage for themselves into the atmosphere after having overcome the resistance of the soil, which finally loses its cohesion.

When the evolution takes place in the interior of cavities filled with water, the name of lagoni is given to these accidental basins, which become the seat of a constant ebullition. The artificial basins, arranged in the form of cascades and capable of communication amongst themselves, receive the waters coming from a higher level; the latter become charged with boracic acid, and at the end of twenty-four or thirty-six hours they are conducted into evaporating caldrons and replaced by fresh supplies of cold water.

* Translated from the Comptes Rendus, August 23, 1858, p. 317.
In one single locality, boracic acid occurs in solution in a true small lake, which is actually called Lago.

M. de Larderel, the proprietor of the boracic acid works, has recently caused to be made artesian borings which have furnished him with boiling waters, accompanied by the same gaseous emanations and holding in solution boracic acid to an amount at least equal to that of the waters of the lagoons. They rise to a considerable height (about 20 metres) at the moment when the borier meets with them, and cause an eruption of muddy matters and stones which recall the phænomena of the Geysers of Iceland.

The tertiary soil, in the midst of which these emanations break forth, consists of a compact limestone with spathic veins called alberese, converted into gypsum in many localities, of micaceous grit called macigno, and schistose clays.

The triangular space comprised within these various emanations is included between the culminating points of Monte Catini, Monte Calvi, and the mountain Gerfalco. The soil is traversed at several points by masses of serpentine.

In the course of the autumn of 1857, being encouraged by the kind offers addressed to us by the Count de Larderel, whose recent loss we have to deplore, we made our preparations for a journey into Tuscany. We started provided with all the apparatus necessary either to collect the gases, or to execute upon the spot a series of experiments and summary analyses.

In a letter to M. Elie de Beaumont, dated Pomarance, November 1857*, we have already announced some results of the first experiments made upon the spot. In particular we indicated the existence, in the gases, of a combustible hydrocarbon retted mixture free from oxide of carbon, and also the absence of oxygen; this last fact was in accordance with the observations of Professor Schmidt.

In the memoir which we have now the honour to present to the Academy, and which is the fruit of the labours of several months, we treat only of our investigation of the gaseous products, reserving for a second communication the results of experiments made upon the condensed liquid products, upon the different products arising from the alteration of the rocks in contact with the emanations, and upon the mother-liquors of the lagoons†.

† Although this first memoir relates particularly to the analyses of the gases, there is a question regarding the production of boracic acid which we desired to elucidate at the outset, and of which it appears to us indispensable to furnish the solution.

The presence of boracic acid in the emanations of gases and vapours has been doubted of late years; it has been necessary therefore to have recourse to complicated explanations to account for the presence of boracic acid in
It will only be after the complete exposition of our investigations that we shall be able to devote ourselves to the discussion of the hypotheses suggested by the totality of the facts, and that we shall show the relations of these singular strata with the principal geological conditions of the country.

To collect the gases, we have in general employed the same means which have been described in our previous memoir upon the volcanic emanations of Southern Italy*. Nevertheless the circumstance of the evolution of gases under pressure at the surface of the soil, and the facility with which the points of emanation may be surrounded, have permitted us to fill tubes open at both ends, which we then sealed with the blowpipe, after having cleared them of air by the gaseous current itself; the latter had been previously freed from aqueous vapour by its passage through a Woolf's apparatus. The greater part of the water was deposited by simple condensation in a bottle; the last traces of aqueous vapour were retained by the passage of the gas through concentrated sulphuric acid. The violence of the current effected a rapid displacement of the air in the vessels.

We shall now pass in review the various localities in which we have experimented, and present a summary of the results of our analytical investigations.

Larderello, or Monte Cerboli.

This is the first and the most considerable of the establishments in which boracic acid is manufactured. The numerous waters of basins which have been for a certain time the recipients of the currents of vapour,—waters which, when evaporated afterwards by means of the heat communicated by the vapours of the soil, furnish boracic acid.

It will be understood that, on condensing the aqueous vapour carried up by the soffioni, the small quantity of boracic acid cannot be detected in it by evaporating the liquid; for this acid will be carried along by the aqueous vapour in virtue of the same mechanism which brings it to the surface. On the other hand, by receiving the vapours of the soffioni in cold water, the latter becomes charged with a certain quantity of boracic acid in consequence of their condensation; this quantity goes on increasing up to a certain limit; but it will be evident that, after a certain moment, there will be a loss of boracic acid by its being carried away by the vapours, a loss which prevents the water from becoming saturated with acid, even by continuing the passage of the vapours into the water of the basins much longer than is usually done. M. Schmidt had already announced that the water of condensation of the soffioni contained boracic acid; this fact appears to us to be placed beyond a doubt by our experiments. We have condensed the vapours in great quantity, not in water, but in a solution of potash; the latter was then carefully saturated with pure sulphuric acid, evaporated to dryness, and treated with alcohol to separate the sulphates from the slightly acid residue. The alcoholic solution, when evaporated, furnished a residue exhibiting all the characters of boracic acid.

soffioni which are made use of there commence at less than 1 kilometre above the old village of Monte Cerboli, and follow nearly the direction of the valley upon the right of the torrent of the Possera. Monte Cerboli is built upon the abrupt point of a mass of serpentine, which extends, towards the north-west, to the foot of the hill of San Michele. In its vicinity the calcareous strata of the alberese are converted into gypsum; and the traces of these ancient metamorphic influences may be followed up to the present emanations, which are their last representatives and continue their work.

Our investigations upon the gases of the emanations of Larderello were more complete than those made anywhere else. There we examined separately the gases evolved from the highest and lowest of the soffioni which are made use of,—the emanations which break forth in the bed of the Possera itself, those which arise from the artesian borings, and lastly those which, in the upper part of the valley, are distinguished by a strong odour of petroleum.

The following are the numbers furnished by analysis*:

<table>
<thead>
<tr>
<th>Lowest soffione</th>
<th>Highest soffione</th>
<th>Mixture of the three un-absorbable residues</th>
</tr>
</thead>
<tbody>
<tr>
<td>Temperature</td>
<td>Temperature</td>
<td></td>
</tr>
<tr>
<td>F.</td>
<td>F.</td>
<td></td>
</tr>
<tr>
<td>Hydrogen sulphuric acid .... 4·1</td>
<td>3·7 85·1</td>
<td>Nitrogen ...... 43·35</td>
</tr>
<tr>
<td>Carbonic acid ...... 91·6</td>
<td>90·7</td>
<td>Hydrogen ...... 28·56</td>
</tr>
<tr>
<td>Oxygen .......... 0·0</td>
<td>0·0 2·7</td>
<td>Protocarbonated hydrogen ...... 28·09</td>
</tr>
<tr>
<td>Nitrogen + combustible gas 4·3</td>
<td>5·6 12·2</td>
<td>H : C² H² : 1 : 0·98</td>
</tr>
</tbody>
</table>

In order to determine accurately the composition of the combustible part, which only exists in a very small proportion (2 to 3 per cent.) in the material of the emanations, we collected the gas after having successively passed it through a solution of potash and through concentrated sulphuric acid. Two analyses of the unabsorbed residue gave,—

<table>
<thead>
<tr>
<th>I.</th>
<th>II.</th>
<th>Average.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Nitrogen .......... 43·68</td>
<td>44·64</td>
<td>44·16</td>
</tr>
<tr>
<td>Hydrogen .......... 25·52</td>
<td>25·40</td>
<td>25·46</td>
</tr>
<tr>
<td>Protocarbonated hydrogen . 30·80</td>
<td>29·96</td>
<td>30·38</td>
</tr>
</tbody>
</table>

These analyses are very concordant, and show that, in the soffioni turned to account at Larderello, the relation of the hydrogen to the protocarbonated hydrogen differs but little from unity.

The proportion of hydrogen seems to be rather less in the emanations of the ravine of the Possera. The following are the

* The small quantities of oxygen presented by certain analyses must be attributed either to the imperfection of the vacuum in the tubes, or, more rarely, to an accidental introduction of air at the moment of collecting the gas.
analyses of the contents of three different tubes. The temperature of the gas varied between 207° and 210° F.:—

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>Mixture of the unabsorbable residues of the tubes I. and II.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hydrosulphuric acid</td>
<td>1·62</td>
<td>2·13</td>
<td>Nitrogen ........ 56·75</td>
</tr>
<tr>
<td>Carbonic acid</td>
<td>92·68</td>
<td>87·67</td>
<td>Hydrogen ........ 18·81</td>
</tr>
<tr>
<td>Oxygen</td>
<td>0·21</td>
<td>1·05</td>
<td>Protocarbonated hydrogen .........................................</td>
</tr>
<tr>
<td>Nitrogen + combustible gas</td>
<td>5·49</td>
<td>9·15</td>
<td>24·44</td>
</tr>
</tbody>
</table>

Fearing that any small quantity of oxygen and aqueous vapour which might have escaped the sulphuric acid would destroy a portion of the hydrosulphuric acid in our tubes during their transport, we determined this acid by precipitation on the spot, as well as the carbonic acid. For this purpose we passed the gas of the soffioni into an acid solution of acetate of lead, and then successively into two bottles containing baryta water, and weighed, in the one case the sulphuret converted into sulphate of lead, and in the other the carbonate of baryta. This experiment, twice repeated upon the lower soffione, furnished the following volumetric proportions:

- Hydrosulphuric acid . . 6·4 5·1
- Carbonic acid . . 93·6 94·9

The preceding analyses led to the following proportions:

- Hydrosulphuric acid . . 4·3 3·9 2·0
- Carbonic acid . . 95·7 96·1 98·0

The comparison of these numbers shows, in the first place, that the determination by precipitation furnishes for hydrosulphuric acid a higher number than the analysis of the gas collected and preserved in the tubes: this might have been expected. We may also conclude therefrom that the relative quantities of carbonic acid appear to increase in proportion as we ascend to a distance from the centre of the soffioni.

It was interesting to learn whether the gas brought up by the artesian borings presented the same composition. The following are the numbers which we have obtained—temperature of the gas 207° to 209° F.:—

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>Mixture of the two unabsorbable residues.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hydrosulphuric acid</td>
<td>1·95</td>
<td>1·6</td>
<td>Nitrogen ........ 64·75</td>
</tr>
<tr>
<td>Carbonic acid</td>
<td>92·80</td>
<td>83·7</td>
<td>Hydrogen ........ 18·24</td>
</tr>
<tr>
<td>Oxygen</td>
<td>0·66</td>
<td>2·2</td>
<td>Protocarbonated hydrogen ........................</td>
</tr>
<tr>
<td>Nitrogen + combustible gas</td>
<td>4·59</td>
<td>12·5</td>
<td>17·01</td>
</tr>
</tbody>
</table>

Lastly, in the upper part of the valley of the Possera, beyond the limits of the soffioni made use of, two localities of emanation occur which are very singular, inasmuch as they
both evolve a gas which possesses a strong odour of petroleum. One of them, the highest by some metres, and the nearest to the centre of the soffioni, has a temperature of 179°-6 F., and furnishes unmistakable indications of hydrosulphuric acid with acetate of lead; the other, which is cold, scarcely colours acetate of lead paper: both contain much carbonic acid and an inflammable gas.

To complete what relates to the fissure of Monte Cerboli, we must add that at the upper limit of the soffioni, in the bed of the Possera, there are the mineral waters of the Bagni a Morbo, acidulous springs, with a temperature of 118°-4 F., one of which, that of La Peria, evolves at once hydrosulphuric and carbonic acids. The presence of these mineral waters and of the bituminous exhalations indicates very distinctly the decrease of intensity in the eruptive phenomena, in proportion both as we depart from the centre of the soffioni and from the serpentine mass of Monte Cerboli. These peculiarities present a striking resemblance to the phenomena which were exhibited to us by the volcanic masses of Southern Italy.

From the impossibility of giving here the same details upon the different centres of emanations, we shall confine ourselves to citing the localities, and giving a tabular summary of the results of the analyses of the gases.

Castelnuovo is separated from Larderello by a small ridge; the fissure which unites its soffioni is directed towards the valley of the Pavone, and is nearly the continuation of that of Monte Cerboli.

Lago.—This centre of emanations is more complex than the others. The principal fissure is met towards the top by another which intersects it at a considerable angle; and towards the bottom it spreads out into a lake from which numerous soffioni escape.

Monte Rotondo and Sasso.—These two centres of emanations are united by a single fissure, which principally traverses the micaceous grits or macigno, and has modified them in a thousand ways.

At Serrazano and Lustignano, the strata traversed and modified consist principally of alberese. In these two localities, however, we confined ourselves to some short experiments, which gave us exactly the same results as for the other lagoons.

* The following is the analysis of the gas collected at the hot emanations:

<table>
<thead>
<tr>
<th>Substance</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hydrosulphuric acid</td>
<td>traces</td>
</tr>
<tr>
<td>Carbonic acid</td>
<td>81.1</td>
</tr>
<tr>
<td>Oxygen</td>
<td>2.9</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>13.3</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>1.3</td>
</tr>
<tr>
<td>Protocarbonated hydrogen</td>
<td>1.4</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Temperatures</td>
<td>201°2 F.</td>
<td>201°2 F.</td>
<td>202°1 F.</td>
<td>202°1 F.</td>
<td>192°2 F.</td>
<td>212° F.</td>
<td>204°8 F.</td>
<td>203°9 F.</td>
<td>203°9 F.</td>
</tr>
<tr>
<td></td>
<td>L.</td>
<td>II.</td>
<td>I.</td>
<td>II.</td>
<td>I.</td>
<td>II.</td>
<td>I.</td>
<td>II.</td>
<td>I.</td>
</tr>
<tr>
<td>Hydrosulphuric acid</td>
<td>6.9</td>
<td>4.6</td>
<td>3.8</td>
<td>3.6</td>
<td>3.6</td>
<td>3.3</td>
<td>1.1</td>
<td>2.9</td>
<td>2.2</td>
</tr>
<tr>
<td>Carbonic acid</td>
<td>88.2</td>
<td>90.3</td>
<td>89.8</td>
<td>88.8</td>
<td>88.1</td>
<td>84.2</td>
<td>88.6</td>
<td>88.0</td>
<td>90.5</td>
</tr>
<tr>
<td>Oxygen</td>
<td>0.6</td>
<td>0.6</td>
<td>0.0</td>
<td>0.2</td>
<td>0.3</td>
<td>1.1</td>
<td>0.3</td>
<td>0.7</td>
<td>0.4</td>
</tr>
<tr>
<td>Nitrogen</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hydrogen</td>
<td>4.3</td>
<td>4.5</td>
<td>6.4</td>
<td>7.4</td>
<td>4.3</td>
<td>3.0</td>
<td>4.6</td>
<td>7.4</td>
<td>6.9</td>
</tr>
<tr>
<td>Protocarbonated hydrogen</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unabsorbable gas</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Nitrogen</td>
<td>37.4</td>
<td>Nitrogen</td>
<td>14.6</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hydrogen</td>
<td>25.1</td>
<td>Hydrogen</td>
<td>68.1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Protocarbonated hydrogen</td>
<td>37.5</td>
<td>Protocarbonated hydrogen</td>
<td>17.3</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Proportion of H : C2 H4...</td>
<td>1:1.5</td>
<td>1:0.27</td>
<td>1:0.45</td>
<td>1:0.87</td>
<td>1:0.81</td>
<td>1:0.81</td>
<td></td>
<td></td>
<td>1:0.83</td>
</tr>
</tbody>
</table>
The fissure which unites the two centres of emanations of Monte Rotondo and Sasso is extremely interesting to study in regard to the modifications which these emanations undergo from the bottom of the valley up to the summit of the ridge. A little above Monte Rotondo vapours are met with issuing from the soil at 209°-3 F.; although they are not put to any use, and under the influence of the air they become acidified and aluminize the schists, they have a composition absolutely similar to those of the lower soffioni, and are, like the latter, free from oxygen.

In proportion as we ascend, following the fissure, the action of the air which penetrates into the interstices of the rock oxidizes the sulphuretted gases, and produces true solfataras with their accompaniments of alum, sulphate of lime, sulphur, &c. These phenomena go on constantly diminishing in intensity up to the little ridge, where we still meet with emanations at a temperature of 194° F., consisting principally of aqueous vapour, but carrying with them also traces of hydrosulphuric acid, and small quantities of carbonic acid, and even depositing traces of sulphur. Lastly, some hot acidulous or aluminiferous springs occur on the slope of Sasso, as on that which faces Monte Rotondo.

From the totality of these phenomena it is impossible not to recognize relations of analogy between the causes which produce boric acid in Tuscany, and those which give rise to the transportation and deposition of silica in the solfataras, and especially in the geysers of Iceland.

To recapitulate, this first portion of our memoir leads us to the following conclusions:—

1. The temperature of the gases, both in the soffioni and in the artesian wells, sometimes attains, but never exceeds, 212° F. at the surface, although the rapidity of the evolution seems to indicate an internal pressure.

2. All the emanations contain the same gases, the proportions of which even are but slightly variable. As had been ascertained by M. Payen, carbonic acid is the predominant gas. The proportion of hydrosulphuric acid to carbonic acid was, at the maximum, 6:4 to 93:6. Oxygen appears to be entirely wanting. Nitrogen, on the contrary, always exists in them in the proportion of 2 or 3 per cent.

3. Lastly, a fact which has never yet been pointed out is the following,—all the gases without exception contain a mixture of free hydrogen and protocarbonated hydrogen (C²H⁴), of which the sum attains on the average that of the nitrogen. Upon the fissure which joins Larderello and Castelnuovo, the proportion of the two combustible gases is nearly unity, the carbonated
hydrogen nevertheless having a tendency to predominate; the reverse is the case with the other centres of emanations, especially at Lago, where the proportion of hydrogen to the carbonated gas attains 1:0·27.

The presence of hydrogen in these emanations presents a new feature of resemblance to the celebrated geysers and solfataras of Iceland, and confirms the opinion previously expressed by one of us. But the simultaneous existence of carbonated hydrogen seems to differentiate the soffioni from the emanations of Iceland, where, as is well known, M. Bunsen has never signalized the latter gas.

XXXI. On some Crystallized Furnace Products. By W. H. Miller, M.A., F.R.S., Professor of Mineralogy in the University of Cambridge.*

Oxide of Zinc.

SOME crystals of oxide of zinc, for which I am indebted to Dr. Percy, exhibit forms which have not been hitherto correctly determined. The crystals belonged to the rhombohedral system. I observed the forms 1 1 1, 1 1 0, 4 2 3, 2 0 1, 3 1 1, and perhaps 4 2 1, the last symbol being rather doubtful on account of the unevenness of the faces. One crystal appeared to be either a combination of the forms 1 1 0, 0 1 1, 4 1 1, or else a twin, the individuals of which are combinations of the forms 1 1 0, 0 1 1, having 1 1 1 for the twin face. This is probably the combination observed by Descloizeaux (Ann. des Mines, sér. 4. i. 482). I was unable to discover a trace of the form given upon the authority of Koch, and denoted by the symbol a : a : ∞ a : 2 c, in Rammelsberg's most useful Handbuch der krystallographischen Chemie, p. 28.

The axis of the zone 1 1 1, 4 2 3, 2 0 1, 3 1 1, 4 2 1 makes an angle of 80° with the axis of the zone 0 1 1, 1 1 1, 4 1 1.

A mean of twenty-four observed values of 2 0 1, 0 2 1 gave 1 1 1, 2 0 1 = 42° 56'. The faces of the crystals were bright, but extremely uneven. The measurements are therefore not capable of competing in point of accuracy with those of G. Rose (Das krystallochemische Mineralsystem, p. 64) and Schabus (Wiener akad. Berichte, xi. p. 8). According to Rose, the value of 3 1 1, 3 1 1 is between 52° 17' and 52° 20'. Schabus found 3 1 1, 3 1 1 = 52° 17½'. The angles computed from a mean of the results obtained by Rose and Schabus are—

* Communicated by the Author.
Prof. Miller on some Crystallized Furnace Products. 293

111, 423 17 7
111, 201 43 1
111, 311 61 49
111, 421 70 20
111, 110 90 0
111, 011 38 56
111, 411 38 56
423, 432 16 56
201, 210 39 53
311, 311 52 18
421, 412 56 11
110, 101 60 0
110, 101 57 1
011, 411 77 52
411, 110 57 1
411, 110 36 38
011, 110 65 58

Oxide of Chrome.

Oxide of chrome obtained in the laboratory by Wöhler and Ebelmjen, and found by Blake as a furnace product, occurs in crystals belonging to the rhombohedral system, isomorphous with haematite, and exhibiting the forms $111$, $110$, $100$, $011$, $311$ (Gurtt, *Übersicht der pyrogenenartigen künstlichen Mineralien*, p. 48). Some small crystals formed during a metallurgical process carried on in the United States, were lately given to me for examination by Dr. Percy. One of these, about 1 millim. long, 0·5 millim. wide, and 0·1 millim. thick, is a combination of the forms $111$, $110$, $100$, $311$. A crystal of about half the size of the preceding is a combination of the forms $111$, $110$, $100$, $122$, $311$, $201$. A third crystal, about 0·33 millim. long, 0·2 millim. wide, and 0·06 millim. thick, is a combination of the forms $111$, $110$, $100$, $1134$, $1043$. In the first crystal the observed value of $100$, $010$ was $94^\circ 10^\prime 3$. In the second the observed value of $100$, $010$ was $94^\circ 4^\prime 9$, and that of $122$, $100$ was $115^\circ 28^\prime 4$. The celebrated Brooke collection, for which the Mineralogical Museum of this University is indebted to the liberality of Charles Brooke, Esq., contains a crystal obtained by Wöhler's method, which, as usual in crystals thus obtained, is a twin having a face of the form $100$ for the twin face. The individuals of which it is composed are combinations
Prof. Miller on some Crystallized Furnace Products.

of the forms $1\overline{1}0, 1\overline{0}0, 011, 32\overline{2}, 12\overline{2}, 201, 3\overline{1}\overline{1}$ with obscure indications of $6\overline{5}\overline{5}$ and $6\overline{1}\overline{1}$.

The axis of the zone $111, 201, 11\overline{3}\overline{4}, 3\overline{1}\overline{1}, 10\overline{4}\overline{3}, 1\overline{1}$ makes an angle of $30^\circ$ with the axis of the zone $1\overline{2}\overline{2}, 011, 1\overline{2}\overline{2}, 6\overline{5}\overline{5}, 3\overline{2}\overline{2}, 100, 6\overline{1}\overline{1}$. The angles deduced from a mean of the three observed angles are—

<table>
<thead>
<tr>
<th>Form</th>
<th>Angle 1</th>
<th>Angle 2</th>
<th>Angle 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>$111, 201$</td>
<td>42</td>
<td>25:6</td>
<td></td>
</tr>
<tr>
<td>$111, 11\overline{3}\overline{4}$</td>
<td>57</td>
<td>59:2</td>
<td></td>
</tr>
<tr>
<td>$111, 3\overline{1}\overline{1}$</td>
<td>61</td>
<td>19:1</td>
<td></td>
</tr>
<tr>
<td>$111, 10\overline{4}\overline{3}$</td>
<td>64</td>
<td>52:7</td>
<td></td>
</tr>
<tr>
<td>$111, 1\overline{1}\overline{0}$</td>
<td>90</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>$201, 210$</td>
<td>39</td>
<td>25:6</td>
<td></td>
</tr>
<tr>
<td>$3\overline{1}\overline{1}, 3\overline{1}\overline{1}$</td>
<td>52</td>
<td>2:2</td>
<td></td>
</tr>
<tr>
<td>$1\overline{1}\overline{0}, 10\overline{1}$</td>
<td>60</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>$111, 1\overline{2}\overline{2}$</td>
<td>57</td>
<td>43:2</td>
<td></td>
</tr>
<tr>
<td>$111, 011$</td>
<td>38</td>
<td>21:8</td>
<td></td>
</tr>
<tr>
<td>$111, 1\overline{2}\overline{2}$</td>
<td>17</td>
<td>3:4:1</td>
<td></td>
</tr>
<tr>
<td>$111, 6\overline{5}\overline{5}$</td>
<td>5</td>
<td>39</td>
<td></td>
</tr>
<tr>
<td>$111, 3\overline{2}\overline{2}$</td>
<td>12</td>
<td>44:6</td>
<td></td>
</tr>
<tr>
<td>$111, 1\overline{0}\overline{0}$</td>
<td>57</td>
<td>43:2</td>
<td></td>
</tr>
<tr>
<td>$111, 6\overline{1}\overline{1}$</td>
<td>70</td>
<td>9:2</td>
<td></td>
</tr>
<tr>
<td>$100, 2\overline{2}\overline{1}$</td>
<td>50</td>
<td>0:8</td>
<td></td>
</tr>
<tr>
<td>$100, 010$</td>
<td>94</td>
<td>8:3</td>
<td></td>
</tr>
<tr>
<td>$011, 101$</td>
<td>65</td>
<td>0:3</td>
<td></td>
</tr>
<tr>
<td>$1\overline{1}\overline{0}, 210$</td>
<td>70</td>
<td>17:2</td>
<td></td>
</tr>
<tr>
<td>$1\overline{1}\overline{0}, 100$</td>
<td>42</td>
<td>55:8</td>
<td></td>
</tr>
<tr>
<td>$1\overline{1}\overline{0}, 3\overline{1}\overline{1}$</td>
<td>63</td>
<td>58:9</td>
<td></td>
</tr>
<tr>
<td>$100, 3\overline{1}\overline{1}$</td>
<td>26</td>
<td>1:1</td>
<td></td>
</tr>
<tr>
<td>$100, 201$</td>
<td>27</td>
<td>21:4</td>
<td></td>
</tr>
</tbody>
</table>

Garnet.

On a fragment of slag in the possession of Dr. Percy are implanted a few crystals having the appearance of a dark-coloured garnet. One of these, nearly 2 millims. in diameter, having very bright and perfect faces, on being measured with the reflective goniometer, proved to be a combination of the forms $011, 211$, like fig. 347 of the new edition of Phillips's 'Mineralogy.' The slag in which the crystal occurs is said to have been found in one of the Monkland furnaces, in the large accumulation, consisting in great proportion of metallic iron, which forms in the hearth of a blast furnace, and is commonly known by the term
M. Wurtz on Propyle-glycol.

'bear.' In a note referring to these crystals, Dr. Percy observes that, apart from the testimony of the finder, he is convinced by the appearance of the specimen that it must have come from the bear of a blast furnace. Other instances of the occurrence of garnet in furnace products were observed by v. Kobell and Studer (Gurtt, Uebersicht der pyrogenen künstlichen Mineralien, p. 61). No attempt was made to analyse the crystals, or to determine their specific gravity, the whole quantity being very small, and apparently mixed up with much foreign matter.

**Silicium.**

Among some crystals of silicium given to me by T. H. Henry, Esq., by whom they were prepared, are several twins, the individuals of which are octahedrons, having a face of the octahedron for the twin face, and, as frequently happens, excessively thin in the direction of the twin axis. One of these, about 1·1 millim. long, 0·6 millim. wide, and 0·08 millim. thick in the direction of the twin axis, enabled me to obtain an approximate value of its index of refraction, by observing the polarizing angle of light reflected from its two largest faces. By a mean of sixteen observations, this angle appeared to be $75.5^\circ$ 1'. The resulting index of refraction is 3·736.

XXXII. Chemical Notices from Foreign Journals.

*By E. Atkinson, Ph.D.*

[Continued from p. 219.]

In his first paper on glycol*, Wurtz indicated the existence of the higher homologues of that body; that as glycol corresponds to alcohol, so propyle-glycol corresponds to propylic alcohol, and amyle-glycol to amyle alcohol; and, in general, that to each monoatomic alcohol there corresponds a biatomic glycol. He has now described more fully the propyle† and amyle‡ glycols.

Propyle-glycol is obtained by synthesis from propylene gas. According to Wurtz, the method of obtaining this gas described by Dusart§ is not very productive. The best method is that of Reynolds, which consists in passing the vapour of amyle alcohol through a porcelain tube heated to dull redness, and collecting the gas, after passing it through caustic potash solution, over water. The crude propylene gas is then treated with bromine, by which it is readily absorbed: the liquid product of the action consists of the bromides of ethylene, propylene, together with small quantities of bromide of butylene. These are separated

---

‡ Ibid. vol. xlvi. p. 244.
§ Phil. Mag. vol. xi. p. 372.
by repeated rectification; the fraction boiling between 140° and 145° C. is pure bromide of propylene.

320 grms. of acetate of silver were treated with 192 grms. bromide of propylene, as much glacial acetic acid added as made the whole of a pasty consistence, and the mixture heated for four days in the water-bath. Ether was then added, the mixture filtered, and the liquid fractionally distilled,—that portion boiling between 180°—190° being acetate of propyle-glycol. Its composition is \( C^{14}H^{12}O^{6} = \left( C^{4}H^{3}O^{2} \right)^{2} \) \( O^{4} \); and its formation may be thus expressed:

\[
C^{6}H^{6}Br^{2} + 2C^{4}H^{3}O^{2}Ag \rightarrow C^{6}H^{6}O^{2} = \left( C^{4}H^{3}O^{2} \right)^{2} + 2Ag Br.
\]

Bromide of Acetate of silver. Acetate of propyle- Bromide of propylene. silver.

Acetate of propyle-glycol is a colourless neutral liquid, insoluble in water, and with an odour somewhat resembling acetic acid. It boils at 186° C., and has the specific gravity 1·109.

Propyle-glycol was obtained by treating pure acetate with hydrate of potash and distilling the mixture. This distillate was rendered slightly alkaline with a small quantity of potash, distilled again, that portion collected which boiled above 180°, and this again rectified. The reaction is thus expressed:

\[
C^{6}H^{6}O^{4} + 2KOH \rightarrow C^{4}H^{3}O^{2} + 2K \left( C^{4}H^{3}O^{2} \right)^{2}.
\]


When pure it is a colourless oily liquid with a sweetish taste. It boils at 188° C., thus exhibiting the strange anomaly that its boiling-point is lower than that of its lower homologue, glycol. When treated with even very dilute nitric acid it is decomposed, hydrogen and carbon being eliminated, and glycolic acid, the oxidation product of glycol, being obtained. But by inducing a slow oxidation by means of platinum-black, a different result is obtained. On the flat bottom of a flask, 70 grms. of a mixture of platinum-black and spongy platinum were placed, and after filling the flask with carbonic acid, 6 grms. of propyle-glycol dissolved in 10 grms. water were added. The flask was left unclosed for eight days, so that the air could gradually penetrate. At the expiration of that time the liquid had become strongly acid, and after purification it was found to consist of lactic acid containing a small quantity of glycolic acid. Hence, by the slow oxidation of propyle-glycol, lactic acid is formed:

\[
C^{6}H^{8}O^{4} + O^{4} = H^{2}O^{3} + C^{6}H^{6}O^{6}.
\]

Propyle-glycol. Lactic acid.

And it is worthy of notice, that the starting-point for the pre-
preparation of propyle-glycol and of lactic acid is propylene gas, which may be prepared from purely inorganic materials.

Amyle-glycol is prepared in a similar manner. The acetate of amyle-glycol is first prepared by acting upon bromide of amylene with acetate of silver; and, from the acetate, amyle-glycol is obtained by the action of potash. When pure it is a colourless, syrupy, bitter liquid, becoming viscous at low temperatures. Its specific gravity (0.987) is lower than that of propyle-glycol. It boils at 177° C., and distils without decomposition; and it is remarkable that its boiling-point is lower than that of propyle-glycol, which, again, is below ethyle-glycol. Treated with nitric acid it is rapidly oxidized, and an acid is formed having the formula C₈H₈O₆, which Wurtz names butylactic acid. It is the third term in the lactic acid series, which now consists of glycemic acid, C₄H₄O₆; lactic acid, C₆H₆O₆; butylactic acid, C₈H₈O₆. Probably lentic acid, C₁₂H₁₂O₆, is a member of this series.

The acetate of glycol has the same composition as a combination of two equivalents of anhydrous acetic acid with one equivalent of aldehyde,

\[
C_4H_4O_2\left\{\frac{C_4H_3O_3}{C_4H_4O_2}\right\}^2\]

and from theoretical considerations Geuther was led to consider this to be the constitution of the body. Geuther sought to verify this supposition experimentally by heating together one equivalent of aldehyde and two equivalents of anhydrous acetic acid in a closed tube to a temperature of 180° C. After keeping the tube for twelve hours at this temperature, it was opened and its contents submitted to fractional rectification. Besides uncombined aldehyde and anhydrous acetic acid, a substance was present which, after being washed with water, dried, and rectified, was found to have a constant boiling-point of 160° C. On analysis it had the same composition as acetate of glycol. It is, however, only isomeric with that body; for, besides the fact that its boiling-point is 20 degrees lower, its other properties are different. It has an alliaceous odour, and it appears to acidify by repeated rectification. Treated with potash it yields acetate of potash, and an odour is perceptible which is like that produced when aldehyde alone is treated with potash.

Anhydrous succinic and benzoic acids seem to form similar compounds with aldehyde; and anhydrous acetic acid forms with benzoic aldehyde a definite compound, with the study of which the author is at present engaged.

In an investigation of the acids contained in croton oil,

Schlippe* has discovered an acid belonging to the oleic series, which lies between acrylic and angelic acids.

After the saponification of the fatty acids contained in this oil, which are stearic, palmitic, myristic, and lauric acids, besides probably some higher members of the oleic series, an almost black subjacent liquor remains, which contains the new acid along with other substances. For the purpose of separation, this liquid was distilled, on which an acid liquid passed over. This was neutralized with baryta, and the baryta salt distilled with tartaric acid, when oily drops passed over along with the acid liquor, and towards the end of the operation crystals of angelic acid were deposited in the condenser. This distillate was neutralized with baryta, and again distilled with tartaric acid, and this treatment repeated. Finally, some of the baryta compound was distilled with aqueous phosphoric acid. In the receiver the acid was obtained in the form of limpid oily drops along with a small quantity of water, in which it is tolerably soluble. It contained, however, other acids; and for the purpose of separation, recourse was had to partial distillation. Of the acid which first distilled, a silver salt was prepared, which gave on analysis numbers agreeing with the formula C⁸ H⁵ Ag O⁴, which would be the formula of the silver salt of an acid between angelic and acrylic acids. Schlippe names the acid crotonic acid.

The acids of this series decompose when heated with excess of caustic potash into acetic acid and an acid of the same series: thus angelic acid decomposes into acetic acid and propionic acid; oleic acid into acetic acid and palmitic acid. In like manner, crotonic acid, if it belong to this series, ought to decompose into two equivalents of acetic acid,

\[ C^8 H^5 O^4 + 2KO HO = 2C^4 H^3 KO^4 + 2H, \]

Crotonic acid. Potash. Acetate of potash.

which Schlippe found to be the case.

Crotonic acid has neither inflammatory nor purgative properties.

XXXIII. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 229.]

February 18, 1858.—Leonard Horner, Esq., V.P., in the Chair.

"On the Functions of the Tymanum." By James Jago, A.B. Cantab., M.B. Oxon, Physician to the Royal Cornwall Infirmary.

As in my present effort to obtain further light upon some of the still obscure points in the physiology of the ear I have been primarily guided by observations made upon my own ears, I should premise that both are very efficient for hearing; but that they differ from each other in the important particular that the faucial orifice

* Liebig’s Annalen, January 1858.
of the right Eustachian tube closes much less tightly than that of the
left, insomuch that there are times when the former becomes quite
patent, with no disposition to collapse. Again, having lately been
troubled for above five weeks with a tympanic deafness, I carefully
registered a series of auditory phenomena resulting therefrom, and
found them exceedingly noteworthy. Lastly, I have made certain
experiments upon the external auditory canals of the sound ears.

I compare, then, with one another, the phenomena yielded by a
normal ear, an ear with an open Eustachian tube, an ear with the drum
impaired in a particular manner, and an ear whose external meatus is
in a known altered condition; calling in facts from other sources in
aid; and, finally, endeavour to determine the uses to be assigned
to the several structures of the drum in order to embrace all the
phenomena*.

I assure myself that my Eustachian tubes are ordinarily shut, by
the difficulty (greater for my left one) of forcing the breath into the
drums when I stop my mouth and nose, and the hinderment to its
evacuation till I swallow or eructate, showing that those acts open the
tubes. If we mark the sinking-in of the lacrimal sac when we swallow
with the mouth and nose stopped, we may see that the naso-guttural
cavity enlarges as the glottis is closed in that act, producing a partial
vacuum in the drums, and therefore from the greater barometric
pressure a feeling of tightness upon the membrana tympani, whilst
from loss of usual pressure the Eustachian tubes thereupon close more
firmly, and the faucial parts swell and stick together.

I can readily distinguish the act of opening the Eustachian tube
from all other guttural ones, both by hearing and feeling. A tearing
sound, or an irregular run of clicks, marks a slower, a sharp click a
quicker opening of the tube, a souffle the rush of air through the
patent tube, and a small crack the displacement of the membrana
tympani. I frequently perceive these phenomena in deglutition,
though, owing to the strong pressure of the current of ejected air in
the faucial parts, more especially in eructation. Sometimes also in yawning,
showing that a suddering contraction of the muscles of the pharynx
and palate attends the opening of the tube.

With the tube patent I feel the membrana tympani, as expiration
and inspiration alternate the greater amount of pressure in its two
surfaces, oscillating from outwards to inwards, as the inner canthus
of the eye, as reached by the nasal duct, may be seen to do. In
violent explosive expirations, the strength of the membrane is
severely tested; the mildest speaking, coughing, or sneezing even, is
always disagreeably felt thereon.

But to pass to the attendant sonorous phenomena:—the rippling
of the air in the tube at each elevation and depression of the ribs
expresses itself by a souffle, and every word I utter is taken to the
labyrinth directly through the tube with a force that proves annoy-
ing;—observations which plainly evince why the Eustachians are
usually impervious, and why they almost never open except at that

* A note shows that, though I speak particularly from these sources, the results
rest on much broader grounds; and mentions how far anything like any portion
of this paper has been previously published by myself or others.
instant of deglutition, or of the reverse act, eructation, which occludes the glottis.

From numberless observations, I am able to affirm that the faculty of audition is not at all deteriorated by patency of the tubes, however the ordinary use of the ear may be perplexed by sounds entering the tube. Nor does stretching the membrana tympani, by augmenting or diminishing the aerial pressure on its inner surface, enfeeble hearing.

I will now turn to observations made upon my left ear when it was deafened. I show that the external meatus was unaffected; and if I rubbed my finger over the skin covering the bone behind the ear, or carried the ticking of a watch to the bottom of the meatus by means of a metallic probe, and then did the like to the other ear, I heard well, and as well upon one as the other. Hence the labyrinth and acoustic nerve remained healthy, and the drum alone was affected. Singing noises in the head had been developed just to the same extent as hearing had been blunted,—phenomena that for three weeks before an instantaneous cure remained quite unchanged.

The noises were caused by the circulation of the blood about the drum, for they rose and fell as the circulation was quick or otherwise. And I was led to the belief that these noises were not created by any morbid change of local circulation, but that, by a morbid change in the acoustic properties of the tympanum, ordinary movements of the blood thereabouts were heard in a multiplied manner; for the click and souffle from air entering the Eustachian tube, as heard in the healthy ear, were wonderfully magnified in the deaf one. The louder souffle, that of eructation, normally but very weak, even when the intruding air strongly forces outwards the membrana tympani, in the deaf ear was always a very pronounced bruit. And a couple of other sounds from distinct sources generated within the site of the membrane are described, which, hardly audible in a normal ear, are loud in an ear thus diseased.

Thus a group of phenomena beckon to the inference, that this deafness had so modified the acoustic properties of the drum, as both to render all sonorons vibrations affecting the air within it by far more audible than before, and all those entering the meatus auditiorius externus as much less audible than before. What physical cause can bring about these inverse effects?

1. If the fenestra rotunda be the chief portal for sound, no change at it could render one set of sounds more audible without doing so for the other also.

2. If sound be mainly conveyed to the labyrinth by undulatory displacements of the membrana tympani, causing bodily oscillation of the ossicles, the membrane could not be rendered more responsive to aerial waves falling upon one side of it without becoming equally so for those falling upon its other.

3. If the fenestra rotunda chiefly afford passage to sound, and the membrana tympani has acquired an abnormally high reflecting power, repelling vibrations that would heretofore have escaped through it from the drum back upon this fenestra, and those that fall upon its outer surface back through the meatus, effects of an in-
verse kind do result. This hypothesis, therefore, cannot be rejected without a careful consideration.

Let us inquire, then, what influence the existence of a membrana tympani would, under this supposition, exert on hearing. Sonorous vibrations impressed upon the walls of the head, that is, of the external meatus, are heard more loudly when we anyhow cover this canal so as to close it, as any cavity when closed resounds like an open one of greater size (J. Müller). In again testing this principle, I have used various materials for closing the meatus, have plugged the entrance, and laid the thing over it, and observe always that the smallest orifice in the occluding body detracts from the resonance; which I know to occur in the confined air, and not in the parietes of the canal, for my deafened ear was deaf to it. Such experiments, however, do not evince that the membrane aids hearing by resonance, but the contrary. Dealing with vibrations already existing in the walls of the cavity insulating the air, they do not at all imitate the case of vibrations passing into the tympanum through a medium,—the membrane. As no substance can be applied over the meatus, however it be done, which does not hinder our hearing of external sounds just as much as it occasions resonance of parietal ones, the membrane on this supposition must in some degree or other be a positive detriment to the auditory function. Besides, were hearing aided by resonance within the drum, a patent Eustachian tube by allowing vibrations to disperse must impair hearing, which I know not to be the case. Again, if we assume the membrane to but slightly arrest the transition of sound from the outer to the tympanic air, to be, in short, an unavoidable impediment to hearing, fulfilling some non-acoustic purpose, the loss of it would not prove at once, as it does, a serious detriment to hearing rather than some benefit. I may append too, that were it but a trifling obstacle, the group of sounds occurring within it, so described, should be augmented by resonance in the external meatus, on its outlet being stopped; yet I can detect nothing of the sort. Further, I squeezed a plug of chewed brown paper, and one of dry paper, firmly into the bottom of the meatus of the healthy ear, against the membrane, covered the membrane with a stratum of wax, and filled the meatus with water; but in not one of these experiments were the said group of sounds rendered louder. So that it appears that the application to the membrane of even a highly reflecting surface fails to intercept and cause to return intra-tympanic sounds, which can only be because the membrane is difficult for such sound to pass through. But if the membrane highly resists the transition of aerial vibrations, it (the fenestra rotunda being the chief portal) is a serious detriment to hearing. Hence this fenestra cannot be of this acoustic consequence. And we must have recourse to the only other theory which suggests itself, which is—

4. That the membrane and ossicles form the essential path for sonorous vibrations, which traverse it by the mode of condensation and rarefaction; that aerial ones impinging upon the outer surface of the membrane easily impress themselves upon its substance, and pass into the ossicles, whilst the inner surface presents a great obstacle
to their escape into the air in the drum, and equally repels vibrations that fall upon it from this air. Thus, when disease nullifies the great reflecting qualities of the inner surface, much of the sound from without passes into the drum and is wasted, or deafness results; whilst much of that in the drum enters the membrane, and some of it finds its way along the ossicles, and noises in the head are engendered.

Now I find that the cutaneous surface of the drum-head admits vibrations from air with very much greater facility than water does, that is, readily; for on filling the external meatus of the sound ear with water, and then letting it leak out again, I remarked that for more than half an hour afterwards septa of water were constantly forming themselves across the canal and producing much deafness, and then breaking again with a loud noise, and the deafness vanishing. After some evaporation the following instructive effects alone took place:—the membrane would attract a film of water over its surface, and deafness ensue; but on a gust of air plunging into the drum through the Eustachian tube, the membrane springing outwards with a smart smack, would throw off the fluid, and the hearing as instantaneously be restored. This would gradually wane away again by the re-atraction of the water, to be instantaneously regained again, and so on. But since the transition is easy between the membrane's outer surface and air, what has been said above shows that it must be difficult between the inner surface and air, and the statement in (4.) is demonstrated.

Accordingly the external layer of the membrane is formed of skin, a dry tissue of loose texture, penetrable by air, and coming into intimate relation with it; whilst the mucous membrane of the drum is, as it were, unparalleled not only for tenuity, but compactness and high vascularity, though it is barely possible to verify the presence of mucous exudation upon it, affording a glassy surface which is a formidable barrier to the passage of vibrations from it to air, and vice versa; and this is so reflected, that the membrane and ossicles leading to the labyrinth lie without it, confining useful vibrations to their destined path, and excluding hurtful ones from it; and the mastoid cells help to further stifle such vibrations as by any accident intrude upon the air in the drum. The membrane of the fenestra rotunda, by its elasticity, protects the acoustic nerve from undue compression, &c. The membrana tympani avails acoustically by its area, whilst its flexibility, the joints in the ossicular chain, &c., are mere machinery for conveying, under all contingencies, vibrations to the fenestra ovalis, and provision against mechanical accidents. The structure of the labyrinth admits of explanation, in a great degree, upon like principles.

The personal case of deafness studied in this paper was from a cold draught on the ear, a mere inflammation of the mucous lining of the drum, ultimately forming a layer of dried mucus upon the membrana tympani, which originally involving much air-bubbles, remained very permeable by air, and assimilated acoustically the inner face of the membrane to the cuticular outer one. The instantaneous dispersion of the noises and deafness was caused by the sudden peeling off of this false cuticle; whilst a film of water upon
the cuticular face assimilates that to the inner one, when the ear excludes both tympanic and outer sounds from the labyrinth. Deafness produced by disease in the external meatus only yields noises when it propagates irritation so as to excite secretion of mucus on the inner face of the drum-head. Simple perforation of the drum-head only deafens in proportion to the extent of surface removed. If there co-exist a more or less fluid discharge from the drum, this spoils hearing by covering the cuticular surface of the membrane, though it may not deviate so much acoustically from that lined by mucous membrane as to very materially damage it. To remedy such deafness mechanically, we should first essay to rescue the cuticular face from the fluid by placing some material to draw off the discharge from it, so as to keep the membrane fit for its duties, and still exposed to aerial vibrations. If the mischief is so extensive that we are obliged to employ some disc to rest against the remaining ossicles as a substitute for the true membrane, we should try to form one with surfaces acoustically imitating those of the membrane itself.

The paper concludes by pointing out how the various injuries which have been known to occur by disease or otherwise to the different parts of the tympanum, are readily accounted for by the functional hypothesis here submitted.

February 25.—W. R. Grove, Esq., V.P., in the Chair.

The following communications were read:—

"Remarks on the interior Melting of Ice," By Professor William Thomson, F.R.S. In a Letter to Professor Stokes, Sec. R.S.

In the Number of the 'Proceedings' just published, which I received yesterday, I see some very interesting experiments described in a communication by Dr. Tyndall, "On some Physical Properties of Ice." I write to you to point out that they afford direct ocular evidence of my brother's theory of the plasticity of ice, published in the 'Proceedings' of the 7th of May last; and to add, on my own part, a physical explanation of the blue veins in glaciers, and of the lamellar structure which Dr. Tyndall has shown to be induced in ice by pressure, as described in the sixth section of his paper.

Thus, my brother, in his paper of last May, says, "If we commence with the consideration of a mass of ice perfectly free from porosity, and free from liquid particles diffused through its substance, and if we suppose it to be kept in an atmosphere at or above 0° Cent., then, as soon as pressure is applied to it, pores occupied by liquid water must instantly be formed in the compressed parts, in accordance with the fundamental principle of the explanation I have pronounced—the lowering, namely, of the freezing-point or melting-point, by pressure, and the fact that ice cannot exist at 0° Cent. under a pressure exceeding that of the atmosphere." Dr. Tyndall finds that when a cylinder of ice is placed between two slabs of box-wood, and subjected to gradually increasing pressure, a dim cloudy appearance is observed, which he finds is due to the melting of small portions of the ice in the interior of the mass. The permutation into portions of the ice for a time clear "by the water squeezed against it from such parts as may be directly subjected to the pressure," theoretically demonstrated by my brother, is beautifully illustrated by
Dr. Tyndall's statement, that "the hazy surfaces produced by the compression of the mass were observed to be in a state of intense commotion, which followed closely upon the edge of the surface as it advanced through the solid. It is finally shown that these surfaces are due to the liquefaction of the ice in planes perpendicular to the pressure."

There can be no doubt but that the "oscillations" in the melting-point of ice, and the distinction between strong and weak pieces in this respect, described by Dr. Tyndall in the second section of his paper, are consequences of the varying pressures which different portions of a mass of ice must experience when portions within it become liquefied.

The elevation of the melting temperature which my brother's theory shows must be produced by diminishing the pressure of ice below the atmospheric pressure, and to which I alluded as a subject for experimental illustration, in the article describing my experimental demonstration of the lowering effect of pressure (Proceedings, Roy. Soc. Edinb. Feb. 1850), demonstrates that a vesicle of water cannot form in the interior of a solid of ice except at a temperature higher than 0° Cent. This is a conclusion which Dr. Tyndall expresses as a result of mechanical considerations: thus, "Regarding heat as a mode of motion," "liberty of liquidity is attained by the molecules at the surface of a mass of ice before the molecules at the centre of the mass can attain this liberty."

The physical theory shows that a removal of the atmospheric pressure would raise the melting-point of ice by \( \frac{3}{4} \) of a degree Centigrade. Hence it is certain that the interior of a solid of ice, heated by the condensation of solar rays by a lens, will rise to at least that excess of temperature above the superficial parts. It appears very nearly certain that cohesion will prevent the evolution of a bubble of vapour of water in a vesicle of water forming by this process in the interior of a mass of ice, until a high "negative pressure" has been reached, that is to say, until cohesion has been called largely into operation, especially if the water and ice contain little or no air by absorption (just as water freed from air may be raised considerably above its boiling-point under any non-evanescent hydrostatic pressure). Hence it appears nearly certain that the interior of a block of ice originally clear, and made to possess vesicles of water by the concentration of radiant heat, as in the beautiful experiments described by Dr. Tyndall in the commencement of his paper, will rise very considerably in temperature, while the vesicles enlarge under the continued influence of the heat received by radiation through the cooler enveloping ice and through the fluid medium (air and a watery film, or water) touching it all round, which is necessarily at 0° Cent. where it touches the solid.

I find I have not time to execute my intention of sending you today a physical explanation of the blue veins of glaciers which occurred to me last May, but I hope to be able to send it in a short time.

Jan. 21, 1858.

William Thomson.

"On the Practical Use of the Aneroid Barometer as an Ororometer." By Capt. W.S. Moorsom, Member of the Institution of Civil Engineers.

A Government Commission to Ceylon in the beginning of 1857,
led the author, as Chief Engineer in charge of the Expedition, to provide (among other instruments) some aneroids, as a means of saving time in ascertaining the levels of the mountain passes of that Island. The aneroids offered by makers did not appear sufficiently graduated to admit of minute observation, and at the author's suggestion Messrs. Elliott furnished a more complete vernier, which, however, was shown to be susceptible of material improvement.

With these comparatively imperfect instruments, it was shown that an elevation of 950 feet may be taken to correspond with the fall through the first inch of the aneroid; that about 970 feet more corresponds with the fall through the second inch, and about 1000 feet corresponds with the fall through the third inch. These altitudes having been checked by levels taken with the ordinary surveyor's spirit-level, it was shown that this experience corresponds with the Tables published by Mr. Belville, within 1 per cent.

The thermometer, which is usually attached to the aneroid, is not a necessary adjunct, but is frequently useful, and always interesting. The compensations introduced to provide against variations of temperature, as affecting the results given by the instrument, were shown to be effectual without the aid of the thermometer.

The difficulties to be contended with in taking accurate observations were shown to be local variation, diurnal variation, and some irregularity in the action of the mechanical parts of the instrument itself. These difficulties were examined seriatim, and modes of approaching to their corrections were explained. The modes of compensation for variations of temperature affecting the instrument were shown as at present practised by the makers: the diaphragm-box being compensated by means of the introduction of a small portion of aeriform fluid, instead of being allowed to act with a perfect vacuum, and the metallic connexions between the diaphragm-box and the index being compensated by compound arms or connexions of steel and brass so adjusted as to neutralize mutually the respective contraction or expansion of each at variations extending to 100 degrees of temperature.

The mode now practised by makers of graduating the aneroid (when thus compensated) by comparison with a standard mercurial barometer, was stated, and it was suggested that improvement on this practice might be made by reference to standard elevations running up to 2000 feet at least in Great Britain. Practical examples were given of the use of the instrument in Ceylon, showing the variations of the aneroid (when properly checked) to lie between 1 foot and 6 feet, as compared with the surveyor's spirit-level: other examples were given of practice on the Great Western, South-Eastern, and North Kent Railways, varying from the true levels from 6 inches to 6 feet, over distances of between 300 and 400 miles.

March 4.—The Lord Wrottesley, President, in the Chair.

"On the Stratifications and Dark Bands in Electrical Discharges as observed in Torricellian Vacuums." [The Bakerian Lecture.] By John P. Gassiot, Esq., V.P.R.S.

The author refers to the stratified appearance of the electrical dis-
charge when taken from the terminals of a Ruhmkorff's induction-coil in the vapour of phosphorus, and in highly attenuated gases, first noticed by Mr. Grove (Phil. Trans. Part I. 1852, and Phil. Mag., Dec. 1852). Having witnessed the experiments of Mr. Grove, Mr. Gassiot in the same year examined the discharge in a barometrical vacuum in which the mercury had been carefully boiled, but he could not obtain any signs of stratification. These experiments were subsequently repeated by several continental electricians, who all describe the induction-discharge in a barometrical vacuum as intensely white, and filling the whole tube without stratification.

After alluding to the experiments of the Rev. Dr. Robinson (Proc. R. I. Acad., Dec. 1856), and to some recent improvements in the construction of the induction-coil, the author proceeds to describe apparatus which he had constructed for the more careful examination of the character of the induction-discharge. His first experiments were made on glass tubes about 10 inches long, in which the mercury could be lowered or raised to any required level by means of the air-pump. He also experimented with barometrical vacuums obtained by inverting a tube of about 44 inches in length, filled with boiled mercury, over a vessel containing that metal, and then sealing the tube 2 or 3 inches above the barometrical height.

The results obtained by these methods having been found unsatisfactory, the author had recourse to that first suggested by Mr. Welsh (Phil. Trans. 1856, p. 507), by which that gentleman constructed the large barometer at the Kew Observatory. Following out the principles indicated by Mr. Welsh, by carefully removing all trace of moisture, and thoroughly cleaning the tubes before introducing the mercury, the author succeeded in obtaining Torricellian vacuums which exhibit the stratifications in a uniform and very marked manner.

The sealed tubes generally used by Mr. Gassiot are then described. They are made of the usual glass tubing, about an inch internal diameter, and of the form fig. 1.

They vary from 10 to 38 inches in length. In the latter case the platinum wires \(ab\) are about 32 inches apart. One tube is described 5 feet 3 inches in length, with wires 4 feet 9 inches apart.

With a tube prepared on Mr. Welsh's principle, and the usual-sized Ruhmkorff's induction-coil excited by a single cell of Grove's nitric acid battery, with or without a condenser, the phenomena of the stratified discharge can be seen and examined with ease, and without the trouble and uncertain manipulation of an air-pump, or the employment of phosphorous or other vapours.

If the discharges are made in one direction, a black deposit takes place on the sides of the tube nearest the negative terminal. This deposit is platinum in a state of minute division emanating from the wire, which becomes black and rough as if corroded. The minute particles of platinum are deposited in a lateral direction from the negative wire, and consequently in a different manner from what is described as occurring in the voltaic arc, so that the luminous ap-
pearance of discharge from the induction-machine can in no way arise from the emanation of particles of the metal.

The author describes a series of experiments made in the apparatus first prepared, by which the mercury is lowered or raised in the vacuum tube; he describes the peculiar appearance when the mercury is made either positive or negative. In some instances, and particularly when, instead of wires, platinum balls \( \frac{1}{4} \)th of an inch in diameter were used for terminals, the stratifications instantly ceased when the mercury rose above the negative ball; but when the pole of a magnet was presented to the positive ball, the stratifications were drawn to the length of two or three inches down the tube.

In the sealed tubes the stratified discharge was obtained by frictional electricity; and if a charged Leyden jar is discharged through the vacuum by a wet string, the stratifications are as distinct as from the induction-coil.

The author next proceeds to show, that by a single disruptive discharge of the primary current excited by a single cell, the entire tube, whatever may be its length, is filled with stratifications as far as the dark band near the negative wire; and from this experiment he is of opinion that the phenomenon cannot be in any way due to the vibrations of the contact-breaker. With one, two or three cells no appearance of a luminous discharge could be perceived on making contact, it only appeared on breaking. If, however, the intensity of the primary current is increased by using ten or more cells, stratifications appear on making as well as on breaking the contact of the primary circuit. These stratifications are always concave towards the positive terminal, and as the discharges, on making and on breaking, emanate from different terminals, their concavities are in opposite directions,—a fact which explains the different ways in which several electricians have described and figured the form of the discharge with the coil. These stratifications appear in quick succession, but they can always be separated in any part of the tube by a magnet.

Under certain conditions the positive discharge assumes a peculiar form, of which the author gives a drawing. He considers that this exhibits a direction of a force from the positive to the negative, centering to the axis of stratification, which cannot be connected with the passage of particles, and that the latter phenomenon, as it occurs in the voltaic arc, may be but the result of a secondary action.

The author notices the peculiar difference between the positive and negative discharge; he describes an apparatus by which both terminals could be made of surfaces of mercury, or the positive of a surface of mercury, and the negative of a wire, or the reverse. In this apparatus, moreover, the mercury at one end could be elongated 8 or 10 inches. When the mercury was negative, its entire surface was covered with a brilliant glow; when positive, the extreme point of the mercury exhibited intense light, but the remainder of the surface appeared unaffected by the discharge. In order to test whether any signs of interference could be detected, he had a tube prepared with four wires, by which discharges could be observed when taken from separate coils, as shown in fig. 2, where \( a b \) and \( a' b' \) are platinum wires hermetically sealed, as in the previously described apparatus. Care was taken to manipulate with induction-coils giving
discharges of equal intensity; but in no case did any sign of inter-
ference appear. The discharges, whether in the same or in opposite
directions, mingled; the stratifications, having a tendency to rotate
round the poles of a magnet and obeying the well-known law of
magnetic rotations, could be separated by either pole.

If, instead of sealed wires, tin-foil coatings, a b (fig. 3), are placed
on the vacuum tube, and the coatings are attached to the terminals
of the induction-apparatus, brilliant stratifications immediately ap-
pear in the portion of the vacuum between the coatings, but without
any dark discharge. On approaching a powerful magnet, the stra-
tifications divide into two equal series, in which the bands or strata
are concave in opposite directions.

If a vacuum tube, with or without wires or coatings, is placed on
the induction-coil, or on the prime conductor of an electrical machine,
stratifications appear which are divided by the magnet. Having
thus ascertained that there are two distinct forms of the stratified
electrical discharge, the author, for the sake of clearness of expres-
sion, terms them the direct and the induced discharge. The direct
discharge is that which is visible in a vacuum when taken from two
wires hermetically sealed therein; this discharge has a tendency to
rotate, as a whole, round the poles of a magnet. The induced dis-
charge is that which is visible in the same vacuum when taken from
two metallic coatings attached to the outside of the tube, or from
one coating and one wire; this discharge is divided by the magnet,
and the two divisions have a tendency to rotate in opposite direc-
tions. The character of these two forms of electrical discharge can
always be determined by the magnet.

The author concludes his paper in the following words:—“I refrain
for the present from any observations as to the action of the magnet
on the discharge. The intimate relation of magnetic and electric
action has long since been shown; but the curious effect of the
power of a magnet to draw out the stratifications from the positive
terminal, and in some instances its powerful action on that portion of
the discharge which exhibits the phosphorescent light in its great-
est intensity, are worthy of further examination. In the preceding
experiments my object was directed to the examination of the stra-
tified and of the dark band discharge; at present I am inclined to
the opinion that the stratifications in the positive, and the dark band
between it and the negative glow, although apparently similar, are
effects arising from distinct causes—the former from pulsations or
impulses of a force acting in a highly attenuated but resisting
medium, the latter from interference. I am at this time engaged in
making further experiments for the elucidation of this novel and
remarkable phenomenon.”
March 11.—Dr. Hooker, Vice-President, in the Chair.

The following communication was read:


Former investigations had led me to some general conclusions regarding the molecular constitution of the organic bases, which I have communicated to the Royal Society, and which have been published in the 'Transactions' (1850, p. 93; 1851, p. 357). My experiments had proved that each equivalent of hydrogen in ammonium may be replaced by an equivalent of a mono-atomic electro-positive radical, such as methyle, ethyle, &c.;—a series of compound ammoniums being produced, the salts of which may be thus formulated:

\[
\begin{align*}
\text{N} \left\{ \begin{array}{c} 
H \\
H \\
H \\
H \\
\end{array} \right\} \text{Cl} \left\{ \begin{array}{c} 
R' \\
H \\
H \\
H \\
\end{array} \right\} & \text{Cl.} \\
\text{N} \left\{ \begin{array}{c} 
R' \\
H \\
H \\
H \\
\end{array} \right\} & \text{Cl.} \\
\text{N} \left\{ \begin{array}{c} 
R' \\
H \\
H \\
H \\
\end{array} \right\} & \text{Cl.} \\
\text{N} \left\{ \begin{array}{c} 
R' \\
H \\
H \\
H \\
\end{array} \right\} & \text{Cl.}
\end{align*}
\]

R' representing a mono-atomic electro-positive radical.

These successive substitutions were accomplished by the action of ammonia upon the bromides and iodides of the alcohol-radicals, which since that time have become most valuable agents of substitution in the hands of chemists.

All the bases produced by this process being derived from 1 equiv. of ammonium, contain 1 equiv. of nitrogen; they differ in this respect essentially from the majority of the alkaloids extracted from plants, and more particularly so from those which, like quinine, morphine, strychnine, &c., specially claim our interest. By far the greater number of the vegetable alkaloids contain 2 equiv. of nitrogen. In some vegetable and animal bases we find even 3 and 4 equiv. of nitrogen. The molecular construction of these bodies is still obscure, but it is extremely probable that they are derived from 2, 3 or 4 ammonia equivs., in which the hydrogen is more or less replaced by poly-atomic molecules, and that the stability of such complicated structures essentially depends upon the substituting capacity of their replacing molecules.

It was long my intention to extend my researches to the poly-ammonium bases. But my attention has been specially called to the subject by the beautiful results obtained of late, especially in France, by the study of the poly-acid alcohols, by the experiments of M. Berthelot, and more particularly by the classical researches of M. Wurtz, which enable us to take a general view of this subject.

Taking as a point of departure the neutral compounds which are formed by the action of ammonia upon bibasic and tribasic acids, the diamides and triamides, derived respectively from 2 or 3 equivs. of ammonia, it became extremely probable that the action of ammonia upon poly-acid alcohols would give rise to poly-ammonium bases. In the conception of this analogy there appeared but little doubt that ammonia, under the influence of the bromides and iodides of bi-acid alcohols, would furnish a series of bi-ammonium bases, exactly as treatment of ammonia with the analogous compounds of mono-acid alcohols has given rise to the formation of the mon-ammonium bases above referred to. In other words, it was to be expected that
a compound ether R" Br₂ or R" I₂ (R" representing a bi-atomic electro-positive radical) would act upon two equivalents of ammonia, producing a series of salts expressed by the following formulæ:—

\[
\text{N}_2 \left\{ \frac{\text{R}''}{\text{H}_2} \right\} \text{Br}_2, \quad \text{N}_2 \left\{ \frac{\text{R}''}{\text{H}_2} \right\} \text{Br}_2, \quad \text{N}_2 \left\{ \frac{\text{R}''}{\text{H}_2} \right\} \text{Br}_2, \quad \text{N}_2 \left\{ \frac{\text{R}''}{\text{H}_2} \right\} \text{Br}_2.
\]

In endeavouring experimentally to verify this idea, it became necessary to examine what had hitherto been done in this direction. Science possesses already some very interesting observations on the ammonia derivatives of bi-acid alcohols. About five years ago, soon after the publication of my experiments upon the action of ammonia upon bromide and iodide of ethyle, M. Cloëz* obtained a series of bases on submitting ammonia to the action of the brominated Dutch liquid (C₄H₄Br₂). Two of these bodies he described under the name of formylia and acetylia, whilst a third body subsequently obtained is designated by the term propylia†.

To these three bodies M. Cloëz attributes the following formulæ:—

\[
\text{Formylia} \quad \ldots \ldots \quad \text{C}_4\text{H}_2\text{N} \\
\text{Acetylia} \quad \ldots \ldots \quad \text{C}_4\text{H}_5\text{N} \\
\text{Propylia} \quad \ldots \ldots \quad \text{C}_6\text{H}_4\text{N}.
\]

At a later period M. Natanson has studied the action of ammonia on the chlorinated Dutch liquid (C₄H₅Cl₂). This reaction produces analogous results, but the number of bases is smaller, the chief product being a chloride, which contains a base either identical or isomeric with the acetylia of M. Cloëz.

When carefully considering the results obtained by M. Cloëz, it appeared to me probable that the bases which he describes, are in fact the di-ammonium compounds for which I was searching. The constitution assigned by M. Cloëz to his substances is not very probable. It is difficult to understand how the action of ammonia upon a compound like the Dutch liquid can produce simultaneously three bodies belonging to three different homologous families, the formyle-, acetyle-, and propyle-series. Our doubts are, however, increased if we examine into the physical characters of these bodies, especially if we consider their high boiling temperatures, and the differences between the boiling-points of the three bases:—

\[
\text{Formylia} \ldots \ldots \quad \text{C}_4\text{H}_2\text{N} \ldots \ldots \quad 123^\circ \text{C}\] difference 47. \\
\text{Acetylia} \ldots \ldots \quad \text{C}_4\text{H}_5\text{N} \ldots \ldots \quad 170^\circ \text{C}\] difference 40. \\
\text{Propylia} \ldots \ldots \quad \text{C}_6\text{H}_4\text{N} \ldots \ldots \quad 210^\circ \text{C}\] difference 40.

Methylamine, C₂H₂N, which contains only 2 equivalents of hydrogen more than formylia, is at the common temperature a gas, and liquefies only considerably below the freezing-point of water. Again, the differences of the boiling-points of substances, related in the way that the formulæ of M. Cloëz suppose, do not often exceed 20°, and very rarely rise to 40° and 47°.

All these difficulties disappear by submitting the formulæ of M. Cloëz to a slight alteration, and by regarding formylia, acetylia and propylia as the di-ammonium bases of the same series, of the ethylene series. If we adopt this view, the three bodies are de-

Dr. Hofmann on the Poly-ammonias.

rived from 2 of ammonia, in which 2, 4 or 6 equivalents of hydrogen are replaced respectively by 1, 2 or 3 equivalents of the bi-atomic molecule ethylene; and the formylia, acetylia and propylia of M. Cloüz present themselves as monethylene-diamine, diethylene-diamine and triethylene-diamine.

I have endeavoured experimentally to solve this question. The analysis of acetylia, which is remarkable for the definite character of its salts, appeared to promise an answer to it.

When repeating the beautiful experiments of M. Cloüz, I had occasion to confirm all the indications given by this able chemist, regarding the formation of the bases derived from bibromide of ethylene. The analysis, however, furnished a discrepant result.

M. Cloüz represents formylia by the formula

\[ C_2\cdot H_3\cdot N \]

when the hydrochlorate becomes

\[ C_2\cdot H_3\cdot N \cdot H\cdot Cl = C_2\cdot H_4\cdot N\cdot Cl. \]

When considered as a di-ammonium compound, this salt has the composition

\[ C_4\cdot H_8\cdot N_2, 2HCl = C_4\cdot H_{10}\cdot N_2\cdot Cl_2 = 2C_2\cdot H_5\cdot N\cdot Cl. \]

The two formulae only differ by one equivalent of hydrogen.

The analysis of a magnificently crystallized hydrochlorate has furnished me the following results:

<table>
<thead>
<tr>
<th>Formula of M. Cloüz</th>
<th>New formula</th>
<th>Analysis</th>
</tr>
</thead>
<tbody>
<tr>
<td>( C_2\cdot H_4\cdot N\cdot Cl )</td>
<td>( C_4\cdot H_{10}\cdot N_2\cdot Cl_2 )</td>
<td>Mean</td>
</tr>
<tr>
<td>Carbon .... 18·32 ......</td>
<td>18·04 ......</td>
<td>17·87</td>
</tr>
<tr>
<td>Hydrogen ... 6·10 ......</td>
<td>7·51 ......</td>
<td>7·55</td>
</tr>
<tr>
<td>Chlorine .... 54·19 ......</td>
<td>53·38 ......</td>
<td>53·17</td>
</tr>
</tbody>
</table>

On preparing the free base by the action of hydrate of potassa upon the hydrochlorate, I was surprised to find that this body retains hydrogen and oxygen in the proportion in which they exist in water, which cannot be separated by prolonged contact with, or by repeated distillation over, anhydrous baryta.

The analysis of the free base has given the following result:

<table>
<thead>
<tr>
<th>Formula of M. Cloüz</th>
<th>New formula</th>
<th>Analysis</th>
</tr>
</thead>
<tbody>
<tr>
<td>( C_2\cdot H_4\cdot N\cdot O )</td>
<td>( C_4\cdot H_{10}\cdot N_2\cdot O_2 )</td>
<td>Mean</td>
</tr>
<tr>
<td>Carbon .... 31·58 ......</td>
<td>30·76 ......</td>
<td>30·67</td>
</tr>
<tr>
<td>Hydrogen .... 10·52 ......</td>
<td>12·82 ......</td>
<td>12·97</td>
</tr>
<tr>
<td>Nitrogen .... 36·84 ......</td>
<td>35·90 ......</td>
<td>36·32</td>
</tr>
</tbody>
</table>

These numbers appear to me in favour of the formula which I propose for formylia; there remains but little doubt that acetylia and propylia are analogously constituted.

There remains yet to find the last term of the series, the tetraethylene-diammonium compound. Up to the present moment I have only established by experiment that the three lower bases are powerfully attacked by bibromide of ethylene, a non-volatile compound being produced possessing properties in every respect analogous to the character of tetramethyl- and tetrethylammonium.

If further experiments confirm the hypothesis which I have advanced, the action of ammonia on bibromide of ethylene would give rise to four compounds analogous to the bases which I have obtained by the action of bromide of ethyle:
Bromide of ethyl-ammonium.

$$\text{Br} \left\{ \text{C}_4\text{H}_5 \right\} \text{N} \left\{ \text{C}_4\text{H}_5 \right\} \text{Br} \left\{ \text{C}_4\text{H}_5 \right\}$$

Bromide of diethyl-ammonium.

$$\text{Br} \left\{ \text{C}_4\text{H}_5 \right\} \text{N} \left\{ \text{C}_4\text{H}_5 \right\} \text{Br} \left\{ \text{C}_4\text{H}_5 \right\}$$

Bromide of triethyl-ammonium.

$$\text{Br} \left\{ \text{C}_4\text{H}_5 \right\} \text{N} \left\{ \text{C}_4\text{H}_5 \right\} \text{Br} \left\{ \text{C}_4\text{H}_5 \right\}$$

Bromide of tetraethyl-ammonium.

$$\text{Br} \left\{ \text{C}_4\text{H}_5 \right\} \text{N} \left\{ \text{C}_4\text{H}_5 \right\} \text{Br} \left\{ \text{C}_4\text{H}_5 \right\}$$

The conception of dianimonium-compounds has suggested to me the idea to extend my observations also to the triacid-alcohols, and to submit ammonia to the action of the bodies

$$\text{C}_2\text{H}_2 \text{Br}_3, \quad \text{C}_4\text{H}_3\text{Br}_3, \quad \text{C}_6\text{H}_5\text{Br}_3.$$  

Analogy suggested the formation in this reaction of a series of triammonium-bases, the salt of which might be thus formulated:

$$N_3 \left\{ \text{R}'''' \right\} \text{Br}_3, \quad N_3 \left\{ \text{R}''' \right\} \text{Br}_3, \quad N_3 \left\{ \text{R}'' \right\} \text{Br}_3, \quad N_3 \left\{ \text{R}''' \right\} \text{Br}_3, \quad N_3 \left\{ \text{R}'''' \right\} \text{Br}_3.$$  

I have not yet succeeded in realizing these compounds by treating under several conditions ammonia by the above chlorides and bromides of triacid-alcohols. The processes which I have as yet tried have led to other transformations. A different result is, however, obtained by replacing the ammonia in these processes by amidogen-bases. In this reaction, and especially with aniline and chloroform, a series of beautifully crystallized alkaloids is formed, the study of which engages at present my attention.

In conclusion, I may remark that several of the known basic compounds appear to belong to the triammonium-type.

The cyanethine of Kolbe and Frankland may be viewed as such a compound,

$$\text{C}_{18} \text{H}_{18} \text{N}_3 = \left\{ \left( \text{C}_6\text{H}_5 \right)'''' \right\} \text{N}_3,$$

This substance appears to me to be derived from 3 equivs. of ammonia, in which 3 equivs. of hydrogen are replaced by 3 equivs. of the tri-atomic radical which chemists assume in glycerine-alcohol.

Cambridge Philosophical Society:

[Continued from p. 233.]

Feb. 8, 1858.—A paper was read by the Rev. O. Fisher, “On the probable origin of numerous Deep Pits on some Heaths in Dorsetshire.”
Also a paper was read by Professor De Morgan, "On the Syllogism, No. III., and on Logic in general."

This paper is divided into two sections, the first of which is descriptive and controversial, the second is an abstract of the system.

Between the opinion of Kant that logic cannot be improved, and that of some recent writers, who hold it perverted, and not always correct, the truth is held to lie in this,—that existing logic, in its quod semper, quod ubique, quod ab omnibus, is true and accurate; but that it is only a beginning, and that the low estimation in which it has been held is a consequence of its incompleteness.

The modern definition of logic, the form of thought, relates to a distinction which is more familiar to mathematicians than to logicians, but is rather in the common use of the mathematician than in his clear apprehension. Aristotle, who first implicitly made the distinction of form and matter, was a mathematician; and so also was Kant, who first explicitly introduced this distinction into the definition of logic. The only two nations who had a logic taking character from the distinction, the Greeks and Hindus, are precisely the two nations to whom we owe the rudiments of our mathematics. It is affirmed by the author, that, in our time, the distinction is more in the theory of the logician than in his practice, more in the practice of the mathematician than in his theory.

Various illustrations are given of the manner in which recent logical writers have, according to Mr. De Morgan, misconceived the distinction of formal and material. In another part of the paper he suggests that this distinction has been confounded with the distinction which he designates as onymatic and non-onymatic. By onymatic he means what arises out of the use and meaning of nomenclature: thus the relation of containing and contained is an important relation of names to each other as names, or an onymatic relation.

The modern logic, by the simplicity of its final examples, is prevented from being of much use as a mental gymnastic. Instances are given of a proposition and a syllogism which are more worthy of being propounded as exercises than the instances which are found in works on logic.

The objections to symbols are discussed. Every science which has thriven has organized symbols of its own: and logic, the only science which confessedly has made no progress for many centuries, is also the only science which has grown no symbols.

The logicians have confined themselves hitherto within what Mr. De Morgan calls the logico-mathematical field: they now begin to contend for the inclusion of what he calls the logico-metaphysical. This distinction they take as that of extension and comprehension. The author contends for a distinction of extension and intension in both the sides of logic, the mathematical and the metaphysical; though undoubtedly extension predominates in the mathematical side, and intension in the metaphysical. These distinctions are onymatic. If the name C contain all that is in A or in B, or in both, symbolized by C=(A, B), then A and B are in the extension of C. But if C be contained both in A and in B, symbolized by C=A.B, or AB, then A and B are in the intension of C.
A name is used in four senses. It is the name of an object, or of a quality inhering in an object, and distinguishing a class: these two uses are objective. It is also the name of a class, or of an attribute by which the mind thinks of a class: these two uses are subjective. The subjective uses are reductions of plurality to unity, a description for the truth and reality of which the author contends.

It is affirmed that the logicians have not only confined themselves within the mathematical side of logic, but that even the recent attempts to introduce the metaphysical appear like attempts to create a second mathematical branch. This is evidenced by the manner in which unity of attribute has been discarded in favour of plurality of qualification. Thus it has been said, in obedience to the theory of quantification of the predicate, that the humanity of Newton is a different thing from the humanity of Leibnitz. That this view, though true, belongs to the mathematical side of logic, is contended for and enforced at length.

Aristotle made the distinction which the logicians now recognize as that of extension and comprehension, and which Mr. De Morgan distinguishes as that of mathematical and metaphysical reading, as follows:—In one sense the species is in the genus; in another the genus is in the species. That is, all the species are aggregants of the genus; the whole genus is a component of the notion of the species.

Recent English logicians of high name have misconceived this distinction to the extent of imagining that by changing 'Every A is some B' into 'Some B is every A,' they make the change alluded to by Aristotle. Mr. De Morgan restores the old distinction, and completely incorporates what was only partially introduced, the change of quantity which takes place in passing from the mathematical to the metaphysical reading. Thus 'Every A is B' is in the first reading 'The whole class A is one aggregant of the class B'; and in the second, 'The whole attribute B is one component of the attribute A.'

The limitation of the universe of a proposition, made throughout the author's preceding writings, is again contended for.

A proposition is the assertion or denial of a relation between two notions. Relations which are of necessity involved in nomenclature, are called onymatic; and these must be first studied. Mr. De Morgan believes that the logicians have described, under the distinction of formal and material, no more than the distinction of onymatic and non-onymatic. The mathematical notion of class, and the metaphysical notion of attribute, give four different readings of a proposition:—1. Logico-mathematical, class aggregate of class; man contained in animal. 2. Logico-physical, attribute predicated of class; animality attribute of the class man. 3. Logico-metaphysical, attribute component of attribute; animality a component of humanity. 4. Logico-contraphysical, attribute subjected to class; humanity only predicable within the class animal.

The logicians confined logical predication to the idea of class contained in class, species in genus. The genus in species, attribute component of attribute, they relegated to metaphysics. Hence their distinction of the logical and metaphysical whole. The class composed of individuals they called the mathematical whole; Mr,
De Morgan calls it the arithmetical whole, transferring the word mathematical to what was called the logical whole. The common mode of expression, as ‘Every A is B,’ &c., he considers as speaking the language of the arithmetical whole, though the speaker may attach the idea of either of the other wholes.

Extension predominates in the mathematical whole; intension in the metaphysical. The most usual mode of speech is the physical: man is educated a mathematician as to the subject of his proposition, a metaphysician as to the predicate.

The most remarkable point at issue between Mr. De Morgan and the logicians, is in his opposition to their notion of the whole attribute being the sum of its components. The difference between aggregation and composition is one of the turning-points of his whole system.

The distinctions above drawn require differences of language to express the relations which enter: the logicians have nothing but the copula is. At the outset, however, we have the distinction which is expressed by speaking of relations of terminal ambiguity and relations of terminal precision:—the first seen in ‘A is contained in B,’ where it is left unsaid whether or no A fills B; the second seen in the case in which it is implied that A is part only of B.

By speaking in the arithmetical whole, the logicians have made a system of syllogism from which the numerical syllogism cannot be excluded. The propositions ‘Some As are Bs,’ and ‘50 As are Bs,’ are of the same kind: they are both referred to the arithmetical whole. This whole is subordinate to both the mathematical and metaphysical wholes; though more prominent in the first than in the second.

When inclusion and exclusion are opposed to one another, and combined with assertion and denial, the ordinary proposition takes a form in which quantity is but an emergent incident, and not a fundamental mode of discrimination. Thus the propositions A and O are the assertion and denial of the inclusion of class in class; E and I are the assertion and denial of the exclusion of class from class.

The opposition of the two kinds of quantity, extensive and intensive, is not easy and natural, when the word quantity is used in metaphysical reading. Mr. De Morgan proposes the word force to express quantity in the second case. He finds this word in use. Thus it is sometimes said that a term is or is not used in its complete force, when the meaning is, that all the attributes of which the term is compounded are or are not involved in the use made of the term. This is, according to Mr. De Morgan, one of the cases in which the logical system of the world at large has got beyond that of the logicians.

The spicular notation of the former papers is extended: the signs ) and ] being used to distinguish mathematical and metaphysical reading. Then X))Y signifies that the whole class X is contained in the class Y; X][Y signifies that the whole attribute Y is a component of the attribute X.

The syllogism is the deduction of a relation from the combination of two others. By distinction of the mathematical and metaphysical, of the terminally ambiguous and terminally precise, four modes of combination are obtained. Logicians have but the copula is for all
cases. Mr. De Morgan proposes to use a complete system of schetical terms, by which the combination of relations shall be exhibited. Leaving out the cases of terminal precision, which are more complex and less usual, the two kinds of reading under which the common syllogism is included are as follows:—

**Terminal Ambiguity.** Mathematical reading.

Relation of Class X to Class Y.

The class x is the contrary of X, or contains all the rest of the universe. Proposition.

<table>
<thead>
<tr>
<th>Assertion of X contained in Y</th>
<th>X—of Y.</th>
<th>Y—of X.</th>
<th>Notation.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Denial of X contained in Y</td>
<td>Species</td>
<td>Genus</td>
<td>X((Y))Y</td>
</tr>
<tr>
<td>Assertion of X excluded from Y</td>
<td>Existent</td>
<td>Deficient</td>
<td>X((Z))Y</td>
</tr>
<tr>
<td>Denial of X excluded from Y</td>
<td>Coexternal</td>
<td>Coexternal</td>
<td>X((Y))Y</td>
</tr>
<tr>
<td>Assertion of x contained in Y</td>
<td>Complement</td>
<td>Complement</td>
<td>X((Z))Y</td>
</tr>
<tr>
<td>Denial of x contained in Y</td>
<td>Coinadequate</td>
<td>Coinadequate</td>
<td>X((Y))</td>
</tr>
<tr>
<td>Assertion of x excluded from Y</td>
<td>Genus</td>
<td>Species</td>
<td>X((Y))</td>
</tr>
<tr>
<td>Denial of x excluded from Y</td>
<td>Deficient</td>
<td>Existent</td>
<td>X((Y))</td>
</tr>
</tbody>
</table>

**Terminal Ambiguity.** Metaphysical reading.

Relation of attribute Y to attribute X.

| Assertion of Y a component of X | Essential | Dependent | X(\(Y\))Y |
| Denial of Y a component of X    | Non-essential | Independent | X(\(Y\)) |
| Assertion of Y incompatible with X | Repugnant | Repugnant | X(\(Y\)) |
| Denial of Y incompatible with X  | Irrepugnant | Irrepugnant | X(\(Y\)) |
| Assertion of Y a component of x | Alternative | Alternative | X(\(Y\)) |
| Denial of Y a component of x    | Inalternative | Inalternative | X(\(Y\)) |
| Assertion of Y incompatible with x | Dependent | Essential | X(\(Y\)) |
| Denial of Y incompatible with x  | Independent | Inessential | X(\(Y\)) |

The extension of the four forms to eight, the notation, &c., are treated in the second paper on syllogism. The two sets contain the same propositions, differently read; and the quantities in the two are different. In the first reading X) and (X denote X taken universally in extension; X( and )X denote X taken particularly. In the second reading ]X and X[ are universals, X] and [X are particulars. Thus, when we say that the classes X and Y are copartient, or in common language 'some Xs are Ys,' denoted by X(\(Y\)), both X and Y have particular quantity in extension. In saying this we also say that X and Y, as attributes, are irrepugnant, or not incompatible, denoted by X(\(Y\)). But the intensive force of both X and Y is universal; no one attribute of X is repugnant to any one attribute of Y.

The syllogism denoted by X()Y)(Z contains the assertions that X is a genus of Y and Y a coinadequate of Z, (Y and Z not together filling the universe). The conclusion is X(\(Y\))Z, X is a coinadequate of Z, and the combination of relations is seen in—Every species of a coinadequate is a coinadequate. In metaphysical reading, we have X()Y)(Z, X is a dependent of Y, Y an inalternative of Z. The conclusion is X(\(Z\)), X is an inalternative of Z, and the combination of relations is seen in—The dependent of an inalternative is an inalternative. When the terms become as familiar as genus and species, the axiomatic character of the combination is as clearly manifest as in—Species of species is species. Mr. De Morgan gives the following
instance of a good inference which would probably not be seen with ease in its present form, though the phrases are not technical: "We must not say that either bodily strength or meanness is a necessary alternative, for courage and meanness are incompatible, while courage does not depend on bodily strength." And he maintains that the educated world has made considerable advance in the use of relations of attributes, though the logician has nothing but what he calls the arithmetical abacus on which to exhibit the process.

Some modern logicians have so completely fallen into the mathematical view of quantity, that there is a school which treats all thought as relation of more and less. Mr. De Morgan opposes this view.

The second part of this paper, being a non-controversial summary of Mr. De Morgan's system, so far as onymatic relations are concerned, hardly admits of abstract. Its principal points have been touched on.

In a postscript, such notice is taken of the late Sir W. Hamilton's criticism on Mr. De Morgan's second paper as circumstances require and will allow.

Feb. 22.—Dr. Donaldson, of Trinity College, read a paper "On the Statue of Solon mentioned by Æschines and Demosthenes."

The object of the author of this paper was to fix the age and subject of a beautiful statue in the Museo Borbonico at Naples, which was recovered from the ruins of the theatre at Herculaneum. This statue has generally been regarded as representing Aristides the Just, the son of Lysimachus; and one attempt has been made to show that it is a portrait of Ælius Aristides, the rhetorician, who was born 38 years after the destruction of Herculaneum in A.D. 79. A more plausible hypothesis, supported by great names, considers the statue as a portrait of Æschines. But this rests on a palpable misconception. After refuting these theories, the author undertook to show that the statue was probably a copy of that erected in honour of Solon in the agora at Salamis, and mentioned in a striking manner by Æschines (c. Timarch. p. 4) and Demosthenes (De Fals. Leg. p. 420). This was argued from the peculiar and distinctive attitude; from the fact that the treatment of the drapery accorded with that belonging to the school of Scopos, and the costume corresponded to that of the epoch (about fifty years before B.C. 343) assigned to the statue of Solon by Demosthenes; and from the suitableness of a statue of Solon, who was an elegiac poet as well as a legislator, to the place where the statue was found, namely, the theatre at Herculaneum. Attention was also directed to the improbability of a later appropriation of a statue in such a peculiar posture, and Dion Chrysostomus was cited to show that even the Rhodians, who had adopted the practice of altering the inscriptions of honorary statues, abstained from interfering with those which were defined, not only by the name, but by the characteristics of the person represented.

March 8.—A paper was read by Dr. Paget, "On some Instances of remarkable Defects in the Voluntary Muscles."

Four original cases, in which large and important muscles, such as the pectorals, were wholly absent or in a state of extreme tenuity; the defects either congenital, or existing from early infancy; limited to certain groups of muscles, and unaccompanied
Intelligence and Miscellaneous Articles.

with any defect or deformity of the bones. In three of the cases, the effects symmetrical; in the fourth confined to one side of the chest. Enormous development of the calves in one of the cases.

Also a paper was read "On Organic Polarity," by H. F. Baxter, Esq. The object of the paper is to show the intimate connexion that exists between organic force and the ordinary polar force, such as chemical force, for example.

The principal experiments, showing that organic action, viz. secretion, is accompanied with the manifestation of current force, have already appeared in the Royal Society's 'Transactions' for the years 1848 and 1852; but in the present communication the author enters more minutely into the resemblance between the actions which take place in the voltaic circle and those that occur during secretion than could be prudently attempted in his previous papers. But whatever view may be entertained in regard to the origin of the power in the voltaic circle, whether by mere contact or by chemical action, the decision of this point is of no importance to the question under consideration; since the manifestation of current force during voltaic action is allowed both by the chemical theorist as well as the contact theorist; and if we admit the manifestation of this force (current force) to be evidence of polar action in one class of cases, viz. during voltaic action, we are certainly justified in logically concluding that it may be adduced as evidence of polar action in other cases also, viz. during organic action as in secretion. Reference is made to Prof. Graham's researches on osmose. According to Prof. Graham, osmose would appear to be dependent upon chemical action, and consequently, should we be disposed to class the phenomena of secretion with those of osmose, we should be thus compelled to acknowledge that the act of secretion must be polar in its nature.

The author does not attempt to show in what manner secretion is effected, his great object being to point out what does occur during this act, viz. the manifestation of polar action, and consequently it is upon this ground that we may logically infer that the force must be polar in its nature.

XXXIX. Intelligence and Miscellaneous Articles.

ON THE DOUBLE TANGENTS TO PLANE CURVES.

BY THE REV. GEORGE SALMON, TRINITY COLLEGE, DUBLIN.

ONE of the most difficult problems in the theory of plane curves is that of finding the points of contact of double tangents to a curve of the $n$th degree. A new solution of that problem may therefore be acceptable.

The tangent to a plane curve meets the curve in $n - 2$ points beside the point of contact. If now it were possible to form the equation of a curve of the $(n - 2)$nd degree passing through those $n - 2$ points, then by forming the condition that the tangent to the given curve should touch this curve of the $(n - 2)$nd degree, we should immediately obtain an equation to be satisfied by the points of contact of double tangents. Now I have succeeded in forming the equation of such a curve of the $(n - 2)$nd degree, as follows.
Consider the curve itself; its first polar or emanant, viz.

\[ (X \frac{d}{dx} + Y \frac{d}{dy} + Z \frac{d}{dz}) U, \]

its second, third polars, and so on. Form the Hessians of each of these curves, \(H, H_1, H_2, \&c\), which will contain \(XYZ\) in the degrees 0, 3, 6, \&c. I suppose them to have been so multiplied by numerical factors that the substitution of \(x, y, z\) for \(X, Y, Z\) shall make all identical. Form now the emanants of the \((n - 2)\)nd degree, or the polars of that degree of \(H, H_1, H_2, \&c\) with respect to \(XYZ\); these will all be of the same degree in \(XYZ\), viz. \(2(n - 2)\), and by a syzygetic combination of these can be formed the curve of the \((n - 2)\)nd degree required. For instance, for a curve of the fourth degree it is

\[ D^3H - 3D^3H_1; \]

for a curve of the fifth degree,

\[ D^3H - 4D^3H_1 + 6D^3H_2, \]

and so on.

September 10, 1858.

ON THE ELECTRO-MAGNETIC ROTATION OF LIQUIDS.

BY A. BERTIN.

Experiments upon electro-magnetic rotations are usually made by employing, as moveable conductors, copper wires turning round a central pivot. By this method, however, none but slightly varied phenomena can be observed; and in this manner the electro-dynamic theories can only be submitted to a small number of verifications. It is otherwise when a liquid is taken as the moveable conductor. The experiment is also very easy: it is sufficient to cause a current to pass into a liquid, taking as electrodes two concentric circles of copper, and to submit it at the same time to the action of a magnet, an electro-magnet, or an electro-dynamic coil. The liquid is immediately seen to turn rapidly round the common centre of the two electrodes, even when the pile employed consists only of a small number of elements. On changing its place, the rapidity and the direction of the rotation are varied, according to the position given to the moveable current. These changes are subject to the following laws:

1. The centrifugal and centripetal currents turn in opposite directions, other things remaining the same.

2. There is a series of points in the space where the rotation is null; their totality constitutes a neutral surface, on each side of which the rotation changes its direction.

3. For a vertical magnet, the neutral surface has for generator a curve with two branches passing through the poles, and resembling a hyperbola mounted upon the axis of the magnet. Between the two branches of this neutral line the rotation of the centrifugal current is positive, or in the same direction as the circulation of the current which would produce magnetization; outside the neutral line this rotation is in the opposite direction, or negative.

Theory explains these phenomena in their smallest details. The action of the magnet upon the horizontal current which traverses the liquid in the nearest part is a horizontal force perpendicular to the moveable current, and has for its expression.
Intelligence and Miscellaneous Articles.

\[ F = m \text{d} s \left( \frac{\cos \theta}{r^2} - \frac{\cos \theta'}{r'^2} \right); \]

\( m \) being the intensity of the magnetism;
\( i \) that of the current;
\( ds \) the element of the moveable current nearest to the magnet;
\( r \) and \( r' \) the radii vectores drawn from this element to the southern and northern poles;
\( 0 \) and \( \theta' \) the angles of these radii with the axis of the magnet on the side of the south pole.

Taking \( ds \) with the sign + for the centripetal current, and with the sign — for the centrifugal current, the sign of the force is in all cases the sign of the rotation. This rotation becomes null if

\[ \frac{\cos \theta}{r^2} = \frac{\cos \theta'}{r'^2}. \]

The neutral line constructed by starting from this equation does not differ notably from that traced experimentally.

4. The same law is applicable to electro-magnets, with this difference, that the neutral line starts from the margins of the electro-magnet, as though the poles were at the extremities.

5. Electro-dynamic coils behave like weak electro-magnets, with this peculiarity, that their action is in the opposite direction internally and externally.

6. From this it results that a hollow coil and a hollow magnet do not produce the same effect except upon currents which are exterior to them; their actions upon the interior currents are of opposite signs. To explain this difference, the magnets must be assimilated, not to coils, but to bundles of solenoids.

7. The original experiment of Davy upon the rotation of mercury, the only one of this kind cited in our treatises on physics, may be easily repeated when the mercury is replaced by an aqueous solution. It is explained by the preceding laws.

8. When, in experiments upon magnets, the vertical magnet is replaced by a horizontal one, we observe other phænonena, of which the laws are furnished by the following theoretical formula,

\[ F = m \text{d} s \cdot y \left( \frac{1}{r^3} - \frac{1}{r'^3} \right), \]

\( y \) being the vertical distance from the magnet to the level of the liquid; regarding it as positive when the liquid is above the magnet, and retaining the preceding values, the sign + of the force indicates a rotation from left to right, and the sign — an opposite rotation. The formula is always in agreement with experiment: both show two neutral planes, the one corresponding with \( y = 0 \), and the other with \( r = r' \).

9. We may also submit to magnetic action currents which would move vertically in a liquid. We then find a new series of phenomena, which may be easily referred to the two preceding formulæ.

To sum up, the electro-magnetic rotations of liquids are phenomena which it is easy to produce, curious to observe, and interesting to study in a theoretical point of view; they consequently deserve a place amongst the classical experiments in electro-magnetism. — Comptes Rendus, August 16, 1858, p. 307.
XXXV. On the Diffraction of Light.
By John Bridge, M.A.*

In the Philosophical Magazine for October 1855, I suggested the application of photography to the illustration of the diffraction of light. The attempts which I had then made were sufficiently successful to warrant me in publishing that notice. But renewing the experiments this summer, I have been able to produce a very splendid series of phænomena, which illustrate the theory in every point. First, we draw on a large sheet of paper a series of figures arranged at equal distances in a circle. A collodion picture of these being taken, we have a series of small transparent apertures in the elsewhere opaque film. This is then mounted so that each may be in turn brought before the centre of a telescope, which is adjusted to view an image of the sun. In this manner we have an apparatus of the most compact form, by which a series of fifty or more different phænomena may be viewed in a few minutes. Also the figures being very small (occupying on an average an area one-tenth of an inch in diameter), the inaccuracies of surface and substance of the glass may be neglected†. There is another advantage in the figures being small; for the size of the image is in inverse proportion to the size of the aperture. For this reason the apertures have been made as small as was consistent with sufficient brightness.

I will now give a sketch of the several points which I have sought to illustrate. In doing so I will endeavour to simplify the mathematical treatment of such questions, and to explain some new and curious phænomena.

* Communicated by the Author.
† The film of Canada balsam with which a glass is cemented over the picture, of course, produces no disturbance.
Mr. J. Bridge on the Diffraction of Light.

(1) The problem may be stated as follows:—A series of waves, plane or spherical, meet in their course an aperture of given form; it is required to find the actual or virtual intensity of light at any given point.

The answer according to the undulatory theory is,—let a sphere be described having the given point as centre; then the displacement (actual or virtual) at the given point is proportional to the geometrical sum of the displacement simultaneously existing on this sphere, and the intensity as the square of this displacement. For example, if the given point be on the side towards which the light proceeds after leaving the aperture, the displacements simultaneously existing on the sphere referred to will all, when equally modified by distance, reach the given point together.

I shall take as the normal case a series of parallel waves falling on the aperture, and viewed either by the naked eye, or by a telescope in focus. It will then be necessary to sum the displacements simultaneously existing on a plane making a given angle \( \alpha \) with the aperture. This will determine the intensity of light proceeding in the direction of the normal to the plane, and brought to a focus at a distance \( f\alpha \) from the principal focus.

(2) The plane, which is perpendicular to the plane of the aperture and to that in which we have to sum the displacements, meets the aperture in a line which we may call the axis of \( x \); also let \( y \) be the breadth of the aperture perpendicular to this at the distance \( x \). Then the ether in the plane of the diaphragm being all supposed in the same state of displacement,

\[
\int y \sin \frac{2\pi}{\lambda} (vt - \alpha x) dx
\]

is the sum of the displacements which will at last coincide in the image plane at the distance \( f\alpha \) from the focus.

The intensity of the light at this point is \( A^2 + B^2 \), where

\[
A = \int y \cos \frac{2\pi \alpha x}{\lambda} dx, \quad B = -\int y \sin \frac{2\pi \alpha x}{\lambda} dx.
\]

(3) If there be two apertures, one of which may be converted into the other by moving every \( y \) in the direction of its length, or multiplying them all by the same ratio, they will give the same diffraction image in the direction (reckoning from the principal focus) of the axis of \( x \).

For example, two triangles having the same altitude, and either equal or unequal bases, give the same changes of intensity in the focal line perpendicular to the base; for the displacement in a given plane arising from any point of the aperture is not altered by moving that point parallel to the plane; and in the second
case the positions of maximum and minimum displacements will be unaltered.

(4) If any focal line (line through focus) be drawn, the intensities in it at equal distances from the focus are equal. This is seen immediately in the formulae, for a change of sign of \( \alpha \) does not affect \( A^2 + B^2 \). Or thus: the displacement at any time \( t \) in the one case is equal to the displacement at the time \( \frac{\lambda}{2\nu} - t \) in the other case, so that the maximum displacements being the same, the intensities are also the same in both cases.

(5) Similar apertures give similar figures of diffraction with dimensions inversely as their own.

For if \( x \) and \( y \) are multiplied, and at the same time \( \alpha \) divided by a constant, \( A \) and \( B \) will also be multiplied by the same constant.

Otherwise; in a small right-angled triangle one side is constant if the hypothenuse and opposite angle vary inversely. From this it appears that the displacements in two given planes making angles with the apertures inversely as their dimensions are the same.

(6) If the apertures have the values of \( y \) equally distributed on opposite sides of the axis of \( x \), \( B = 0 \), and \( A \) is double what it would be on one side.

(7) If the aperture be moved to any parallel position, so that every \( x \) is increased by \( \xi \), \( A \) and \( B \) become

\[
A \cos \frac{2\pi \alpha \xi}{\lambda} + B \sin \frac{2\pi \alpha \xi}{\lambda},
\]

and

\[
B \cos \frac{2\pi \alpha \xi}{\lambda} - A \sin \frac{2\pi \alpha \xi}{\lambda}.
\]

(8) The figures formed by a series of equal similarly situated apertures may be found from that produced by one aperture and that produced by a series of points having the same arrangement as the apertures. The intensity at any point will be the product of the intensities from the two figures.

For the disturbance arising from each point of one aperture, combined with that from corresponding points of the others, becomes multiplied in a certain proportion depending on the arrangement; therefore the disturbance from one aperture becomes multiplied by the ratio of that from a series of points to that from one point; so that the intensity is the product of the intensities in the two cases. Darkness will therefore exist from the series of apertures wherever it would exist from one aperture and from the series of points.
Mr. J. Bridge on the Diffraction of Light.

The same thing may be seen by the formulæ. For, by (7), instead of A and B must be put

\[ A(1 + c_1 + c_2 + \ldots) + B(s_1 + s_2 + \ldots) \]
\[ B(1 + c_1 + c_2 + \ldots) - A(s_1 + s_2 + \ldots) \]

\( c_1, s_1, c_2, s_2, \&c. \) being the cosines and sines of certain angles. And in the case of the points for A and B we have

\[ 1 + c_1 + c_2 + \ldots \]
\[ s_1 + s_2 + \ldots \]

The sum of the squares of the first pair is equal to those of the second pair multiplied by \( A^2 + B^2. \)

(9) For determining the expressions for intensity arising from any apertures bounded by straight lines, we only need integration in the cases of a triangle and of a parallelogram whose bases are in the axis of \( y. \) The remainder of the process is supplied by means of the preceding observations, which also afford a simple means of determining the figures produced by apertures bounded by conic sections.

For a parallelogram of height \( h, \) base \( b, \) the base being in the axis of \( y, \) the law of variation in the direction perpendicular to the base is obtained from

\[ A = \frac{b\lambda}{2\pi\alpha} \sin \frac{2\pi ah}{\lambda}; \quad B = \frac{-b\lambda}{2\pi\alpha} \left(1 - \cos \frac{2\pi ah}{\lambda}\right), \]

Intensity \( = \frac{b^2\lambda^2}{4\pi^2\alpha^2} \sin^2 \frac{\pi ah}{\lambda}. \)

For a triangle of height \( h, \) base \( b, \) the base being in the axis of \( y, \)

\[ A = \frac{\lambda^2b}{4\pi^2\alpha^2 h} \left(1 - \cos \frac{2\pi ah}{\lambda}\right), \]
\[ B = \frac{\lambda^2b}{4\pi^2\alpha^2 h} \sin \frac{2\pi ah}{\lambda} - \frac{\lambda b}{2\pi\alpha}, \]

Intensity \( = \frac{\lambda^2b^2}{4\pi^2\alpha^2 h} \left(1 - \frac{\lambda}{\pi ah} \sin \frac{2\pi ah}{\lambda} + \frac{\lambda^2}{\pi^2\alpha^2 h^2} \sin^2 \frac{\pi ah}{\lambda}\right). \)

(10) As an application of these results, I will take the case of an equilateral triangle. We may divide it into two triangles having their bases in the axis of \( y; \) bases, \( \frac{a\sqrt{3}}{2 \cos \theta}; \) heights, \( a\sin(30 + \theta) \) and \( a \sin (30 - \theta). \) Whence
Mr. J. Bridge on the Diffraction of Light.

\[ A = \frac{\lambda^2 \sqrt{3}}{8 \pi^2 a^2 \cos \theta} \left( \frac{2 \sin^2 \pi a a \sin (30 + \theta)}{\lambda} + \frac{2 \sin^2 \pi a a \sin (30 - \theta)}{\lambda} \right), \]

\[ B = \frac{\lambda^2 \sqrt{3}}{8 \pi^2 a^2 \cos \theta} \left( \frac{2 \pi a a \sin (30 + \theta)}{\lambda} - \frac{2 \pi a a \sin (30 - \theta)}{\lambda} \right). \]

If we take in the plane of the image supposed formed by a lens of focal length \( f \), axes perpendicular to two sides of the triangle, we must put

\[ \alpha \cos \theta = \frac{(x+y) \sqrt{3}}{2f}, \quad \alpha \sin (30 + \theta) = \frac{x \sqrt{3}}{2f}, \quad \alpha \sin (30 - \theta) = \frac{y \sqrt{3}}{2f}, \]

and then

\[ A = \frac{\lambda^2 f^2}{6 \pi^2 a(x+y)} \left( \frac{1 - \cos \frac{\pi a \sqrt{3}}{\lambda f} x}{x} + \frac{1 - \cos \frac{\pi a \sqrt{3}}{\lambda f} y}{y} \right), \]

\[ B = \frac{\lambda^2 f^2}{6 \pi^2 a(x+y)} \left( \frac{\sin \frac{\pi a \sqrt{3}}{\lambda f} x}{x} - \frac{\sin \frac{\pi a \sqrt{3}}{\lambda f} y}{y} \right), \]

\[ A^2 + B^2 = \frac{\lambda^4 f^4}{9 \pi^4 a^2(x+y)^2} \left( \frac{\sin^2 \frac{\pi a \sqrt{3}}{2 \lambda f} x}{x^2} + \frac{\sin^2 \frac{\pi a \sqrt{3}}{2 \lambda f} y}{y^2} \right) \]

\[ \left( \frac{\pi a \sqrt{3}}{2 \lambda f} x \right) \cdot \sin \left( \frac{\pi a \sqrt{3}}{2 \lambda f} y \right) \times \cos \frac{\pi a \sqrt{3}}{2 \lambda f} (x+y). \]

For a square in any direction. Taking origin at centre, \( B = 0 \) by (6), and \( \Lambda \) is twice the difference of the results from two triangles whose heights are \( \alpha (\cos \theta + \sin \theta), \gamma (\cos \theta - \sin \theta) \); and bases

\[ \frac{ah}{\cos \theta \sin \theta}, \frac{al}{\cos \theta \sin \theta}. \]

Therefore

\[ \Lambda = \frac{\lambda^2}{\pi^2 a^2 \cos \theta \sin \theta} \cdot \left( \cos \frac{2 \pi ah}{\lambda} - \cos \frac{2 \pi ah'}{\lambda} \right) = \frac{\lambda^2}{\pi^2 a^2} \cdot \frac{2 \pi a a \cos \theta \sin \lambda}{\sin \cos \theta \sin \lambda} ; \]

or if \( x, y \) be coordinates in the plane of the image,

\[ \Lambda = \frac{\lambda^2 f^2}{\pi^2} \cdot \frac{2 \pi a x \sin \frac{\lambda f}{x}}{x \cdot \sin \frac{2 \pi a y \sin \frac{\lambda f}{y}}{y}}. \]
(11) The result of summation for two points at distance $a$, axis of $x$ being taken parallel to the line joining the two, is that the intensity is $4 \cos^2 \left( \frac{\pi ax}{\lambda f} \right)$. For four points in the corners of a square it is $16 \cos^2 \left( \frac{\pi ax}{\lambda f} \right) \cos^2 \left( \frac{\pi ay}{\lambda f} \right)$. For three equidistant points, oblique axes being taken perpendicular to the sides of the triangle, it is

$$3 + 2 \cos \frac{\pi a \sqrt{3}}{\lambda f} (x + y) + 2 \cos \frac{\pi a \sqrt{3}}{\lambda f} x + 2 \cos \frac{\pi a \sqrt{3}}{\lambda f} y,$$

or

$$1 + 8 \cos \frac{\pi a \sqrt{3}}{2 \lambda f} (x + y) \times \cos \frac{\pi a \sqrt{3}}{\lambda f} x \times \cos \frac{\pi a \sqrt{3}}{\lambda f} y.$$

The interpretation of these formulæ is very simple, and will be found to agree with the phænomena, at least in their most prominent characters.

(12) The effect of a numerous series of points in regular order may be best obtained from the consideration, that in any direction in which the retardations of the light differ for different points by whole wave lengths, the effects of all will coincide, and there will be a maximum.

Take a series of points in equidistant parallel order, the rows being $a a' a'' a'''$, $b b' b'' b'''$, &c., or across $a b c d$, $a' b' c' d'$, &c. In directions perpendicular to $a a'$, $a b'$, $a c'$, $a d'$, &c., by (5) and (3), there will be maxima at distances inversely as the distances between the lines $a a'$, $b b'$; $a b'$, $b c'$; $a c'$, $b d'$, &c. Now the product of the distance of two consecutive points in any of these rows by the perpendicular distance between it and the next row is always the same, being the area of the smallest parallelogram that can be formed by the points. From this it follows that the arrangement of the maxima is similar to that of the apertures turned through a right angle.

(13) An aperture of the form of an ellipse gives an image formed of a series of ellipses similar to itself turned through a right angle.

To explain this effect of an elliptic aperture, suppose all chords perpendicular to a given line to be moved in the direction of their length in such a manner that their middle points may lie in the given line; let them then be all altered in such a proportion that the new ellipse may become a circle, whose radius is the perpendicular from centre on a tangent parallel to the chords. These changes, by (3), do not affect the alternations of light and darkness in the focal line perpendicular to the chords. Now in comparing the effects of two circular apertures by (5), corre-
sponding points lie at distances inversely as the radii of the circles. Therefore corresponding points in different directions in the case under consideration will lie at distances which are inversely as the perpendiculars (from centre on tangent), and therefore proportional to the normals, which have those directions; but using the ordinary notation in conic sections,

\[
\frac{GN^2}{b^2} + \frac{NP^2}{a^2} = \frac{d^2}{a^2}.
\]

The curves are therefore concentric ellipses similar to the aperture, turned through a right angle.

The preceding paragraph applies to an aperture of the form of the difference between two similar concentric ellipses, comparison being made by means of circular annuli.

To find the effect of an aperture of the form of the difference between two similar concentric hyperbolas, we may proceed as in the case of the ellipse, except that instead of a circle, or circular annulus, we must use a pair of equilateral hyperbolas whose axis is in the given direction. The result will then be that the figure obtained is a series of hyperbolic bands, having asymptotes at right angles to those of the aperture, but occupying the supplemental angle, in fact similar to the conjugate hyperbola, turned through a right angle.

The image produced by an aperture of the form of the difference between two equal parabolas, or several such bands, on the same axis is a series of parallel coloured straight bands. As before, let every chord perpendicular to a given line be moved in the direction of its length, so that their middle points may all lie in the given line; then let them all be altered so as to make parabolas of given form. The resulting parabolic strips will then be similar, and the distances of corresponding points in the figure of diffraction in different directions will be inversely as the distance between the vertices, that is, inversely as the perpendicular distances in each direction between parallel tangents in the original parabolas; but these are proportional to the normals; and as the subnormal is constant, corresponding points will lie in a straight line perpendicular to the axis of the parabolas. The figure is therefore a series of parallel bands.

(14) Taking the results for single apertures, those for any arrangement of such apertures may be found by means of (8).

Thus a pair of circular annuli give concentric rings crossed by dark bands perpendicular to the line through the centres of the apertures.

Three circular annuli give concentric rings containing maxima of brightness and points of darkness, each arranged in triangular order. When the annuli are more numerous and regularly
arranged with their centres in a circle, the figures are extremely beautiful. A pair of elliptic or hyperbolic bands give concentric, elliptic, or hyperbolic bands of similar or conjugate form crossed by dark bands.

A series of equilateral triangles gives a six-rayed star, each ray consisting of a multitude of coloured spectra separated by dark bands corresponding to the arrangement. In the case of the arrangement, fig. 167 of Sir John Herschel's treatise, I did not obtain the result in fig. 168, but that which I have described above.

These are a few of the appearances which I have produced, and which may be multiplied to any extent. Beside the appearances when the telescope is in focus, which alone have been referred to above, the appearances out of focus are often very remarkable, and in many cases might suggest to the artist designs which, both by the symmetry of their forms and by the beautiful arrangement of colours, would perhaps surpass any produced by unaided ingenuity.

In conclusion, I may mention that Messrs. Elliott, 30 Strand, will supply the apparatus necessary for exhibiting these phænomena.

59 Stanhope Street, Hampstead Road, October 7, 1858.

XXXVI. On a Photochemical Method of recognizing the Non-volatile Alkalies and Alkaline Earths. By R. Cartmell, Esq.*

As has long been known, the non-volatile alkalies, when put into the flame of a burning body, have the property of communicating to it peculiar colours; and in the case of soda and lithia, these colours are of considerable intensity. As a mere trace of soda, however, is sufficient entirely to obscure the colours communicated to the flame by potash or lithia, this as a means of distinguishing between them has hitherto been of very limited application. The following way of detecting them in the presence of each other depends on a means of observing by coloured media the characteristic rays of light which each emits when heated strongly in a colourless gas flame. On account of the homogeneous nature of the colour given to the flame by soda, I thought it possible to eliminate the soda reaction by the use of a coloured medium. After a great many experiments with various coloured solutions and other coloured media, I found that a bluish-coloured solution would best accomplish this object. The only solution that I have yet found to answer perfectly for obliterating the colour of soda, and allowing other rays, namely red ones from lithia and potash, to pass freely through it, is solution of indigo. This solution possesses these advantages,—

* Communicated by the Author.
recognizing the Non-volatile Alkalies and Alkaline Earths. 329

that it is met with in most laboratories as a reagent, is readily diluted to the required intensity, and when so prepared will keep good for some time.

When the absence of potash has been ascertained by a method which will be presently described, the above solution gives one a means of rendering visible the red rays from lithia in the presence of soda, when the proportions do not exceed one part of the former to a thousand parts of the latter. When potash is viewed through this indigo solution red rays are likewise observed, and these can be seen till the proportion of potash to soda is less than one part to two hundred. To distinguish potash in the presence of soda or lithia, it was necessary to find a medium through which no coloured rays from soda or lithia would pass, but which would admit those peculiar to potash. This was found to be a deep blue cobalt glass which I had cut up for the purpose. Potash viewed through this glass gives to the flame an intense violet colour; and this is still visible when one part of potash is present with two hundred parts of soda. I have not yet succeeded in getting a medium that will permit rays from lithia to pass, and at the same time keep back all those from soda or potash. But the presence of lithia can be ascertained in that of potash, or of potash and soda, in the following way:—Place side by side in the flame two wires, one having on its point a little pure sulphate of potash, and the other the sample to be tested. If there be lithia in the sample to the extent of one part to two hundred parts of potash, a very marked difference will be seen between the colour of the two flames, that containing lithia being of a much brighter red. When soda, potash, and lithia occur together, one part of each can be seen in the presence of two hundred parts of the mixture. As soda in the minutest quantity is visible to the naked eye, we have now a perfect means of separating the three alkalies from one another.

The following Table will show how the separation is best effected:—

Mode of separating the Alkalies.

<table>
<thead>
<tr>
<th>With the naked eye.</th>
<th>Cobalt glass.</th>
<th>Indigo solution.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Yellow:</td>
<td>Violet-red:</td>
<td>Comparative test with pure sulphate of potash.</td>
</tr>
<tr>
<td>Soda.</td>
<td>Potash:</td>
<td>Brighter red than sulphate of potash alone:</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Lithia.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>When the absence of potash has been previously proved by the cobalt glass, red rays visible through the solution prove the presence of lithia.</td>
</tr>
</tbody>
</table>
The indigo solution is conveniently made by adding water to
the common indigotic acid of the laboratories till the point is all
but reached where an intense flame of soda appears slightly
pink when observed through the bottle (of the size and shape of
a common watch) filled with the solution.

The blue glass hitherto used is very thick and of considerable
depth of colour; but on repeating these experiments, I have
found that the blue glass as usually obtained from the manufac-
tory, when increased to the same depth of colour by putting two
pieces together, answers the purpose perfectly. Before using
these glasses, it must first of course be ascertained how small a
quantity of potash mixed with soda or lithia they will clearly
indicate.

For the success of these experiments the importance of em-
ploying a perfectly colourless flame is obvious. I have used the
flame of a Bunsen's lamp, which is of the palest blue except
just at the tip, and devoid of any trace of red. The most intense
flame of soda should only make this blue a little brighter when
viewed through the indigo solution.

The substances to be exposed to the flame are conveniently
supported on the finest platinum wire
melted on to a glass stand, and the
wire is to be somewhat curved, as in
the subjoined diagram. The curved
part is first heated, and so introduced
into the flame that the point of the wire
where the substance rests is just within
the edge of it. When the wire is some-
what curved and heated as above di-
rected, it hinders the escape of the sub-
stance from its proper position.

The alkalies are best tested for in the state of sulphates, as
these salts do not volatilize so rapidly as the chlorides, and hence
their colours are more permanent. To increase the intensity of
these reactions, it is only necessary, after having put the sub-
stance contained on the point of the wire into the reducing flame,
to moisten it with hydrochloric acid and to hold it again in the
edge of the flame.

In making these observations, care should be taken not to
mistake the glowing platinum wire for a coloration of the flame.
The coloured rays passing immediately upwards from it are only
to be regarded.

Separation of the Alkaline Earths.

Hitherto, in the detection of strontia by means of the colour
communicated by its chloride to the flame of alcohol, there has
ever been a difficulty in distinguishing it when small quantities are present. Here I may add that this method is particularly adapted to the discovery of small quantities of strontia; and also, when the substances of this group occur in so small quantities that they are no longer recognizable in the wet way: by the reactions of the flame and coloured medium they are readily distinguishable.

The characteristic colours which baryta, strontia, and lime communicate to the flame, are best observed when their sulphates are exposed on the point of a piece of the finest platinum wire to the outer part of an air-flame. And further during the burning, the colours they produce can be increased in intensity by first putting them into the reducing flame, and then either moistening them with hydrochloric acid, or by holding some asbestos moistened with this acid in the flame directly under the point of the platinum wire on which the substances are supported.

In commencing the experiments with this group, in order to ascertain how far these characteristic colours would serve to distinguish the members of it, I observed that in all proportions, as far as they are capable of being recognized in the presence of each other, they always burn away in the following order:—First is seen the green of baryta, next the bright red of strontia, and lastly the dull and scarcely visible red of lime. When, however, a large quantity of strontia or lime is present in proportion to the baryta, the red colours of the two former are observed at the same time; and in this case the green of baryta can only be seen distinctly by passing the wire on which the substance is supported in and out of the flame during the observation. On continuing these experiments, I found also that when much lime was present it modified considerably the colours of baryta and strontia; so much so, that these two last, when in small proportions, could not be distinguished. I tried further the effect of looking through coloured media, as in the case of the alkalies, but could find none that were altogether satisfactory. By a solution of indigo more dilute than that used for the alkalies, strontia can be distinguished from lime, as through it lime appears olive-green, and strontia intense red, when they are first introduced into the flame after being moistened with HCl. As it is easy to separate the baryta and almost all the strontia from the lime by means of dilute sulphuric acid, and as when only baryta and strontia occur together they are easily distinguished by the naked eye, when one part of baryta occurs with one hundred parts of strontia, and vice versa, I found it best to separate baryta and strontia together from the lime.

The following method answers perfectly for their separation. The carbonates as usually obtained are dissolved in hydrochloric
acid, and to the solution diluted with water, dilute sulphuric acid is added. The sulphates of baryta and strontia produced are to be separated from the lime by filtration through as small a filter as possible, and which has been previously washed out with dilute nitric acid. When the precipitate is very small in quantity, it is necessary to burn the filter, and then to test the residue on platinum wire in the usual way. When strontia occurs in very small quantity, it will not be precipitated; but on adding ammonia and carbonate of ammonia, will go down with the lime, from which it can be separated and tested according to the manner described in the Table.

Separation of the Alkaline Earths.

The carbonates as usually obtained are dissolved in hydrochloric acid, and to this solution diluted with water, dilute sulphuric acid is added.

<table>
<thead>
<tr>
<th>Precipitate.</th>
<th>Filtrate.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sulphate of baryta and sulphate of strontia.</td>
<td>Sulphate of strontia and sulphate of lime.</td>
</tr>
<tr>
<td>Test on platinum wire in the air-flame: green (baryta), red (strontia).</td>
<td>Add ammonia and carbonate of ammonia.</td>
</tr>
<tr>
<td>To the original solution add sulphate of strontia.</td>
<td>Precipitate.</td>
</tr>
<tr>
<td>A precipitate indicates the presence of BARYTA.</td>
<td>Carbonate of strontia and carbonate of lime.</td>
</tr>
<tr>
<td>Residue.</td>
<td>Collect on a filter and dissolve in a little nitric acid, evaporate to dryness on a water-bath, and treat with a little strong alcohol.</td>
</tr>
<tr>
<td>Nitrate of strontia.</td>
<td>Solution.</td>
</tr>
<tr>
<td>Convert into sulphate and test on platinum wire in the flame, after having reduced and subsequently moistened with HCl.</td>
<td>Nitrate of lime.</td>
</tr>
<tr>
<td>Through dilute solution of indigo, Carmine-red: STRONTIA.</td>
<td>Add oxalate of ammonia.</td>
</tr>
<tr>
<td>Precipitate.</td>
<td>Oxalate of lime not soluble in acetic acid.</td>
</tr>
<tr>
<td>Convert into sulphate and test on platinum wire in the flame, after having reduced and subsequently moistened with HCl.</td>
<td>Through dilute solution of indigo, Olive-green: LIME.</td>
</tr>
</tbody>
</table>

The dilute solution of indigo, to distinguish between strontia and lime, is made by diluting the common indigotic acid till it gives with sulphate of lime an olive-green colour, on testing in the manner directed. Care must be taken not to make this solution too dilute, otherwise sulphate of lime appears slightly red through it. It is to be used in the same kind of vessel as the indigo solution for the alkalies.

In testing for this group, it is necessary to allow the soda,
which is always present, to burn off first, then the colours of the other substances become distinctly visible.

For this mode of distinguishing the alkalies and alkaline earths, very small quantities of substance are necessary; and the analyses can be made with somewhat better results when daylight is excluded.

I am indebted to Professor Kirchhoff for determinations of the rays which come through the solutions of indigo and the blue glass that I have used in my experiments. The dilute solution of indigo allows all the rays of the spectrum to pass except the dull red ones bordering on the orange. The solution of indigo used to distinguish the alkalies, allows those rays to pass extending from A to B and from E to G; and the blue glass those extending from A to a, and from F to H of Fraunhofer's lines of the spectrum.

A little reflection will show that the method described in this paper depends on the following principle. If there be any difference in colour between two flames, the medium which will give white light with one of the flames will give coloured light with the other. Two substances, therefore, which communicate colours to the flame, unless these colours be exactly alike, can be distinguished by a medium producing white light with only one of them.

By following out the investigation I hope to be able to determine potash quantitatively in the presence of soda, and to effect separations between some of the heavy metals.

I am indebted to Professor Bunsen, in whose laboratory this investigation was carried on, for many valuable suggestions.

Heidelberg, September 1858.

XXXVII. On some Physical Properties of Ice. By John Tyndall, F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain*. 

The observations upon Glaciers, to be recorded in a subsequent paper, led me to make some experiments upon the physical properties of ice, the results of which are, I hope, of sufficient interest to justify me in presenting them to the Royal Society.

§ I.

1. I availed myself of the fine sunny weather with which we were favoured last September and October, to examine the effects

* From the Philosophical Transactions, Part I. for 1858; having been read at the Royal Society December 17, 1857.
of solar heat upon ice. The experiments were made with the Wenham Lake and Norway ice. Slabs were formed of the substance, varying from one to several inches in thickness, and these were placed in the path of a beam rendered convergent by a double convex lens 4 inches in diameter, and possessing a focal distance of 10½ inches. The slabs were usually so placed, that the focus of parallel rays fell within the ice. Having first found the position of the focus in air, the lens was screened; the ice was then placed in position, the screen was removed, and the effect was watched through an ordinary pocket lens.

2. A plate of ice an inch thick, with parallel sides, was first examined: on removing the screen the transparent mass was crossed by the sunbeams, and the path of the rays through it was instantly studded by a great number of little luminous spots, produced at the moment and resembling shining air-bubbles. When the beam was sent through the edge of the plate, so that it traversed a considerable thickness of the ice, the path of the beam could be traced by those brilliant spots, as it is by the floating motes in a dark room.

3. In lake ice the planes of freezing are easily recognized by the stratified appearance which the distribution of the air-bubbles gives to the mass. A cube was cut from a perfectly transparent portion of the ice, and the solar beam was sent through the cube in three rectangular directions successively. One was perpendicular to the plane of freezing, and the other two parallel to it. The bright bubbles were formed in the ice in all three cases.

4. When the surfaces perpendicular to the planes of freezing were examined by a lens, after exposure to the light, they were found to be cut up by innumerable small parallel fissures, with here and there minute spurs shooting from them, which gave the fissures, in some cases, a feathery appearance. When the portions of the ice traversed by the beam were examined parallel to the surface of freezing, a very beautiful appearance revealed itself. Allowing the light from a window to fall upon the ice at a suitable incidence, the interior of the mass was found filled with little flower-shaped figures. Each flower had six petals; and at its centre was a bright spot, which shone with more than metallic brilliancy. The petals were manifestly composed of water, and were consequently dim, their visibility depending on the small difference of refrangibility between ice at 32° F. and water at the same temperature.

5. For a long time I found the relation between the planes of these flowers and the planes of freezing perfectly constant. They were always parallel to each other. The development of the flowers was independent of the direction in which the beam traversed the ice. Hence, when an irregular mass of transparent
ice was presented to me, by sending a sunbeam through it, I could tell in an instant the direction in which it had been frozen.

Allowing the beam to enter the edge of a plate of ice, and causing the latter to move at right angles to the beam, so that the radiant heat traversed different portions of the ice in succession, when the track of the beam was observed through an eye-glass, the ice, which a moment before was optically continuous, was instantly starred by those lustrous little bubbles, and around each of them the formation and growth of its associated flower could be distinctly observed.

6. The maximum effect was confined to a space of about an inch from the place at which the beam first struck the ice. In this space the absorption, which resolved the ice into liquid flowers, for the most part took place; but I have traced the effect to a depth of several inches in large blocks of ice.

7. At a distance, however, from the point of incidence, the spaces between the flowers became greater; and it was no uncommon thing to see flowers developed in planes a quarter of an inch apart, while no change whatever was observed in the ice between these planes.

8. The pieces of ice experimented on appeared to be quite homogeneous, and their transparency was very perfect. Why then did the substance yield at particular points? Were they really weak points of crystalline structure? or did the yielding depend upon the manner in which the calorific wave impinged upon the molecules of the body at these points? However these and other questions may be answered, the experiments have an important bearing upon the question of absorption. In ice the absorption of the rays which produce the flowers is fitful, and not continuous; and there is no reason to suppose that in other solids the case is not the same, though their constitution may not be such as to reveal it*.

I have applied the term "bubbles" to the little bright discs in the middle of the flowers, simply because they resembled the little air-globules entrapped in the ice; but whether they contained air or not could only be decided by experiment.

9. Pieces of ice were therefore prepared, through which the sunbeams were sent so as to develop the flowers in considerable quantity and magnitude. These pieces were then dipped into warm water contained in a glass vessel; and the effect, when the melting reached the bright spots, was carefully observed through a lens. The moment a liquid connexion was established between

* Notwithstanding the incomparable diathermaneity of the substance, M. Knoblauch finds that, when plates of rock-salt are thick enough, they always exhibit an elective absorption. Effects like those above described may possibly be the cause of this.
them and the atmosphere, the apparent bubbles suddenly collapsed, and no trace of air rose to the surface of the warm water.

10. This is the result which ought to be expected. The volume of water at 32° being less than that of ice at the same temperature, the formation of each flower ought to be attended with the formation of a vacuum, which disappears in the manner described when the ice surrounding it is melted.

Similar experiments were made with ice in which true air-bubbles were enclosed. When the melting liberated the air, the bubbles rose slowly through the liquid and floated for a time upon its surface.

11. Exposure for a second, or even less, to the action of the sun was sufficient to develop the flowers (4) in the ice. The first appearance of the central star of light was often accompanied by an audible clink, as if the substance had been suddenly ruptured. The edges of the petals were at the commencement definitely curved thus, ; but when the action was permitted to continue, and sometimes even without this, when the sun was strong, the edges of the petals became serrated thus, the beauty of the figure being thereby augmented.

Sometimes a number of elementary flowers grouped together to form a thickly-leaved cluster resembling a rose. Here and there also amid the flowers a liquid hexagon might be observed; but such were of rare occurrence.

12. The act of crystalline dissection, if I may use the term, thus performed by the solar beams, is manifestly determined by the manner in which the crystalline forces have arranged the molecules. By the abstraction of heat the molecules are enabled to build themselves together: by the introduction of heat this architecture is taken down. The perfect symmetry of the flowers, from which there is no deviation, argues a similar symmetry in the molecular architecture; and hence, as optical phenomena depend upon the molecular arrangement, we might pronounce with perfect certainty from the foregoing experiments, that ice is, what Sir David Brewster long ago proved it to be, optically speaking, uniaxal, the axis being perpendicular to the surface of freezing.
§ II.

13. On the 25th of September, while examining a perfectly transparent piece of Norway ice, which had not been traversed by the condensed sunbeams, I found the interior of the mass crowded with parallel liquid discs, varying in diameter from the tenth to the hundredth of an inch. These discs were so thin, that when looked at in section they were reduced to the finest lines. They had the exact appearance of the circular spots of oily scum which float on the surface of mutton broth, and in the pieces of ice first examined they always lay in the planes of freezing.

14. As time progressed, this internal disintegration of the ice appeared to become more pronounced, so that some pieces of Norway ice, examined in the middle of November, appeared to be reduced to a congeries of water-cells entangled in a skeleton of ice. The effect of this was rendered manifest to the hand on sawing a block of this ice, by the facility with which the saw went through it.

15. There seems to be no such thing as absolute homogeneity in nature. Change commences at distinct centres instead of being uniformly and continuously distributed; and in the apparently most homogeneous substance we should discover defects, if our means of observation were fine enough. The above observations show that some portions of a mass of ice melt more readily than others. The melting temperature of the substance is set down at 32° F.; but the absence of perfect homogeneity, whether from difference of crystalline texture or some other cause*, makes the melting temperature oscillate to a slight extent on both sides of the ordinary standard. Let this limit expressed in parts of a degree be $t$. Some parts of a block of ice will melt at a temperature of $32 - t$, while others require a temperature of $32 + t$: the consequence will be that such a block raised to the temperature of $32^\circ$ will have some of its parts liquid, and others solid.

16. When a mass exhibiting the water-discs was examined by a concentrated sunbeam, the six-leaved flowers before referred to were always formed in the planes of the discs.

17. In all my earlier experiments I found the rule to hold good, that both discs and flowers were developed in the planes of freezing; but I was subsequently surprised to find, in the same mass of ice, the discs lying in different planes. On examining such pieces I found them traversed by hazy surfaces of discontinuity, which divided the apparently continuous mass into irregular prismatic segments. When examined by allowing the red light of a fire to cross it, such ice had a beautiful appearance.

* See Mr. Faraday’s Note on this subject at the end.

The interior walls of the segments were thickly covered with rich liquid discs; in some cases the vision plunged unimpeded into the ice to a depth of several inches, while in others the prismatic segments were dotted with discs to their very centres. Fig. 1

Fig. 1.

represents one of a number of cases of this kind which I had an opportunity of examining. The network of lines are the intersections of the bounding surfaces of the segments with the surface of the slab of ice; the short lines in each segment represent the sections of the discs; and it will be observed that, while in each single segment their directions are alike, in no two segments is this the case. The flat rings denote that the discs at these places were parallel to the surface of the ice slab, while in all the other cases they were perpendicular to it.

18. I placed this slab on a table which could be caused to rotate, and bringing it into the path of a concentrated sunbeam, caused the beam to travel all round it. The little flowers started into existence with extreme suddenness and beauty. In all cases the planes of the flowers were parallel to the planes of the discs.

19. Hence the conclusion that the flowers are always parallel to the surface of freezing, must not go without qualification. There is no doubt that each of the fragments above referred to, possesses an optic axis perpendicular to the planes of the flowers developed in it; the directions of these axes being therefore as various as the planes aforesaid. How is this result brought about? Has the molecular structure of the ice been always what the last experiments show it to be? Sir David Brewster's observations are in opposition to this idea. Or can it be that the ice has changed, by a rearrangement of the constituent particles of each segment, this arrangement producing the surfaces of discontinuity by which the segments are bounded? At present we
are confined to mere conjectures; but I hope the coming winter will enable me to investigate this highly interesting question.

§ III.

20. What has been already said will prepare us for the consideration of an associated class of phenomena of great physical interest. The larger masses of ice which I examined exhibited layers in which bubbles of air were collected in unusual quantity, marking, no doubt, the limits of successive acts of freezing. These bubbles were usually elongated. Between two such beds of bubbles a clear stratum of ice intervened; and a clear surface layer, which, from its appearance, seemed to have suffered more from external influences than the rest of the ice, was associated with each block. In this superficial portion I observed detached air-bubbles irregularly distributed, and, associated with each vesicle of air, a bleb of water which had the appearance of a drop of clear oil within the solid. The adjacent figure will give a notion of these composite cavities: the unshaded circle represents the air-bubble, and the shaded space adjacent, the water.

21. When the quantity of water was sufficiently large, which was usually the case, on turning the ice round, the bubble shifted its position, rising always to the top of the bleb of water. Sometimes, however, the cell was very flat; and the air was then quite surrounded by the liquid. These composite cells often occurred in pellucid ice, which showed inwardly no other sign of disintegration.

This is manifestly the same phænomenon as that which struck M. Agassiz so forcibly during his earlier investigations on the glacier of the Aar. The same appearances have been described by the brothers Schlagintweit; and finally attention has been forcibly drawn to the subject in a recent paper by Mr. Huxley, published in the Philosophical Magazine*.

22. The only explanation of this phænomenon hitherto given, and adopted apparently without hesitation, is that of M. Agassiz and the Messrs. Schlagintweit. These observers attribute the phænomenon to the diathermancy of the ice, which permits the radiant heat to pass through the substance, to heat the bubbles of air, and cause them to melt the surrounding ice†.

* October 1857.
† Il est évident pour quiconque a suivi le progrès de la physique moderne, que ce phénomène est dû uniquement à la diathermanité de la glace (Agassiz, Système, page 157).
Das Wasser ist dadurch entstanden, dass die Luft Wärmestrahlen absorbierte welche das Eis als diathermaner Körper durchliess (Schlagintweit, Untersuchungen, p. 17).
The apparent simplicity of this explanation contributed to ensure its general acceptance; and yet I think a little reflection will show that the hypothesis, simple as it may appear, is attended with grave difficulties.

23. For the sake of distinctness I will here refer to a most interesting fact, observed first by M. Agassiz, and afterwards by the Messrs. Schlagintweit. In the *Système Glaciaire* it is described in these words: "I ought also to mention a singular property of those air-bubbles, which at first struck us forcibly, but which has since received a very satisfactory explanation. When a fragment containing air-bubbles is exposed to the action of the sun, the bubbles augment insensibly. Soon, in proportion as they enlarge, a transparent drop shows itself at some point of the bubble. This drop, in enlarging, contributes, on its part, to the enlargement of the cavity, and following its progress a little, it finishes by predominating over the bubble of air. The latter then swims in the midst of a zone of water, and tends incessantly to reach the most elevated point, at least if the flatness of the cavity does not hinder it."

24. The satisfactory explanation here spoken of is that already mentioned: let us now endeavour to follow the hypothesis to its consequences. Comparing equal weights of both substances, the specific heat of water being 1, that of air is 0.25. Hence to raise a pound of water one degree in temperature, a pound of air would have to lose four degrees.

25. Let us next compare equal volumes of the substances. The specific gravity of water being 1, that of air is $\frac{1}{770}$; hence a pound of air is 770 times the volume of a pound of water; and hence for a quantity of air to raise its own volume of water 1 degree, it must part with $770 \times 4$, or 3080 degrees of temperature.

26. Now the latent heat of water is 142.6 E. ; hence the quantity of heat required to melt a certain weight of ice is 142.6 times the quantity required to raise the same weight of water 1 degree in temperature; hence a measure of air, in order to reduce its own volume of ice to the liquid condition, must lose $3080 \times 142.6$, or 439,208 degrees of temperature.

27. This then gives us an idea of the amount of heat which, according to the above hypothesis, is absorbed by the bubble and

* Page 168. The figures to which M. Agassiz refers in the note to this page seem to be correctly drawn; but his descriptive reference to them, though in part correct, is in part unintelligible to me. He uses the term *bulles* for the bubbles of air, and *gouttelettes* for the drops of water; and I believe the latter term is always restricted to a liquid. But if we restrict the term thus throughout the passage in question, there is no escape from Mr. Huxley's conclusion, that M. Agassiz has taken the air-bubbles for drops of water, and the drops of water for air-bubbles.
communicated to the ice during the time occupied in melting a quantity of the latter equal in volume to the bubble, which time is stated to be brief; that is to say, the quantity of heat supposed to be absorbed by the air would, if it had not been communicated to the ice, have been sufficient to raise the bubble itself to a temperature 160 times that of fused east iron. Had air this power of absorption, it might be attended with inconvenient consequences to the denizens of the earth; for we should dwell at the bottom of an atmospheric ocean, the upper strata of which would effectually arrest all calorific radiation.

28. It is established by the experiments of Delaroche and Melloni*, that a calorific beam, emerging from any medium which it has traversed for any distance, possesses, in an exalted degree, the power of passing through an additional length of the same substance. Absorption takes place, for the most part, in the portion of the medium first traversed by the rays. In the case of a plate of glass, for example, 17½ per cent. of the heat proceeding from a lamp is absorbed in the first fifth of a millimetre, whereas, after the rays have passed through 6 millimetres of the substance, an additional distance of 2 millimetres absorbs less than 2 per cent. of the rays thus transmitted. Supposing the rays to have passed through a plate 25 millimetres, or an inch in thickness, there is no doubt that the heat emerging from such a plate would pass through a second layer of glass, 1 millimetre thick, without suffering any measureable absorption. For an incomparably stronger reason, the quantity of solar heat absorbed by a bubble of air at the earth’s surface, after the rays have traversed the whole thickness of our atmosphere, and been sifted in their passage through it, must be wholly inappreciable.

29. To the sifting power of the atmosphere we must add, in the case of the glacier, the absorptive power of the ice. Some notion of this power, as compared with that of air, may be gathered from the following facts:—As regards the variation of the intensity of radiant heat with the distance, the law of inverse squares is capable of the strictest experimental verification in air, even when the source of heat is far below 212° F. This implies that the absorption in the space of air through which the heat passes is too small to disturb the harmony of the law. Now a plate of ice, one-tenth of an inch thick, is absolutely impervious to heat emanating from a source, not only of 212°, but of 752° F.; and is capable, moreover, of absorbing 99½ per cent. of the calorific rays emitted by an incandescant platinum wire†. (La Thermochrose, p. 164.)

† I can hardly forbear drawing attention here to the remarkable influence
Such, if I mistake not, are the properties of radiant heat which modern physics have revealed; and I think they render it evident that the hypothesis of M. Agassiz and the Messrs. Schlagintweit was accepted without due regard to its consequences.

But as the sun does reach the lower strata of the atmosphere, it may be thought that we are protected from its effects by the radiation from the air neutralizing its absorption. The fact however is, that air may be heated to an intense degree without showing any measureable amount of radiation. The hot current from an Argand chimney produces, according to Melloni, no sensible effect upon a delicate thermo-electric apparatus. Hence which the element hydrogen appears to exercise upon radiant heat, and the longer waves of light. Wherever hydrogen enters into a compound, I think it may be predicted that it will destroy the longer undulations. For the present I will avoid speculation upon this subject, and confine myself to facts. If we examine the list of solid substances whose power to transmit radiant heat instantaneously have been examined by Melloni, we find that those at the lower end of the list, that is, the most imperfectly diathermanous substances, all contain hydrogen. In no single case, where this element occurs, is the substance capable of transmitting rays from a source of 752° F., while in every case where it does not occur, the power of transmitting rays from this source is manifested in a greater or less degree. Amber, gum, citric acid, alum, sugar-candy, tartrate of potash and soda, are the substances which exercise this destructive agency upon the longer undulations, ice being the most non-diathermanous transparent body hitherto examined. Turning to Melloni's list of liquids, the same fact reveals itself. There is a sudden fall in the power of transmission at the place where hydrogen enters the list. Protocloride of phosphorus transmits 62 per cent. of the rays incident upon it, but the next substance, hydrocarburet of chlorine, transmits only 37 per cent. From this point to the end of the Table the substances named all contain hydrogen, the list being closed by distilled water, which transmits only 11 per cent. of the calorific rays from an Argand lamp.

To the same element, I believe, is to be referred the difference between Sir W. Herschel and Seebeck as to the place of maximum heat in the solar spectrum. Herschel found this place to be beyond the extreme red; but Seebeck, with a prism of sulphuric acid, found it to be in the orange, while with a prism of water he found it in the yellow. Melloni has shown that this is due to the destruction of the less refrangible rays by the two liquids, the sulphuric acid being regarded by him as acting in a manner analogous to water. In both the water and the sulphuric acid used by Seebeck, I believe the hydrogen to be the agent which gives the observed character to the results. The colour of ice and water is also a necessary consequence of this hostility of the element hydrogen to the instantaneous passage of the longer undulations.

The mathematical theory of undulation is, perhaps, perfect; but the physics of the process, that is, the real affections of light and heat in their passage through bodies, are wholly unknown to us. Cases, therefore, like the foregoing, which single out a particular substance as exhibiting a special deportment towards light and heat, are, I think, of great value. Increased knowledge will probably enable us to connect these effects with the other properties of this substance, and thus establish physical relations which are now unknown to us.
Prof. Tyndall on some Physical Properties of Ice.

343

it is not because the air gets rid of its heat by radiation, but simply because its power of absorption is infinitely behind that supposed by the hypothesis of M. Agassiz and the Messrs. Schlagintweit, that animal and vegetable existence at the earth's surface is possible.

When air-bubbles occurred in those portions of the ice where the liquid discs appeared, the associated water spread out on planes parallel to those of the discs, evidently in consequence of the freer yielding of the ice in these planes. Keeping this remark in view, and comparing fig. 1 with fig. 10, plate 4 of M. Agassiz' Atlas, the resemblance of both phenomena will at once be perceived. The glacier ice, like the Norway ice with which I experimented, is divided into segments by surfaces of discontinuity. The air- and water-cavities are represented as flattened; but, as in the lake ice, they seemed flattened in all directions. M. Agassiz regards the flattening of the cavities as a certain proof that they have been squeezed flat by pressure; and he attributes the different directions of flattening to a power of independent motion possessed by each fragment. The perfect similarity, however, of the phenomena presented by the two kinds of ice must render us cautious in accepting an explanation which may apply to the one, but excludes the other.

§ IV.

30. But the question still remains, how are the water-chambers produced within the ice? Mr. Huxley throws out the suggestion which our knowledge at the time of his observations rendered most probable, namely, that the water had never been frozen at all, but had preserved itself, like the liquid in the cavities of a Gruyère cheese, from the névé downwards.

31. One simple test will, I think, decide the question whether the liquid is or is not the product of melted ice. If it be, its volume must be less than that of the ice which produced it, and the bubble associated with the water must be composed of rarefied air. Hence, if on establishing a liquid connexion between this bubble and the atmosphere a diminution of volume be observed, this will indicate that the water has been produced by the melting of the ice.

32. From a block of Norway ice, containing such compound bubbles, I cut a prism, and immersing it in warm water, contained in a glass vessel, I carefully watched through the side of the vessel the effect of the melting upon the bubbles. They invariably shrunk in volume at the moment the surrounding ice was melted, and the diminished globule of air rose to the surface of the water. I then arranged matters so that the wall of the cavity might be melted away underneath, without permitting the bubble
of air at the top to escape. At the moment the melting reached
the cavity the air-bubble instantly collapsed to a sphere, pos-
sessing, in some cases, far less than the hundredth part of its
original volume. The experiments were repeated with several
distinct masses of ice, and always with the same result. I think,
therefore, it may be regarded as certain that the liquid cells are
the product of melted ice *.

33. Considering the manner in which ice imported into this
country is protected from the solar rays, I think we must infer
that in the specimens examined by me†, the ice in contact with
the bubble has been melted by heat which has been conducted through
the substance without visible prejudice to its solidity.

34. Paradoxical as this may appear, I think it is no more than
might reasonably be expected from à priori considerations. The
heat of a body is referred, at the present day, to a motion of its
particles. When this motion reaches such an intensity as to
liberate sufficiently the particles of a solid from their mutual
attractions, the body passes into the liquid condition. Now as
regards the amount of motion necessary to produce this liberty
of liquidity, the particles at the surface of a mass of ice must be
very differently circumstanced from those in the interior, which
are influenced and controlled on every side by other particles.
But if we suppose a cavity to exist within the mass, the particles
bounding that cavity will be in a state resembling that of the
particles at the surface; and by the removal of all opposing action
on one side, the molecules may be liberated by a force which the
surrounding mass has transmitted without prejudice to its solidity.
Supposing, for example, that solidity is limited by molecular
vibrations of a certain amplitude, those at the surface of the
internal cavity may exceed this, while those between the cavity
and the external surface of the ice may, by their reciprocal actions,
be preserved within it, just as the terminal member of a series
of elastic balls is detached by a force which has been transmitted
by the other members of the series without visible separation ‡.

35. Where, however, experiment is within reach, we ought not
to trust to speculation; and I was particularly anxious to obtain
an unequivocal reply to the question whether an interior portion
of a mass of ice could be melted by heat which had passed through
the substance by the process of conduction. A piece of Norway
ice, containing a great number of the liquid discs already

* This of course refers to the lake ice examined as described. I venture
to predict, however, that the same will be found true of the bubbles in
glacier ice.
† And in those portions of glacier ice which are withdrawn from the
direct action of the sun.
‡ Of course I intend this to help the conception merely.
described, and several cells of air and water, was enveloped in tinfoil and placed in a mixture of pounded ice and salt. A few minutes sufficed to freeze the discs to thin dusky circles, which appeared, in some cases, to be formed of concentric rings, and reminded me of the sections of certain agates. Looked at sideways, these discs were no thicker than a fine line. The water-cells were also frozen, and the associated air-bubbles were greatly diminished in size. I placed the mass of ice between me and a gas-light, and observed it through a lens: after some time the discs and water-cells showed signs of breaking up again. The rings of the discs disappeared; the contents seemed to aggregate so as to form larger liquid spots, and finally, some of them were reduced to clear transparent discs as before.

36. But an objection to this experiment is, that the ice may have been liquefied by the radiation from the lamp, and I have experiments to describe which will show the justice of this objection. A rectangular slab, 1 inch thick, 3 inches long and 2 wide, was therefore taken from a mass of Norway ice, in which the associated air- and water-cells were very distinct. I enveloped it in tinfoil and placed it in a freezing mixture. In about ten minutes the water-blebs were completely frozen within the mass. It was immediately placed in a dark room, where no radiant heat could possibly affect it, and examined every quarter of an hour. The dim frozen spots gradually broke up into little water parcels, and in two hours the water-blebs were perfectly restored in the centre of the slab of ice. When last examined, this plate was half an inch thick, and the drops of liquid were seen right at its centre.

37. A second piece, similarly frozen and wrapped up in flannel, showed the same deportment. In an hour and a half the frozen water surrounding the air-bubbles was restored to its liquid condition. Hence no doubt can remain as to the possibility of effecting liquefaction in the interior of a mass of ice, by heat which has passed by conduction through the substance without melting it.

38. I have already referred to the formation of the liquid cavities observed by M. Agassiz, when glacier ice was exposed to the sun. The same effect may be produced by exposure to a glowing coal fire. On the 21st and 22nd of November I thus exposed plates of clear Wenham Lake ice, which contained some scattered air-bubbles. At first the bubbles were sharply rounded, and without any trace of water. Soon, however, those near the surface, on which the radiant heat fell, appeared encircled by a liquid ring, which expanded and finally became crimped at its border, as shown in the adjacent figure. The crimping became more pronounced as the action was permitted to continue*.

* The blebs observed in glacier ice also exhibit this form: see fig. 8, plate 6.
A second plate, crowded with bubbles, was held as near to the fire as the hand could bear. On withdrawing it, and examining it through a pocket lens, the appearance was perfectly beautiful. In many cases the bubbles appeared to be surrounded by a series of concentric rings, the outer ring surrounding all the others like a crimped frill.

39. I could not obtain these effects by placing the ice in contact with a plate of metal obscurely heated*, nor by the radiation from an obscure source. Indeed ice, as before remarked, is impervious to radiant heat from such a source†. The rays from a common fire also are wholly absorbed near the surface upon which they strike; and hence the described internal liquefaction was confined to a thin layer close to this surface.

40. But not only does liquefaction occur in connexion with the bubbles, but the "flowers," already described as produced by the solar beams, start by hundreds into existence when a slab of transparent ice is placed before a glowing coal fire. They, however, are also confined to a thin stratum of the substance close to the surface of incidence. In the experiments made in this way, the central stars of the flowers were often bounded by sinuous lines of great beauty.

41. The foregoing considerations show that liquefaction takes place at the surface of a mass of ice at a lower temperature than that required to liquify the interior of the solid. At the surface, the temperature $32^\circ$ produces a vibration, to produce which within the ice would necessitate a temperature of $32^\circ + x$, the increment $x$ being the additional temperature necessary to overcome the resistance to liquefaction, arising from the action of the molecules upon each other.

42. Now let us suppose two pieces of ice at $32^\circ$, with moistened surfaces, to be brought into contact with each other; we thereby virtually transfer the touching portions of these pieces from the surface to the interior, where $32 + x$ is the melting temperature. Liquefaction will therefore be arrested at those surfaces. Before being brought together, the surfaces had the motion of liquidity; but the interior of the ice has not this motion; and as equilibrium will soon set in between the masses on each side of the liquid film and the film itself, the film will be reduced to a state of the Atlas to the *Système Glaciaire*. In fig. 13 we have also a close resemblance of the flower-shaped figures produced by radiant heat in lake ice.

* To develop water-cavities within ice, a considerable time is necessary; more time, indeed, than was sufficient to melt the entire pieces of ice made use of in these contact experiments.

† Hence the soundness of the ice under the moraines: the sun's rays are converted into obscure heat by the overlying debris; this only affects a layer of infinitesimal depth, and cannot produce the disintegration of the deeper ice, as the direct sunbeams can.
motion inconsistent with liquidity. *In other words, it becomes frozen, and cements the two surfaces of ice between which it is enclosed.*

If I am right here, the importance of the physical principles involved are sufficiently manifest; if I am wrong, I hope I have so expressed myself as to render the detection of my error easy. Right or wrong, my aim has been to give as explicit utterance to my meaning as the subject will admit of.

§ V.

43. Mr. Faraday's experiments on the freezing together of pieces of ice at 32° F., and all of those recounted in the paper published by Mr. Huxley and myself, find their explanation in the principles here laid down. The conversion of snow into néré, and of néré into glacier, is perhaps the grandest illustration of the same principle†. It has been, however, suggested to me that the sticking together of two pieces of ice may be an act of cohesion, similar to that which enables pieces of wetted glass, and other similar bodies, to stick together. This is not the case. There is no sliding motion possible to the ice. When contact is broken, it breaks with the snap due to the rupture of a solid. Glass and ice cannot be made to stick thus together, neither can glass and glass, nor alum and alum, nor nitre and nitre, at common temperatures. I have, moreover, placed pieces of ice together over night, and found them in the morning so rigidly frozen together, that when I sought to separate them, the surface of fracture passed through one of them in preference to taking the surface of regelation. Many sagacious persons have also suggested to me that the ice transported to this country from Norway and the Wenham Lake may possibly retain a residue of its cold, sufficient to freeze a thin film enclosed between two pieces of the substance. But the facts already adverted to are a sufficient reply to this surmise. The ice experimented on cannot be regarded as a magazine of cold, *because parcels of liquid water exist within it.*

44. Nevertheless, as our present knowledge of the facility with which ice permits heat to pass through it by conduction is, as far as I know, absolutely null, I was glad to avail myself of an opportunity which presented itself of obtaining some approximate notion of this power. I owe this opportunity to the kindness of

* It is here implied that the contact of the moist surfaces must be so perfect, or, in other words, the liquid film between them must be so thin, as to enable the molecules to act upon each other across it. The extreme tenuity of the film may be inferred from this. A thick plate of water within the ice would facilitate rather than retard liquefaction.
† On this point see the paper referred to at the commencement.
Mr. Harrison, who had devised and perfected a machine for the manufacture of ice by the evaporation of ether. I first examined a mass of ice of the shape of the frustum of a cone. The diameter of the base was 10\(\frac{1}{2}\) inches, of the top 7\(\frac{1}{2}\) inches, and the length of the frustum was 2 feet. During the freezing of this mass a thermometer fixed on the ice showed a temperature of 8° below the zero of F., or 40° of F. below the freezing-point of water.

45. Fourteen hours after it had been frozen, the temperature of the mass, to a depth of 2 inches below the surface, was accurately 32° F. At the heart of the frustum the temperature was 31\(\frac{1}{4}\)°. The superficial portions of the frustum had been the coldest, and we see that in fourteen hours the ice of these portions rose forty degrees in temperature.

46. On the 24th of April, Mr. Harrison had the kindness to place his excellent machine entirely at my disposition. The vessel which contained the water to be frozen was shaped like the inverted frustum of a cone, and was surrounded by a jacket, between which and the side of the frustum vaporized ether circulated, the whole being placed in a vessel of water. At 11 o’clock A.M., I placed a thermometer (A) in contact with the side of the vessel, and about 2 inches below the surface of the water. The machine commenced to act, an opalescence was soon observable on the sides, and after twenty minutes’ action the thermometer was firmly imbedded in the ice. At 1 o’clock P.M., a second thermometer (B), placed at a distance of an inch from the side of the frustum, was also surrounded by ice. At 2\(\frac{1}{2}\) P.M. a third thermometer (C) was placed at a distance of 1\(\frac{1}{2}\) inch from the side of the vessel, and at 5\(\frac{1}{2}\) P.M. a fourth thermometer (D) was placed at a distance of 3 inches from the side. The observed temperatures of these thermometers at the times stated are given in the following Table:

<table>
<thead>
<tr>
<th>Time of observation</th>
<th>A</th>
<th>B</th>
<th>C</th>
<th>D</th>
</tr>
</thead>
<tbody>
<tr>
<td>h m</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>11</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>20</td>
<td>3(\frac{1}{2})</td>
<td>24</td>
<td></td>
<td></td>
</tr>
<tr>
<td>12</td>
<td>15(\frac{1}{2})</td>
<td>11(\frac{1}{2})</td>
<td>26(\frac{1}{2})</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>7(\frac{1}{2})</td>
<td>21</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>6(\frac{1}{2})</td>
<td>1(\frac{1}{2})</td>
<td>30</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>23(\frac{1}{2})</td>
<td>1(\frac{1}{2})</td>
<td>24</td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>3(\frac{1}{2})</td>
<td>9(\frac{1}{2})</td>
<td>20</td>
<td></td>
</tr>
<tr>
<td>5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8</td>
<td>- 3</td>
<td>+ 4</td>
<td>+11(\frac{1}{2})</td>
<td>+2(\frac{1}{2})</td>
</tr>
</tbody>
</table>

At 8\(\frac{1}{2}\) P.M. the machine was stopped and the supply of ether was cut off. The mercurial columns began to rise gra-
dually, and at 10 o'clock p.m. they were found to be as follows:

<table>
<thead>
<tr>
<th></th>
<th>A.</th>
<th>B.</th>
<th>C.</th>
<th>D.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>27°</td>
<td>27°</td>
<td>27(\frac{1}{2})°</td>
<td>28(\frac{1}{2})°</td>
</tr>
</tbody>
</table>

The machine rested throughout the night, and at 8 o'clock on the morning of the 25th all the thermometers stood at 32°.

47. The machine was set going at 8 A.M. on the 25th, and continued working until 2 o'clock in the afternoon, its office now being, not to freeze, but to cool the ice already frozen. The thermometers were read off every quarter of an hour, but it is not necessary to record all the observations. The following are the temperatures noted at 8 and 11\(\frac{1}{2}\) 30\(\frac{1}{2}\) A.M. and at 2 P.M.:

<table>
<thead>
<tr>
<th>Time</th>
<th>A.</th>
<th>B.</th>
<th>C.</th>
<th>D.</th>
</tr>
</thead>
<tbody>
<tr>
<td>8</td>
<td>32</td>
<td>32</td>
<td>32</td>
<td>32</td>
</tr>
<tr>
<td>1130</td>
<td>+1</td>
<td>11(\frac{1}{2})</td>
<td>23</td>
<td>30</td>
</tr>
<tr>
<td>1150</td>
<td>-11</td>
<td>-9</td>
<td>-3</td>
<td>-1</td>
</tr>
</tbody>
</table>

48. At 2\(\frac{1}{2}\) 45\(\frac{1}{2}\) the machine was stopped and the supply of \(\varpi\) cut off. The upper surface of the ice was covered with 2 inches of hair-felt: the water surrounding the frustum and its jacket was at a temperature of 32°; the thermometers were read off every half-hour, but I will limit myself at present to a few observations. The temperatures at 2 o'clock were those stated at the close of the last Table.

<table>
<thead>
<tr>
<th>Time of observation</th>
<th>A.</th>
<th>B.</th>
<th>C.</th>
<th>D.</th>
</tr>
</thead>
<tbody>
<tr>
<td>3 15 P.M.</td>
<td>+11</td>
<td>+9</td>
<td>+6</td>
<td>+2</td>
</tr>
<tr>
<td>4 15</td>
<td>18</td>
<td>17</td>
<td>14</td>
<td>11(\frac{1}{2})</td>
</tr>
<tr>
<td>5 15</td>
<td>24</td>
<td>22(\frac{1}{2})</td>
<td>20</td>
<td>18</td>
</tr>
<tr>
<td>8 15</td>
<td>28(\frac{1}{2})</td>
<td>28(\frac{1}{2})</td>
<td>27(\frac{1}{2})</td>
<td>26(\frac{1}{2})</td>
</tr>
<tr>
<td>11 15</td>
<td>31</td>
<td>30(\frac{1}{2})</td>
<td>30</td>
<td>29(\frac{1}{2})</td>
</tr>
</tbody>
</table>

On the 26th the following temperatures were observed:

<table>
<thead>
<tr>
<th>Time</th>
<th>A.</th>
<th>B.</th>
<th>C.</th>
<th>D.</th>
</tr>
</thead>
<tbody>
<tr>
<td>9 A.M.</td>
<td>32</td>
<td>31(\frac{3}{4})</td>
<td>31(\frac{1}{2})</td>
<td>31(\frac{1}{4})</td>
</tr>
<tr>
<td>1 30  P.M.</td>
<td>32</td>
<td>32</td>
<td>31(\frac{3}{4})</td>
<td>31(\frac{1}{2})</td>
</tr>
</tbody>
</table>

At 1\(\frac{1}{2}\) 30\(\frac{1}{2}\) a hole was bored in the centre of the frustum to a depth of 5 inches, and in this another thermometer (E) was placed; its temperature at 3\(\frac{1}{2}\) 30\(\frac{1}{2}\) P.M. was 30\(\frac{1}{2}\)°. At 8 o'clock in the morning of the 27th the observed temperatures were the following:

<table>
<thead>
<tr>
<th></th>
<th>A.</th>
<th>B.</th>
<th>C.</th>
<th>D.</th>
<th>E.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>32°</td>
<td>32°</td>
<td>32°</td>
<td>32°</td>
<td>32°</td>
</tr>
</tbody>
</table>
At the termination of the experiment the water surrounding
the frustum was at a temperature of 36° F.

49. These experiments show, that however bad the conduct-
ing power of ice, in comparison with other substances, may be,
the assumption that blocks of it which have been preserved in
this country through months of summer weather should still
possess a magazine of cold beyond that due to a temperature of
32°, is wholly incompatible with the physical character of the
substance.

§ VI.

50. In a very interesting paper communicated to the British
Association during its last meeting, Mr. James Thomson has
explained the freezing together of two pieces of ice at 32° in the
following manner:—"Two pieces of ice, on being pressed togeth-
er at their point of contact, will at that place, in virtue of the
pressure, be in part liquefied and reduced in temperature, and
the cold evolved in their liquefaction will cause some of the liquid
film intervening between the two masses to freeze." I am far
from denying the operation under proper circumstances of the
*vera causa* to which Mr. Thomson refers, but I do not think it
explains the facts. For freezing takes place without the inter-
vention of any pressure by which Mr. Thomson's effect could
sensibly come into play. It is not necessary to squeeze the
pieces of ice together; one bit may be simply laid upon the
other, and they will still freeze. Other substances besides ice
are also capable of being frozen to the ice. If a towel be folded
round a piece of ice at 32°, they will freeze together. Flannel
is still better. A piece of flannel wrapped round a piece of ice
freezes to it sometimes so firmly that a strong tearing force is
necessary to separate both. Cotton wool and hair may also be
frozen to ice without the intervention of any pressure which could
render Mr. Thomson's cause sensibly active*.

51. But there is a class of effects to the explanation of which
the lowering of the freezing-point of water by pressure may, I
think, be properly applied. The following statement is true
of fifty experiments or more made with ice from various quarters.
A cylinder of ice 2 inches high and an inch in diameter, was
placed between two slabs of box-wood and submitted to a gra-
dually increasing pressure. Looked at perpendicular to the axis,
cloudy lines were seen drawing themselves across the cylinder;

* It might perhaps be stated generally, that those substances which
cause the precipitation of water from the gaseous to the liquid state, are
also most influential in converting the liquid into the solid. "Hemp, wool,
hair," are also the substances on which the so-called *ground-ice* is formed
most readily. See Arago's Report, *Annuaire pour l'an 1833.*
and when the latter was looked at obliquely, these lines were found to be the sections of dim hazy surfaces which traversed the cylinder, and gave it an appearance closely resembling that of a crystal of gypsum whose planes of cleavage had been forced out of optical contact by some external force.

Fig. 2 represents the cylinder looked at perpendicular to its axis, and fig. 3 the same cylinder where looked at obliquely.

52. To ascertain whether the rupture of optical contact which these experiments disclosed was due to the intrusion of air between two separated surfaces of ice, a cylinder of ice, 2 inches long and 1 inch wide, was placed in a copper vessel containing ice-cold water. The ice-cylinder projected half an inch above the surface of the water. Placing the copper vessel on a slab of wood, and a second slab of wood upon the cylinder of ice, the whole was subjected to pressure. When the hazy surfaces were well developed in the portion of the ice above the water, the cylinder was removed and examined. The planes of rupture extended throughout the entire length of the cylinder, just the same as if it had been squeezed in free air.

Still the removal of the cylinder from its vessel might be attended with the intrusion of air into the fissures. I therefore placed a cylinder of ice, 2 inches long and 1 inch wide, in a stout vessel of glass, which was filled with ice-cold water. Squeezing the whole, as in the last experiment, the surfaces of discontinuity were seen forming under the liquid quite as distinctly as in air.

53. The surfaces are due to compression, and not to any tearing asunder of the mass by tension, and they are best developed where the pressure, within the limits of fracture, is a maximum. A cylindrical piece of ice, one of whose ends was not parallel to the other, was placed between slabs of wood and subjected to pressure. Fig. 4 shows the disposition of the experiment. The
effect upon the ice-cylinder was that shown in fig. 5, the surfaces being developed along that side which had suffered the pressure.

54. Sometimes the surfaces commence at the centre of the cylinder. A dim small spot is first observed, which, as the pressure continues, expands until it sometimes embraces the entire transverse section of the cylinder.

55. On examining these surfaces with a pocket-lens, they appeared to me to be composed of very minute water parcels, like what is produced upon a smooth cold surface by the act of breathing. Were they either vacuous plates, or plates filled with air, their aspect would, on optical grounds, be far more vivid than it really was.

56. A concave mirror was so disposed, that the diffused light of day was thrown full upon the cylinder while under pressure. Observing the expanding surfaces through a lens, they appeared in a state of intense commotion; this was probably due to the molecular tensions of the little water parcels. This motion followed closely on the edge of the surface as it advanced through the solid ice. Once or twice I observed the hazy surfaces pioneered through the mass by dim offshoots apparently liquid. They constituted a kind of negative crystallization, having the exact form of the crystalline spines and spurs produced by the congelation of water upon a surface of glass. I have no doubt, then, that these surfaces are produced by the liquefaction of the solid in planes perpendicular to the direction of pressure.

57. The surfaces were developed with great facility where they corresponded to the surfaces of freezing. Wherever the liquid discs before described were observed, the surfaces were always easily developed in the planes of the discs. By care I succeeded, in some cases, in producing similar effects in surfaces at right angles to the planes of the discs, but this was very difficult and uncertain.
I think the following new facts have been established in the foregoing paper:

1. The interior of a mass of ice may be melted by radiant heat which has passed through exterior portions of the substance without melting them.

2. In the dissolution of the ice thus effected, the substance yields by the formation of liquid spaces, each possessing the shape of a flower with six petals, a small vacuum being formed at the centre of each flower.

3. These flowers are generally formed in planes parallel to the surface of freezing; but some of the specimens of ice examined were divided into prismatic segments by surfaces of discontinuity. Each distinct segment had its own system of flowers arranged in parallel planes; but the parallelism did not extend to the flowers of two distinct segments. This subject requires further investigation.

4. The appearance presented by masses of lake ice composed of these segments, resembles that of certain specimens of glacier ice in which the air- and water-cavities are flattened in different planes. Hence the explanation of the latter, which refers them to actions peculiar to the glacier, must be received with caution.

5. The explanation hitherto given of the water associated with the air-bubbles in glacier ice is untenable. In this paper the phenomenon is explained in accordance with the dynamical theory of heat. It is sought to be shown that, owing to the mutual action of the ice-molecules, a quantity of heat which has been conducted through the substance without prejudice to its solidity may liberate the molecules which bound an internal cavity, and thus produce water-cells in association with the bubbles of air.

6. The converse of this takes place where two moist surfaces of ice at 32° F. are brought into contact. Superficial portions are thus virtually rendered central; liquefaction is checked, the film of moisture on the surfaces in contact congeals, and the pieces of ice freeze together. To this process the term regelation has been applied.

7. By the application of pressure, parallel surfaces of discontinuity are formed in lake and river ice perpendicular to the directions in which the pressure is exerted; thus giving the substance the appearance of selenite, in which the planes of cleavage are not in optical contact. The discontinuity consists in the liquefication of the ice in these planes by the pressure. Such surfaces are formed with great facility parallel to the planes in which the liquid flowers are formed by radiant heat, while it is very difficult to produce them perpendicular to these planes.

Thus, whether we apply heat or pressure, lake ice melts with peculiar facility in certain directions.

Note from Mr. Faraday.

My dear Tyndall,

I have the following remarks, made in reference to the irregular fusibility of ice, to which you drew my attention, any interest to you, or by an occasional bearing on such cases, any value in themselves? Deal with them as you like.

Imagine a portion of the water of a lake about to freeze, the surface $S$ being in contact with an atmosphere considerably below $32^\circ$, the previous action of which has been to lower the temperature of the whole mass of water, so that the portion below the line $M$ is at $40^\circ$, or the maximum density, and the part above at progressive temperatures from $40^\circ$ upwards to $32^\circ$; each stratum keeping its place by its relative specific gravity to the rest, and having therefore, in that respect, no tendency to form currents either upwards or downwards. Now generally, if the surface became ice, the water below would go on freezing by the cold conducted downwards through the ice; but the successive series of temperatures from $32^\circ$ to $40^\circ$ would always exist in a layer of water contained between the ice and the dense water at $40^\circ$ below $M$. If the water were pure, no action of the cold would tend to change the places of the particles of the water or cause currents, because, the lower the cold descended, the more firmly would any given particle tend to retain its place above those beneath it: a particle at $e$, for instance, at $36^\circ$ F., would, when the cold had frozen what was above it, be cooled sooner and more than any of the particles beneath, and so always retain its upper place as respects them.

But now, suppose the water to contain a trace of saline matters in solution. As the water at $32^\circ$ froze, either at the surface or against the bottom of the previously-formed ice, these salts would be expelled; for the ice first formed (and that always formed, if the proper care be taken to displace the excluded salts) is perfectly free from them, and pure. The salts so excluded would pass into the layer of water beneath, and there produce two effects: they would make that layer of greater specific gravity than before, and so give it a tendency to sink into the warmer under layer; but they would also make it require a
lower temperature than 32° for congelation; this it would acquire from the cold ice above, and by that it would become lighter and float, tending to remain uppermost; for it has already been shown that the diminution of temperature below 32° in sea water and solution of salts, is accompanied by the same enlargement of bulk as between 32° and 40° with pure water. The stratum of water, therefore, below the ice, would not of necessity sink because it contained a little more salt than the stratum immediately below it; and certainly would not if the increase of gravity conferred by the salts was less than the decrease by lowering of temperature. An approximation of the strata between the freezing place and the layer at 40° would occur, i.e. the distance between these temperatures would be less, but the water particles would keep their respective places.

When water freezes, it does not appear that this process is continuous; for many of the characters of the ice seem to show that it is intermittent: i.e. either a film of ice is formed, and then the process stops until the heat evolved by solidification has been conducted away upwards, and the next stratum of water has been sufficiently cooled to freeze in turn; or else the freezing being, so to speak, continuous, still is not continued at the same constant rate, but, as it were, by intermittent pulsations. Now it may well be, when a layer next the previously-formed ice, and containing an undue proportion of salts, has been cooled down to its required temperature for freezing (which would be below 32°), that, on freezing, the congelation will pervade at once a certain thickness of the water, excluding the salts from the larger portion of ice formed, but including them as a weak solution within its interstices. The next increment of cold conducted from the ice above would freeze up these salts in the ice containing them, at the same time that a layer of pure ice was formed beneath it. Thus a layer of ice fusible at a lower temperature than the ice either above or below it might be produced; and by a repetition of the process many such layers might be formed.

It does not follow necessarily that the layers would be perfectly exact in their disposition. Very slight circumstances tending to disturb the regularity of the water-molecules would be sufficient, probably, to disturb the layers more or less. Ice contains no air, and the exclusion of a minute bubble of air from the water in the act of freezing might disturb the direction and progress of the congelation, and cause accumulation of the extra saline liquid in one spot rather than another: so might the tendency to the formation of little currents, either arising from the separation of the saline water from the forming ice, or from the elevation of temperature in different degrees at those places where the congelation was going on at different rates.

2 A 2
The effect would not depend upon the quantity of salts contained in the freezing water, though its degree would. The proportion of salts necessary to be added to pure water to lower its freezing-point 1° F. may be very sensible to chemical tests; but the proportion required to make the difference \( \frac{1}{100} \)th or \( \frac{1}{1000} \)th of a degree would be far less: and if we suppose that only \( \frac{1}{100} \)th of a piece of ice is brought into the condition of melting before the rest of the mass, and that the salts in that proportion were originally in the whole of the water, then its quantity there may be so small as to escape detection except by very careful analysis. However, it would be desirable to examine the water chemically which is produced by ice distinguished by having in its interior much that liquefies before the rest.

It is easy to make ice perfectly free from air, and, as I believe, from salts, by a process I formerly described. It would be interesting to see if such ice had within it portions melting at a lower temperature than the general mass. I think it ought not.

Ever truly yours,

M. Faraday.

Royal Institution, Dec. 9, 1857.

XXXVIII. On a Theorem relating to Hypergeometric Series.

By A. Cayley, Esq.*

In attempting to verify a formula of Hansen’s relating to the development of the disturbing function in the planetary theory, I was led to a theorem in hypergeometric series: viz.

writing, as usual,

\[
F(\alpha, \beta, \gamma, x) = 1 + \frac{\alpha \cdot \beta \cdot x}{1 \cdot \gamma} + \frac{\alpha \cdot \alpha + 1 \cdot \beta \cdot \gamma + 1}{1 \cdot 2 \cdot \gamma} x^2 + \ldots
\]

then the product

\[
F(\alpha, \beta, \gamma + \frac{1}{2}, x)F(\gamma - \alpha, \gamma - \beta, \gamma + \frac{1}{2}, x)
\]

is connected with

\[
(1 - x)^{-(\gamma - \alpha - \beta)}F(2\alpha, 2\beta, 2\gamma, x)
\]

by a simple relation; for if the last-mentioned expression is put equal to

\[
1 + Bx + Cx^2 + Dx^3 + \ldots
\]

then the product in question is equal to

\[
1 + \frac{\gamma}{\gamma + \frac{1}{2}} Bx + \frac{\gamma \cdot \gamma + 1}{\gamma + \frac{1}{2} \cdot \gamma + \frac{3}{2}} Cx^2 + \frac{\gamma \cdot \gamma + 1 \cdot \gamma + 2}{\gamma + \frac{1}{2} \cdot \gamma + \frac{3}{2} \cdot \gamma + \frac{5}{2}} Dx^3 + \&c.
\]

* Communicated by the Author.
The form of the identity thus arrived at will be best perceived by considering a particular case. Thus, comparing the coefficients of $x^3$, we have

$$\frac{\alpha \cdot \alpha + 1 \cdot \gamma + 2 \cdot \beta \cdot \beta + 1 \cdot \beta + 2}{1 \cdot 2 \cdot 3 \cdot \gamma + \frac{1}{2} \cdot \gamma + \frac{3}{2} \cdot \gamma + \frac{5}{2}}$$

$$+ \frac{\alpha \cdot \beta \cdot \gamma - \alpha \cdot \gamma - \alpha + 1 \cdot \gamma - \beta \cdot \gamma - \beta + 1}{1 \cdot 2 \cdot \frac{1}{2} \cdot \gamma + \frac{3}{2} \cdot \gamma + \frac{5}{2} \cdot \gamma + \frac{7}{2}}$$

$$+ \frac{\gamma - \alpha \cdot \gamma - \alpha + 1 \cdot \gamma - \alpha + 2 \cdot \gamma - \beta \cdot \gamma - \beta + 1 \cdot \gamma - \beta + 2}{1 \cdot 2 \cdot 3 \cdot \gamma + \frac{1}{2} \cdot \gamma + \frac{3}{2} \cdot \gamma + \frac{5}{2} \cdot \gamma + \frac{7}{2}}$$

$$= \frac{2 \alpha \cdot 2 \alpha + 1 \cdot 2 \alpha + 2 \cdot 2 \beta \cdot 2 \beta + 1 \cdot 2 \beta + 2}{1 \cdot 2 \cdot 3 \cdot 2 \gamma \cdot 2 \gamma + 1 \cdot 2 \gamma + 2}$$

$$+ \frac{2 \alpha \cdot 2 \beta \cdot \gamma - \alpha - \beta}{1 \cdot 2 \cdot 2 \gamma \cdot 2 \gamma + 1 \cdot \frac{1}{2} \cdot \gamma - \alpha - \beta + 1}$$

$$+ \frac{2 \alpha \cdot 2 \gamma}{1 \cdot \gamma - \alpha - \beta + 1 \cdot \gamma - \alpha - \beta + 2 \gamma - \alpha - \beta + 2}$$

It may be observed that the function on the right-hand side is, as regards $\alpha$, a rational and integral function of the degree 3, and as such may be expanded in the form

$$\Lambda \alpha \cdot \alpha + 1 \cdot \alpha + 2$$

$$+ B \alpha \cdot \alpha + 1 \cdot \gamma - \alpha$$

$$+ C \alpha \cdot \gamma - \alpha \cdot \gamma - \alpha + 1$$

$$+ D \gamma - \alpha \cdot \gamma - \alpha + 1 \cdot \gamma - \alpha + 2,$$

and that the last coefficient $D$ can be obtained at once by writing $\alpha = 0$; this in fact gives

$$D \gamma + 1 \cdot \gamma + 2 = \frac{\gamma - \beta \cdot \gamma - \beta + 1 \cdot \gamma - \beta + 2}{1 \cdot 2 \cdot 3 \cdot \gamma + \frac{1}{2} \cdot \gamma + \frac{3}{2} \cdot \gamma + \frac{5}{2}}$$

and thence

$$D = \frac{\gamma - \beta \cdot \gamma - \beta + 1 \cdot \gamma - \beta + 2}{1 \cdot 2 \cdot 3 \cdot \gamma + \frac{1}{2} \cdot \gamma + \frac{3}{2} \cdot \gamma + \frac{5}{2}}$$

which agrees with the left-hand side of the equation; and the value of the first coefficient $\Lambda$ may be obtained in like manner with a little more difficulty; but I have not succeeded in obtaining a direct proof of the equation. The form of the equation shows that the left-hand side should vanish for $\gamma = -\frac{1}{2}$, which may be at once verified.

Grasmere, August 25, 1858.
XXXIX. On Chloro-arsenious Acid, and some of its Compounds.
By William Wallace, Ph.D., F.C.S., Glasgow*.

IN the course of a former series of experiments on the chloride of arsenic†, I observed that arsenious acid dissolved freely in the anhydrous chloride. Believing that a definite combination was formed, I have recently investigated the subject more fully, and have succeeded in preparing a new and highly interesting compound, to which I have given the name of chloro-arsenious acid. My examination of the acid and its compounds is still far from complete; but I purpose in the mean time to submit a short abstract of my results to the members of the British Association.

1. Solution of Arsenious Acid in Chloride of Arsenic.

When chloride of arsenic is heated to gentle ebullition in a small tubulated retort, the beak of which is inclined upwards, and powdered arsenious acid is gradually introduced, the solution of the latter appears to cease when the liquid contains equal equivalents of the two compounds. The readiest method of preparing this solution in quantity is to introduce a few ounces of arsenious acid into a flask, and pass hydrochloric acid gas through it until all the arsenious acid disappears. The flask should be agitated occasionally. The action is very violent, and is attended by the elimination of much heat. If the passage of the hydrochloric acid gas is continued as long as absorption takes place, pure chloride of arsenic is obtained. The nature of the reaction has been fully explained in the paper already referred to.

2. Chloro-arsenious Acid.

When the solution of arsenious acid in chloride of arsenic is gradually distilled until it begins to froth up, there separates, on cooling, a pasty, viscid, semi-fluid mass, from which the more liquid portion may be poured off. Analysis gave as follows:—

\[
\begin{align*}
\text{Arsenic} & \quad \ldots \quad 1=75 \quad 59.29 \\
\text{Chlorine} & \quad 28.11 \quad 1=35.5 \quad 28.06 \\
\text{Oxygen} & \quad \ldots \quad 2=16 \quad 12.65 \\
\sum & \quad 126.5 \quad 100.00
\end{align*}
\]

The formula of this compound, therefore, is AsCl₂ + 2AsO₃ or AsClO₂, that is, arsenious acid in which one equivalent of oxygen is replaced by chlorine.

Anhydrous chloro-arsenious acid is a viscid fluid or a very soft

* Communicated by the Author, having been read at the Meeting of the British Association at Leeds, September 24, 1868.
On Chloro-arsenious Acid, and some of its Compounds. 359

solid, according to the temperature to which it is exposed. It is transparent, but has a brown colour, which does not appear to be owing to the presence of any impurity. It fumes slightly in the air, parting with a small portion of its chlorine as hydrochloric acid, and absorbing oxygen. When strongly heated, it boils up with considerable frothing, and affords a distillate of pure chloride of arsenic. When brought up to about the temperature at which arsenious acid sublimes, it leaves a glassy, hard, transparent substance, which was found to contain 10.94 per cent. of chlorine, agreeing with the formula $2\text{AsO}_3$, $\text{AsClO}_3$.

The fluid poured off from the chloro-arsenious acid was found, on analysis, to contain an amount of chlorine which corresponds with the formula $\text{AsCl}_3$, $\text{AsO}_3$. It is therefore similar to the solution prepared by adding arsenious acid to heated chloride of arsenic. I do not believe, however, that this liquid is a compound of these two substances; if a definite combination at all, its constitution is probably $3\text{AsClO}_3$, $\text{AsCl}_3$.

Chloro-arsenious acid is also formed when chloride of arsenic is treated with a quantity of water not quite sufficient to dissolve it. On adding small successive portions of water to the chloride, the proportion of chlorine in the undissolved quantity gradually diminishes, until the last globules consist chiefly of the compound acid.

3. Hydrated Chloro-arsenious Acid.

Chloride of arsenic is dissolved in the smallest possible quantity of water (about 16 equivalents), and the solution set aside in a closed flask. In two or three days minute nucleated crystals begin to form; and these gradually increase until about one-half of the liquid is occupied by them. A second crop of crystals may be obtained by placing a fragment of rock-salt in the mother-liquor: these take a long time to form, and are much larger and better defined than those which result from the first operation. The crystals may be well pressed with a platinum spatula, and then dried by pressure between numerous folds of blotting-paper. A portion was analysed which had been completely dried by powerful pressure, and the following results were obtained:

<table>
<thead>
<tr>
<th>Substance</th>
<th>Amount</th>
<th>Analysis</th>
<th>Dried</th>
</tr>
</thead>
<tbody>
<tr>
<td>Arsenic</td>
<td>51.80</td>
<td>$1 = 75$</td>
<td>51.90</td>
</tr>
<tr>
<td>Chlorine</td>
<td>24.97</td>
<td>$1 = 35.5$</td>
<td>24.57</td>
</tr>
<tr>
<td>Oxygen</td>
<td>...</td>
<td>$2 = 16$</td>
<td>11.07</td>
</tr>
<tr>
<td>Water</td>
<td>12.35</td>
<td>$2 = 18$</td>
<td>12.46</td>
</tr>
<tr>
<td></td>
<td></td>
<td><strong>141.5</strong></td>
<td><strong>100.00</strong></td>
</tr>
</tbody>
</table>

The crystallized acid contains, therefore, two equivalents of water, and is represented by the formula $2\text{H}_2\text{O}$, $\text{AsClO}_3$. It becomes anhydrous over oil of vitriol, but at the same time loses
2 or 3 per cent. of chlorine. The crystals are exceedingly minute, and form in mammillated masses resembling the mineral *Prehnite*. The slowly-formed crystals are acicular, and collect in stellate groups, presenting, while in the liquid, a very beautiful appearance. The smaller crystals have a dazzling white colour, and emit a little hydrochloric acid on exposure to the air.

Chloro-arsenious acid combines with chlorides, as arsenious acid does with oxides. It appears to be bibasic, the two equivalents of water being capable of being replaced by two equivalents of an alkaline chloride. The ammonia-salt is the only one which I have as yet succeeded in obtaining in a distinctly crystalline form, and of definite composition. Potash and lime compounds have been obtained as white powders which contain much less than two equivalents of alkaline chloride; so that these compounds have probably only one equivalent of alkaline chloride, and one equivalent of basic water.

Two interesting reactions of the solution of terechloride of arsenic in water may here be mentioned: oil of vitriol immediately throws down the anhydrous compound, while chloride of calcium causes the separation of the chloride mixed with a small proportion of chloro-arsenious acid. The same reactions occur with a saturated solution of arsenuous acid in concentrated aqueous hydrochloric acid: indeed, an ounce or two of chloride of arsenic may readily be prepared by adding an equal bulk of strong oil of vitriol to such a solution. It is not so pure, however, as that obtained from the aqueous solution of chloride of arsenic, and must be rectified if required in a state of purity.


The aqueous solution of chloride of arsenic is mixed with strong liquid hydrochloric acid in sufficient quantity to prevent the formation of chloro-arsenious acid, and a small lump of chloride of ammonium is introduced. At first small, hard, reddish-coloured, cubical crystals, consisting of almost pure chloride of ammonium, make their appearance; but after some days, long fibrous needles of snow-white colour and pearly lustre begin to form, and gradually fill up the liquid. These consist of the salt under consideration. They are well drained, and dried by pressure between folds of blotting-paper. The following results were obtained with the salt dried over oil of vitriol:

<table>
<thead>
<tr>
<th></th>
<th></th>
<th>1 =</th>
<th>100 =</th>
</tr>
</thead>
<tbody>
<tr>
<td>Arsenic</td>
<td>32.23</td>
<td>75</td>
<td>32.12</td>
</tr>
<tr>
<td>Ammonium</td>
<td>15.23</td>
<td>36</td>
<td>15.42</td>
</tr>
<tr>
<td>Chlorine</td>
<td>44.78</td>
<td>106.5</td>
<td>45.61</td>
</tr>
<tr>
<td>Oxygen</td>
<td>...</td>
<td>16</td>
<td>6.85</td>
</tr>
<tr>
<td></td>
<td></td>
<td>233.5</td>
<td>100.00</td>
</tr>
</tbody>
</table>
The formula of the dry salt is therefore $2\text{NH}_4\text{Cl}, \text{AsClO}_3$. The loss of water by exposure over oil of vitriol amounted to 4·27 per cent.; one equivalent of water gives 3·71 per cent. During the desiccation a little chlorine is evolved and replaced by oxygen.

I am still engaged in prosecuting the investigation of the compounds of chloro-arsenious acid with the metallic chlorides, and in endeavouring to form corresponding acids containing iodine and bromine.

---

**XL. Chemical Notices from Foreign Journals.**

*By E. Atkinson, Ph.D.*

[Continued from p. 298.]

In the series of bibasic acids of the general formula $\text{C}_n\text{H}_{2n-2}\text{O}_3$, commencing with oxalic acid, $\text{C}_4\text{H}_2\text{O}_3$, and terminating with sebacic acid, $\text{C}_{20}\text{H}_{18}\text{O}_3$, the terms containing 18, 10, and 6 equivalents were until lately wanting. Recently some new acids have been described which fill up the vacant spaces.

Two acids have been described of the formula $\text{C}_{10}\text{H}_8\text{O}_3$, lipic and pyrotartaric acids. Lipic acid was found by Laurent along with suberic acid, $\text{C}_{16}\text{H}_{14}\text{O}_3$, pimelic acid, $\text{C}_{14}\text{H}_{12}\text{O}_3$, and adipic acid, $\text{C}_{12}\text{H}_{10}\text{O}_3$, among the products of oxidation of oleic acid by nitric acid; but since then it has not been again obtained, and its existence was doubtful. Gerhardt put in its place pyrotartaric acid, obtained by the destructive distillation of tartaric acid; but Arppe, who investigated this acid, found that, although it had the same formula, it differed materially in form, solubility, melting-point, and properties of its salts, from the other members of the series.

An acid was discovered by Barral in tobacco, which he named nicotic acid. This acid he did not analyse, but from its decompositions he assigned to it the formula $\text{C}_6\text{H}_4\text{O}_3$, which would be the missing term containing 6 equivs. carbon. In a recent investigation of the oxidation products of Chinese wax, Mr. Buckingham* found a new acid which he named anchoic acid. The analyses of the acid, and of its silver, lead, and potassium salts, fix its formula at $\text{C}_{18}\text{H}_{16}\text{O}_3$, which is the formula of the missing term containing 18 equivs. carbon.

Most of these bibasic acids are formed by the action of nitric acid on the fats: the acids produced in this reaction have been recently investigated by Wirz†, who, besides establishing the ex-

† Liebig's Annalen, December 1857.
istence of lipic acid, has found among them an acid containing 18 equivalents of carbon. This acid is identical with Buckton's acid, with whose results, however, Wirz was unacquainted. The material for the investigation was obtained by acting on about five pounds of the fatty acids from cocoa-nut oil with nitric acid at the boiling temperature; at the end of two months the action was terminated, and the mass in the retort solidified to a crystalline mass. On treating this with water, it separated into a fatty mass which floated on the surface; a yellowish, thick, oily body, heavier than water, and which on further investigation proved to be a mixture of nitrocapric and nitrocaprylic acids, and an aqueous solution containing five or six acids, whose separation was effected by fractional crystallization.

The crystals which first separate are generally considered to be suberetic acid. But an analysis of the silver salt showed that a higher acid was present; and on recrystallization it was found to contain, besides suberetic acid, C_{16}H_{14}O_{8}, an acid which gave on analysis numbers agreeing with the formula C_{18}H_{16}O_{8}. The description of the properties of this acid, which Wirz names lepargyllic acid, leave no doubt that it is identical with Buckton's anchoic acid. Wirz analysed also the silver and lead salts.

In the mother-liquor from these acids Wirz found pimelic acid, C_{14}H_{12}O_{8}, and adipic acid, C_{12}H_{10}O_{6}; and besides these he was enabled to confirm the existence of Laurent's lipic acid. This acid differs in many respects from the rest of the series. It crystallizes from water in transparent crusts formed of aggregated small hard nodules, which again consist of minute prisms. It can be sublimed after one distillation, and is then obtained in the form of long brilliant needles. By repeated distillation it is transformed, like succinic acid, into the anhydrous acid. Wirz prepared and analysed several of the salts, and made a comparative investigation of this acid and pyrotartaric acid, which proved that they were not identical. In the last mother-liquor Wirz found succinic acid, but neither the acid below this, C_{6}H_{4}O_{8}, nor oxalic acid.

Dessaignes has recently described* a new acid obtained in small quantities by the oxidation of malic acid by bichromate of potash, which is the missing term of the series, containing six atoms of carbon. It appears identical with Barral's nicotic acid; but until its identity is established, Dessaignes names it malonic acid. To obtain it, bichromate of potash is gradually added to a weak solution of malic acid, and the action is complete when about an equivalent in weight has been added. The mixture is then treated with milk of lime to precipitate the chrome, and

* Comptes Rendus, July 12. Chemical Gazette, September 15, 1858.
the filtrate precipitated by acetate of lead. The precipitate, which contains chromate of lead, is treated with nitric acid, which, when dilute, only dissolves the organic salt. When this liquid is three-fourths neutralized by ammonia, the salt precipitates; when this precipitate is treated with sulphured hydrogen, and the liquid filtered, the acid is obtained in crystalline plates on evaporating the solution.

It forms large rhombic crystals, which have a lamellar structure; they are soluble in water, with a strongly acid reaction and odour. Heated to 100° the acid loses water; at 140° it melts, and boils at 150° C., disengaging carbonic acid, while acetic acid distils over. Treated with sulphuric acid it is decomposed; and it reduces chloride of gold.

It forms with potash and ammonia neutral salts, which are deliquescent, but crystallize in dry air. The corresponding acid salts are readily obtained in large crystals. It precipitates lime, baryta, and silver salts.

Its analogies with oxalic acid are manifest. As the latter decomposes into carbonic and formic acids, so malonic acid decomposes into acetic and carbonic acids.

\[
\begin{align*}
C_4H_2O_8 & = C_2H_2O_4 + 2CO_2, \\
\text{Oxalic acid. Formic acid.} \\
C_6H_4O_8 & = C_4H_4O_4 + 2CO_2, \\
\text{Malonic acid. Acetic acid.}
\end{align*}
\]

But it does not present with succinic acid, the next homologue, that regular gradation of chemical functions which distinguishes the true homologue:

The aldehydes and acetones may be regarded, according to Gerhardt and Chancel, as being formed on the type of hydrogen, \( \text{H} \), the general formula of an aldehyde being \( \text{X} \), and of acetone \( \text{Y} \), in which \( \text{X} \) is a ternary oxygen radical, and \( \text{Y} \) an alcohol radical. Thus the aldehyde of acetic acid, \( C_4H_4O_2 \), is \( C_2H_3O_2 \), or \( \text{Y} \), in which an atom of hydrogen is replaced by the radical othyle; and the corresponding acetone, \( C_4H_3O_2 \), has the other atom of hydrogen replaced by methyle. A powerful support for this view is found in the experiment of Williamson, who, by distilling a mixture of one atom valerianate of potash with one atom acetate of potash, obtained a mixed acetone, \( C_{10}H_9O_3 \).

Friedel* has found that a mixed acetone is obtained by distil-

Friedel on the Aldehydes and Acetones.

ling a mixture, in equal equivalents, of acetate and benzoate of potash. By distilling the crude product from this reaction, which contains acetone, benzole, and a substance resembling benzophenone, a body is obtained which boils at 198°, and solidifies at 14° C. into large crystalline plates. Its formula is \( \text{C}^6\text{H}^8\text{O}^2, \text{or} \ \frac{\text{C}^4\text{H}^4\text{O}^4}{\text{C}^2\text{H}^3} \), or methyluret of benzoyle, as acetone is methyluret of othyle. By distilling a mixture of acetate and butyrate of potash, the mixed acetone, \( \text{C}^8\text{H}^7\text{O}^2 \), was obtained.

Friedel found also that the action of pentachloride of phosphorus on acetone is analogous to its action on aldehyde. When acetone is gradually added to pentachloride of phosphorus, a violent action is set up, hydrochloric acid is disengaged, and the residue contains, besides oxychloride of phosphorus, two substances which, after washing with water, may be separated by fractional distillation. One boiling at 70° C. has the formula \( \text{C}^6\text{H}^6\text{Cl}^2 \), is isomeric with chloride of propylene, but boils at 30 degrees lower; the other, which is probably a product of decomposition of the first, boils at 30°, and has the composition \( \text{C}^6\text{H}^5\text{Cl} \). The action would thus be

\[ \text{C}^6\text{H}^6\text{O}^2 + \text{PCl}^5 = \text{PO}^2\text{Cl}^8 + \text{C}^6\text{H}^6\text{Cl}^2. \]

Acetone. New body.

A further support for the view that the acetones contain alcohol radicals, Friedel* found in the deportment of hydrochloric acid towards acetone, which also furnishes a means of obtaining the alcohol from the acid. At ordinary temperatures hydrochloric acid has no other action on acetone than that of readily dissolving. But when a saturated solution of HCl in acetone is heated in a sealed vessel in the water-bath for some time, it separates into two layers. On opening the vessel a large quantity of gas escapes, which is chloride of methyle, and the residual liquid contains acetic acid and unaltered acetone. The decomposition is thus—

\[ 2\text{C}^6\text{H}^6\text{O}^2 + 4\text{HCl} = \text{C}^4\text{H}^4\text{O}^4 + 4\text{C}^2\text{H}^3\text{Cl}. \]

Acetone. Acetic acid. Chloride of methyle.

Hydriodic acid acts at ordinary temperatures on acetone. When this body is saturated with hydriodic acid gas, it may be at once distilled; at 43°—45° iodide of methyle passes over, which, however, is slightly contaminated with acetone, from which it is with difficulty separated. The residue contains acetic

Action of Pentachloride of Phosphorus on Hydride of Benzoyle. 365

acid; and this with iodide of methyle are the only products of the action. The iodide of methyle treated with oxalate of silver yielded oxalate of methyle, from which pure methyllic alcohol was prepared. The best means of producing the above decomposition is to heat acetone with concentrated aqueous hydriodic acid in closed vessels.

If, according to the above view, the acetones are aldehydes in which hydrogen is replaced by an alcohol radical, we might expect to obtain the acetones by the action of the iodide of an alcohol radical on the metallic compounds of the aldehydes. Ebersbach* has tried this experiment with valerianic aldehyde, expecting the action to be in accordance with the equation—

$$\text{C}^{10} \text{H}^{9} \text{O}^{2}_2 \text{Na} + \text{C}^{4} \text{H}^{5} \text{I} = \text{C}^{10} \text{H}^{9} \text{O}^{2}_2 \text{C}^{4} \text{H}^{5} + \text{NaI.}$$

Sodium-valeraldehyde. Acetone.

The sodium compound of valeraldehyde was obtained by adding sodium to valeraldehyde, in which it dissolved with evolution of hydrogen and disengagement of heat, to a gelatinous yellowish mass. Iodide of ethyle did not act on this in the cold, and only slightly at 100°C.; but heated to 120°C. for some time, a large quantity of iodide of sodium separated. The mass was then washed with water, the excess of iodide of ethyle removed by heating in the water-bath, and the liquid then fractionally distilled. In this way a product boiling at 200°C. was obtained, heavier than water, and not miscible with it, readily soluble in ether and alcohol, with a pleasant atherial odour and a bitter taste. It yielded on analysis numbers agreeing with the formula C^{18}H^{18}O^{2}. Hence it was not the compound

$$\text{C}^{10} \text{H}^{9} \text{O}^{2}_2 \text{C}^{4} \text{H}^{5} \text{Na}_2 \rightleftharpoons \text{Na}_2 \text{C}^{10} \text{H}^{8} \text{O}^{2}_2 + 2\text{NaI.}$$

but was valeraldehyde in which two equivalents of hydrogen had been replaced by two equivalents of ethyle, according to the equation

$$\text{C}^{10} \text{H}^{8} \text{O}^{2}_2 \text{Na}_2 + 2\text{C}^{4} \text{H}^{5} \text{I} = \text{C}^{10} \text{H}^{8} \text{O}^{2}_2 \text{C}^{4} \text{H}^{5} + 2\text{NaI.}$$

Sodium-valeraldehyde. New substance.

By the action of pentachloride of phosphorus on hydride of benzoyle, C^{14}H^{6}O^{2}, Cahours obtained chlorobenzole, C^{14}H^{6}Cl^{2}, and he regarded it as hydride of benzoyle in which oxygen is replaced by chlorine. Later investigations of Wicke† and of Engelhardt‡ seem to prove that this is not the constitution of the body, but that it is rather the chloride of a biatomic radical, C^{14}H^{6}, analogous to olefiant gas.

* Liebig's Annalen, June 1858.
† Phil. Mag. October 1857.
This action of pentachloride of phosphorus has been tried on the aldehydes of the fatty acid series.

By acting on c<e><e>anthylic aldehyde, C<sup>14</sup>H<sup>14</sup>O<sub>2</sub>, Limpricht* obtained chloro<e>anthanole. Equal equivalents of pentachloride of phosphorus and c<e>anthanole were mixed together and the product distilled, and the part boiling above 150° C. collected separately. This product, washed with water, then shaken with bisulphite of ammonia, dried and rectified, yielded pure chloro<e>anthanole, C<sup>14</sup>H<sup>14</sup>Cl<sub>2</sub>. The action was in accordance with the equation—

\[
\text{C}^{14}\text{H}^{14}\text{O}^{2} + \text{PCl}^{5} = \text{C}^{14}\text{H}^{14}\text{Cl}^{2} + \text{PO}^{2} \text{Cl}^{3}.
\]

It is a transparent, mobile liquid, lighter than water, and boiling at 191° C. Heated with sodium-alcohol in closed tubes to 250° C. it is decomposed into hydrochloric acid, monochlorinated chloro<e>anthanole, C<sup>14</sup>H<sup>13</sup>Cl, which boils at 155° C., and a hydrocarbon of which the formula is probably C<sup>14</sup>H<sup>13</sup>. Treated with sodium, and then frequently rectified over bright metallic sodium, chloro<e>anthanole yielded the hydrocarbon C<sup>14</sup>H<sup>14</sup>. It is a liquid boiling at 95°, and with a peculiar odour.

Ebersbach† in a similar manner has obtained the corresponding compound C<sup>10</sup>H<sup>10</sup>Cl<sub>2</sub> from valeraldehyde. It is a transparent, mobile liquid, with a feeble and not unpleasant odour: it has the specific gravity 1.05, is insoluble in water, and boils at 180°. Boiled with alcoholic potash it loses chlorine and yields the compound C<sup>10</sup>H<sup>9</sup>Cl.

Geuther‡ examined the action of pentachloride of phosphorus on common aldehyde. Aldehyde was added to pentachloride of phosphorus in a retort which was kept cool, until only a slight excess of that substance remained; a slight evolution of HCl took place, arising from the presence of a trace of water in the aldehyde. On applying heat, the whole dissolved up to a homogeneous liquid, which on being rectified gradually blackened. When the temperature had reached 100° C. the receiver was changed; the distillate which then passed over was oxychloride of phosphorus. On mixing the distillate below 100° with water, an oily body separated, which, when washed, dehydrated over chloride of calcium and rectified, had the boiling-point 60° C. The liquid has a sweetish ætherial smell and taste, strongly resembling chloroform: at 4° it has the specific gravity 1.9. It

* Liebig's Annalen, July 1857.
† Ibid. June 1858. ‡ Ibid. March 1858.
is attacked by potash with extreme difficulty. Heated in the water-bath in a sealed tube with that substance, chloride of potassium was formed, and a volatile chlorine compound, which evaporated at the warmth of the hand, and burned with a green-edged flame.

These three chlorides, from their mode of formation, and from the regular gradation of their properties as far as they have been examined, evidently belong to one series. They are isomeric with the chlorides of the hydrocarbons, C\(^{2n}\)H\(^{2n}\), but they are not identical; they differ in the boiling-points and in other properties. Thus chloride of ethylene boils at 82° C., and is readily attacked by alcoholic potash; the chloride from aldehyde boils at 60° C., and is attacked by potash with extreme difficulty. The difference in boiling-points corresponding to a difference of C\(^{2}\)H\(^{2}\) is 19 degrees in the series of chlorides of ethylene and its homologues, while it is 26 degrees in the series of chlorides from the aldehydes.

The properties of the body obtained by Geuther, as far as they have been studied, are not very dissimilar to those of Regnault's chlorinated chloride of ethyle. This body boils at 64°, and has the specific gravity 1.24, smells like oil of olefiant gas, and has a sweet but peppery odour. In its deportment towards alcoholic potash it has some of the characteristics of a derivative of aldehyde; for when treated with that reagent it yields chloride of potassium and a brown sticky resin, while the same odour is perceived which is produced when aldehyde is treated by potash. On the other hand, this substance is not altered by boiling with potassium; and although the corresponding aldehyde compound has not been examined in this respect, its higher homologue, chloroenanthole, is decomposed by sodium without very great difficulty. The higher homologues of chlorinated chloride of ethyle are not known; but the corresponding methyle compound, C\(^{2}\)H\(^{2}\)Cl\(^{2}\), has been described by Regnault, and its boiling-point is 30°. Hence a difference of 34.5 C. corresponding to a difference of C\(^{2}\)H\(^{2}\).

It is probable, on the whole, that these three chlorides belong to three different isomeric series—the chlorides of olefiant gas and its homologues, the chlorides from the aldehydes, and the chlorinated chlorides of the alcohol radicals.

Pasteur has observed* that, in the alcoholic fermentation, a portion of the sugar becomes changed into succinic acid. When the fermented liquid is evaporated and neutralized, and precipitated with a silver salt, the precipitate decomposed by sulphur-

* *Comptes Rendus*, vol. xlvi, p. 179.
etted hydrogen, and the liquid evaporated, the succinic acid is obtained in crystals. The quantity amounts to a half per cent. of the fermented sugar. In wine also Pasteur found succinic acid.

According to the same chemist*, glycerine also invariably occurs among the products of the alcoholic fermentation of sugar; and he estimates its amount at 3 per cent. of the fermented sugar. It is further contained in all fermented liquors, and especially in wine.

Bunsen explained the formation and occurrence of sal-ammoniac in lava, by the theory that, in the passage of lava over surfaces covered with vegetation, the nitrogen of the plants becomes free in the form of carbonate of ammonia, which, with the hydrochloric acid continuously evolved from the heated lava, forms sal-ammoniac. Bunsen had observed, some months after an eruption at Hecla, that only the lower part, which had flowed over grass-land, showed sal-ammoniac fumaroles, and that higher up, and in proportion as the vegetation disappeared, the formation of sal-ammoniac ceased also. This theory was opposed by von Waltershausen, who considered that the volcanic sal-ammoniac arose from the atmospheric ammonia.

Ranieri† has communicated some observations on the lava stream of Vesuvius in 1850, which confirm Bunsen's view. The course of the lava was partly over the old stream of 1834, partly (and especially where it reached the plain) over cultivated land. In the latter portions, sal-ammoniac was found for a year and a half after the eruption, and in considerable quantities. In the lava which had flowed over the older stream, very little sal-ammoniac was found, and few fumaroles.

The greater number of fumaroles which occur in the lava which has passed over cultivated land, arises from the gases formed by the action of the lava on organic matter, which force their way through the still soft lava. And the small quantities of fumaroles which occur near the older lava may arise from the decomposition of accidental animal deposits.

Ranieri admits that the ammonia of the atmosphere may give rise to formation of sal-ammoniac, but only in a subordinate degree.

† Liebig's Annalen, December 1857.
XLI. Note on the Equation in Numbers of the First Degree between any number of Variables with Positive Coefficients. By J. J. Sylvester, M.A., F.R.S., Professor of Mathematics in the Royal Military Academy.

I propose to show that all the systems of values \((x, y, z \ldots w)\) which satisfy a given equation in integers,

\[ ax + by + cz + \ldots + lw = n, \]

\((a, b, c \ldots l)\) being all positive, and the number of systems therefore definite, may be made to depend on algebraical equations whose coefficients are known functions of \(a, b, c \ldots l\) and \(n\). The fact is somewhat surprising, the proof easy, being an immediate consequence of the theorem I have given in the Quarterly Journal of Mathematics, and also in Tortolino's Annali for Jan. 1857, of the problem of the partition of numbers.

For my present purpose, this theorem may be with advantage presented under a somewhat modified form as follows:—Let \(\Theta(\mathfrak{p}t)\) be used to denote the coefficient of \(\frac{1}{t}\) in the expansion of \(\mathfrak{p}t\) in ascending powers of \(t\). Let \(N\) stand for the number of solutions of the equation

\[ ax + by + cz + \ldots + lw = n; \]

let \(m\) be the least common multiple of \(a, b, c \ldots l\),

\(p\) be any primitive root of \(p^m = 1\),

and \(pe^{-nt}\) be called \(\Lambda p\); then

\[ N = \sum \Theta \left\{ \frac{\Lambda(-n)}{(1-\Lambda a)(1-\Lambda b)\ldots(1-\Lambda l)} \right\}. \]

If now we call \(N'\) what \(N\) becomes when, in lieu of the equation

\[ ax + by + cz + \ldots + lw = n, \ldots \ldots \ldots \quad (1) \]

we write

\[ ax' + ax'' + by + cz + \ldots + lw = n, \ldots \ldots \ldots \quad (2) \]

it is clear that

\[ N' = \sum \Theta \left\{ \frac{\Lambda(-n)}{(1-\Lambda a)^2(1-\Lambda b)\ldots(1-\Lambda l)} \right\}. \]

But it is also clear that all the solutions of equation (2) may be derived from those of equation (1), by writing for each value of \(x\)

\[ x' + x'' = x; \ldots \ldots \ldots \ldots \ldots \quad (3) \]

and as the number of solutions of equation (3) is evidently \(x + 1\),

* Communicated by the Author.

we have $N' = \Sigma x + N$, or

$$\Sigma x = \Sigma \Theta \left\{ \frac{\Lambda a \cdot \Lambda (-n)}{(1 - \Lambda a)^2(1 - \Lambda b) \ldots (1 - \Lambda l)} \right\}.$$

In like manner, if we write

$$ax' + ax'' + ax''' + by + cz + \ldots + lw = n,$$

the solutions of this equation spring from those of equation (1) by making $x' + x'' + x''' = x$, the number of solutions of which equality is $(x + 1)(x + 2)$; wherefore

$$\Sigma \frac{x^2 + 3x + 2}{2} = \Sigma \Theta \left\{ \frac{\Lambda (-n)}{(1 - \Lambda a)^3(1 - \Lambda b) \ldots (1 - \Lambda l)} \right\};$$

from which we may readily deduce, by aid of what has been already shown,

$$\Sigma x^2 = \Sigma \Theta \frac{(\Lambda a)(1 + \Lambda a) \Lambda (-n)}{(1 - \Lambda a)^3(1 - \Lambda b) \ldots (1 - \Lambda l)};$$

and so in general,

$$\Sigma x^i = \Sigma \Theta \frac{\Lambda (a)(1 + \Lambda a) \ldots ((i - 1) + \Lambda a)}{(1 - \Lambda a)^{i+1}(1 - \Lambda b) \ldots (1 - \Lambda l)}.$$

Again, if we write

$$ax + by_1 + by_2 + \ldots + by_e + cz + \ldots + lw = n, \ldots (4)$$

we shall find by parity of reasoning (seeing that in this last equation the solutions may be derived from those of equation (1) by keeping $x, z, \ldots w$ all unaltered, whilst we give to $y_1, y_2, \ldots y_e$ all the values compatible with $y_1 + y_2 + \ldots + y_e = y$), the value of $\Sigma x^i$ in equation (4) will be the same as that of

$$\Sigma x^i \cdot \frac{(y + 1)(y + 2) \ldots (y + e)}{1 \cdot 2 \ldots e}$$

in equation (1). Wherefore we shall evidently obtain

$$\Sigma x^i \cdot y^e = \Sigma \Theta \frac{\Lambda a(1 + \Lambda a) \ldots ((i - 1) + \Lambda a) \times \Lambda b(1 + \Lambda b) \ldots ((e - 1) + \Lambda b)}{(1 - \Lambda a)^{i+1}(1 - \Lambda b)^{e+1}(1 - \Lambda c) \ldots (1 - \Lambda l)};$$

the extension of the theorem to $\Sigma x^i \cdot y^e \cdot z^o \ldots$ is too obvious to need further allusion.

Thus, then, to find $x_1, x_2, \ldots x_N$, we may begin by forming an equation of the Nth degree, whose coefficients are known, because the sums of the powers of the roots are given. Suppose these roots to consist of $N_1$ values $x$, $N_2$ values $x_2$, $\ldots N_\mu$ values $x_\mu$, the solution of $\mu$ simple equations will enable us to find the sum of the $N_1$ values of $y$ corresponding to $x_1$, the sum of the $N_2$ values of $y$ corresponding to $x_2$, $\ldots$, and the sum of
the \( N^\mu \) values of \( y \) corresponding to \( x^\mu \). To effect this, we have only to write down the values of \( \Sigma xy, \Sigma x^2y, \ldots \Sigma x^\mu y \). In like manner we may find the sum of the \( N^1 \) values of \( y^9 \) corresponding to \( x^1 \), the \( N^2 \) values of \( y^2 \) corresponding to \( x^2, y \), and so in general for \( y^\mu \). Thus, then, we may obtain the requisite number of sets of equations for determining independently by means of equations of the degrees \( N^1, N^2, \ldots N^\mu \) respectively the values of \( y \) corresponding to each of the distinct values of \( x \); and in like manner for all the other variables. The principal interest of this note consists, however, in the appreciation of the fact that we can represent algebraically, as has been shown above, the value of \( \Sigma x^\alpha y^\beta z^\gamma \ldots \), where the sign of summation extends over all the simultaneous solutions of

\[
ax + by + cz + \&c. = n.
\]

This is a considerable advance upon the conception (itself before my discovery entirely unrecognized*) of the explicit representability of the mere number of the solving systems \( x, y, z \ldots \) by general algebraical formulæ. By this new theorem we pass, as it were, from the shadow to the substance.


In the Opera Minora of the great Euler, in the last page of his second memoir on the partition of numbers (vol. i. p. 400), occur these words:—"Ex hoc principio definiri potest quot solutiones problemata que ab arithmetis ad regulam virginum referri solent, admittunt; hujusmodi problemata huic redunt ut inveniri debant numeri \( p, q, r, s, \&c. \), uta ut his duabus conditionibus satisfiat,

\[
ap + bq + cr + ds &c. = n, \text{ et } \alpha p + \beta q + \gamma r + \delta s &c. = \nu;\]

et jam quæstio est quot solutiones in numeris integris positivis locum sint habiturate ubi quidem tenendum est numeros \( a, b, c, d \ldots n \) et \( \alpha, \beta, \gamma, \delta \ldots \nu \) esse integros;" and he then proceeds to observe that the number in question is the coefficient of \( x^m y^n \) in the expansion of the expression

\[
\frac{1}{(1-x^n y^m)(1-x^b y^\beta)(1-x^c y^\gamma) \ldots}
\]

in terms of ascending positive powers of \( x \) and \( y \).

* As witness the comparatively unfruitful labours of Paoli, Herschel, Kirkman, and even of Cayley. But as honest labour is seldom entirely wasted, so in the present case it was my valued friend Mr. Kirkman’s Manchester memoir on partitions which first drew and fixed my attention on the subject.

† Communicated by the Author.
Why the solution in integers of two simultaneous equations with an indefinite number of variables should be referred to "the rule of the Virgins" I am at a loss to conjecture, unless indeed it be supposed to have some mystical reference to the alligation or coupling of the coefficients of the two equations*. The problem in question may be otherwise stated as having for its object to discover the number of modes in which the couple \( m, n \) may be made up of the couples \( a, \alpha; b, \beta; c, \gamma \&c. \)

I need hardly remark that Euler's form of representation is no solution, but merely a transformation of the question. The problem in its most general form is to determine the number of modes in which a given set of conjoint partible numbers \( l_1, l_2, \ldots l_r \) can be made up simultaneously of the compound elements,

\[
a_1, a_2, \ldots a_r; \quad b_1, b_2, \ldots b_r; \quad c_1, c_2, \ldots c_r; \quad \&c.
\]

The problem of simple partition has been already completely resolved by the author of this notice; but the resolution of the problem of double, and still more of multiple decomposition in general, seemed to be fenced round with insurmountable difficulties.

Let the reader imagine then with what surprise and joyful emotion, within a few days of despatching my previous paper on Partitions to this present Number of the Magazine, following out a train of thought suggested by the simple idea in that paper contained, I found myself led, as by a higher hand, to the marvellous discovery that the problem of compound partition in its utmost generality is capable of a complete solution—in a word, that this problem may in all cases be made to depend on that of simple partition. The theorem by which this is effected has been already confided to the great mathematical genius of England, and will be shortly committed to the 'Transactions' of one of our learned societies; for the present I shall confine myself to a disclosure of the general character of the theorem without

* Professor De Morgan has kindly furnished me with the following information as to the use of this singular phrase:—

"I have seen this process cited as the rule of—Ceres, Series, Virginum, Virginum, Ceres and Virginum, Series and Virginum, Ceres and Virginum, Series and Virginum. I do not think any one of the eight is missing. I cannot find that Ceres is attended by any maidens, and I cannot guess who the ladies were. It is applied by the mathematicians to the rule of alligation when of an indeterminate number of solutions—just Euler's problem which you quote." Mr. De Morgan subsequently writes, "I forget whether they wrote Series or Ceres; I think the latter;" and adds a pleasant caution against indulging a passion for one of these algebraical virgins; "for that though Jupiter did once animate a statue maiden at the prayer of an enamoured sculptor, yet even Jupiter himself could not impart a body to an algebraical abstraction."
going into any details. Thus, then, may the theorem be stated in general terms:—

Any given system of simultaneous simple equations to be solved in positive integers being proposed, the determination of the number of solutions of which they admit may in all cases be made to depend upon the like determination for one or more systems of equations of a certain fixed standard form. When a system of r equations between n variables of the aforesaid standard form is given, the determination of the number of solutions in positive integers of which it admits may be made to depend on the like determination for \( \frac{n(n-1) \ldots (n-r+2)}{1 \cdot 2 \ldots (r-1)} \) single independent equations derived from those of the given system by the ordinary process of elimination, with a slight modification; the final result being obtained by taking the sum of certain numerical multiples (some positive, others negative) of the numbers corresponding to those independent determinations. This process admits of being applied in a variety of modes, the resulting sum of course remaining unaltered in value whichever mode is employed, only appearing for each such mode made up of a different set of component parts*.

In the Problem of the Virgins, where but two equations are concerned, the equations are reduced to the standard form when the two coefficients of every the same variable in the two equations are prime to one another, and when no two pairs of coeffi-

* Since the above was in print, I have discovered a much more specific theorem, which, indeed, is to be regarded as the fundamental theorem in the doctrine of compound partition, and the basis of that given in the text. It is as follows:—If there be r simultaneous simple equations between n variables (in which the coefficients are all positive or negative integers) forming a definite system (i. e. one in which no variable can become indefinitely great in the positive direction without one or more of the others becoming negative), and if the r coefficients belonging to each of the same variable are exempt from a factor common to them all, and if not more than \( r-1 \) of the variables can be eliminated simultaneously between the \( r \) equations, then the determination of the number of positive integer solutions of the given system may be made to depend on like determinations for each of \( n \) derived independent systems, in each of which the number of variables and equations is one less than in the original system.

This reduction in general can be effected in a great but limited variety of modes. When only two equations, however, are concerned, the number of modes is always two, neither more nor less. So that in fact we are still navigating in the narrows, and have not fairly entered upon the wide ocean of the theory of compound partitions until we have passed the case of double partition. When the given system supposed definite is one of three equations between four variables, the number of modes of reduction is twelve or sixteen, according to that type out of two (to one or the other of which it must of necessity belong) under which the system falls. The theory of types applicable to any system of simultaneous simple equations with rational coefficients, here faintly shadowed forth, constitutes, I apprehend, a new and important branch in the theory of inequalities.
Pror. Sylvester on the Problem of the Virgins,

cients have the same ratio; and for this problem the process is always limited to only two modes of application. The method, however, in a very important class of cases admits of being applied in one, and only one mode when these conditions are not strictly fulfilled.

Thus the virgins who appeared to Euler, but with their forms muffled and their faces veiled, have not disdained to reveal themselves to me under their natural aspect. Wonderful indeed has been the history of this theory of partitions. Notwithstanding that the immortal Euler had written two elaborate memoirs on the subject, that Paoli, and I believe other Italian mathematicians, had taken it up from another but less advantageous point of view, so completely had it fallen into oblivion, as far as the mathematicians of this country are concerned, that Sir John Herschel has written a memoir upon it, inserted in the Philosophical Transactions, without any reference to, and evidently in complete unconsciousness of, the labours of his predecessors, and subsequently Professor De Morgan, so justly celebrated for his mathematical erudition, in a paper in the Cambridge and Dublin Mathematical Journal, refers to the doctrine of partitions as being of quite recent creation. The importance of the subject in these later times has been vastly augmented by the magnificent applications which our great mathematical luminary has made of it to the doctrine of invariants.

Postscript.—In the first instance I discovered the theorem above given by a method of induction, aided by an effort of imagination, and confirmed by numerous trials; but I have since obtained a very simple, although somewhat subtle general proof of it. Mr. Cayley on his part, and independently, has also laid the foundation of a most ingenious and instructive method of demonstration entirely distinct from my own. I reason upon the equations, Mr. Cayley upon the Eulerian generating function; but it was by operations performed upon this function that I was myself originally led to a perception of the transcendental analogies out of which I was enabled to evolve the law.

The very interesting case of the composition of a proposed integer out of elements given both in number and specie, to which Euler has called particular attention, falls without preparation under the standard form; for this question is in fact merely that of determining the number of solutions of the binary system of equations,

\[ ax + by + cz + \ldots + lw = m, \]
\[ x + y + z + \ldots + w = \mu, \]

\(a, b, c, \ldots l\) being supposed to be all different.
Thus, by way of very simple illustration, suppose it required to find in how many ways the number $m$ can be made up of $\mu$ elements, limited to consist of the numbers 1, 2, 3. My method gives me at once the following solution. Call $v$ the number required. Then $m$ must be not less than $\mu$, and not greater than $3\mu$, or there will be no solutions. For all values of $m$ between $\mu$ and $2\mu$, both inclusive, 

$$v = \frac{m-\mu}{2} + \frac{3}{4} + (-1)^{\frac{m-\mu}{2}};$$

for all values of $m$ between $2\mu$ and $3\mu$, still both both inclusive,

$$v = \frac{3\mu-m}{2} + \frac{3}{4} + (-1)^{\frac{m-\mu}{2}}.$$

It will be observed that when $m=2\mu$, the two formulæ give the same value, so that either may be employed. Again, suppose we wish to express the number of modes of composition of $m$ with the four elements 1, 2, 3, 4, the number of parts being $\mu$, $\frac{m}{\mu}$ must be not less than 1 nor greater than 4, or there will be no solutions possible.

For all values of $m$ from $\mu$ to $2\mu$ inclusive,

$$v = \frac{1}{12}((m-\mu+3)^2-\frac{7}{6}) + \frac{1}{3}(-1)^{m-\mu} + \frac{1}{3}(\rho^m - \mu + \rho^{m-\mu}),$$

$\rho$, $\rho'$ being the prime cube roots of unity.

For all values of $m$ from $2\mu$ to $3\mu$ inclusive,

$$v = \frac{(m-\mu+3)^2 - (m-2\mu-3)^2}{12} - \frac{(m-2\mu-3)^2}{4} + \frac{73}{36}$$

$$+ \frac{1}{3}(\rho^m - \mu + (-1)^m$$

$$+ \frac{1}{3}(\rho^m - \mu + \rho^{m-\mu}).$$

Finally, for all values of $m$ from $3\mu$ to $4\mu$ inclusive,

$$v = \frac{1}{12}\{(4\mu - m + 3)^2 - \frac{7}{6}\} + \frac{1}{3}(-1)^m$$

$$+ \frac{1}{3}(\rho^m - \mu + \rho^{m-\mu}).$$

At the joining points (so to say) between the successive cases, viz. where $m=2\mu$ or $m=3\mu$, the contiguous formulæ give like results whichever of them is applied, so that the discontinuity in the form of the solution resembles that arising from the juxtaposition of different curves*. This discontinuity (in itself

* The connexion between the contiguous formulæ is always closer than what is symbolized by the phrase used above. The curves must be regarded as not merely placed end to end, but to be, as it were, knit or spliced together through a certain finite portion of the extent of each of them. Thus the first and second formulæ in the text coincide in value, not merely for $m+2\mu$, but also for $m=2\mu-1$ and $m=2\mu-2$; and the second and third formulæ coincide, not merely for $m=3\mu$, but also for
a remarkable phenomenon to be brought to light), far from being a reproach to the method employed, is to be regarded as a quality inherent in the subject matter under representation, and inexpugnable, as such, in the very nature of things.

K Woolwich Common,
October 2, 1858.

---

XLIII. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 312.]

March 18, 1858.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—


The deposits of mineral matters from natural waters offer many points of interest to the geologist. Besides the rock-salt and gypsum, which in many cases have doubtless been formed by the spontaneous evaporation of saline waters, it is well known that many mineral springs charged with carbonic acid under pressure, deposit great quantities of earthy salts when they come to the surface, and that the travertines thus formed often constitute extensive masses. The deposit from the hot alkaline saline springs of Carlsbad, which forms great beds, was found by Berzelius to consist chiefly of carbonate of lime, with portions of oxide or carbonate of iron, and small quantities of silica, strontia, phosphoric acid and fluorine; the analyses of other chemists have added to the list of elements met with in these and similar precipitates, manganese, zinc, cobalt, nickel, chromium, arsenic, antimony, tin, copper and lead. Carbonate of magnesia is however wanting, or present only in very small proportion, in these deposits; and the same is true of the calcareous sinter from cold springs. The Carlsbad water, however, contains for 17 parts of carbonate of lime 10 parts of carbonate of magnesia; but this latter salt, according to Berzelius, is only deposited after evaporation.

The analyses by Berzelius and Struve of the various carbonated waters of Germany, show that carbonate of lime is generally present in much larger quantities than carbonate of magnesia; and it is only in the waters of Püllna and the Elisenbrunnen of Krenznach, which contain very little carbonic acid, that we find a large amount of carbonate of magnesia, with a small portion of carbonate of lime. The water of Püllna, according to Struve, contains in 1000 parts 32.72 of solid matters, consisting of sulphates and chlorides of sodium,

\[ m = 3\mu + 1 \] and \[ m = 3\mu + 2 \]. The adjacent curves have, so to say, in the instance above, the same tangents and circles of curvature at the points of union, so that we may be said to modulate from one formula into another. The raison raisonnée of this fact is easily explicable on à priori analytical principles.
magnesium and a little calcium, besides \(0.1\) of carbonate of lime and 
\(0.83\) of carbonate of magnesia; it contains only \(\frac{7}{100}\)ths of its volume 
of carbonic acid gas.

In my analyses of the waters of the western basin of Canada, I have 
found many brine-springs, which, although rising from Lower Silurian 
limestones, hold no appreciable amount of earthy carbonates, but 
contain, besides common salt, large quantities of chlorides of calcium 
and magnesium; they are in fact veritable bitterns. The mineral 
springs of these palæozoic strata appear to be in all cases connected 
with undulations producing disruptions of the strata, through which 
the subterranean waters find egress. In the almost undisturbed 
region of the west, the springs are consequently rare, but in the 
disturbed country further east, along the north-western limit of the 
Green Mountains, which are composed of these same palæozoic 
strata in an altered condition, the mineral waters become very 
abundant. Five or six springs, often differing in kind, may sometimes 
be found within a short distance along the same line of fault; but 
where the strata become crystalline, the mineral waters are no longer 
met with.

In this eastern region, the saline waters issuing from the same 
limestones as the springs just described are generally more dilute 
than those of the west, and although, like them, containing but very 
little carbonic acid, deposit by boiling or evaporation large quantities 
of earthy salts, chiefly carbonate of magnesia. Many of these waters 
contain earthy chlorides, and are analogous to the Pillna spring, 
while others, still strongly saline, are alkaline from the presence of 
carbonate of soda. The solubility of the carbonate of magnesia in 
these waters is explained by the observations of H. Rose, who has 
shown that the partial precipitate produced in the cold, by carbonate 
of soda in a solution of a neutral salt of magnesia, is redissolved by an 
excess either of the magnesian salt or the alkaline carbonate, and is 
only thrown down from these solutions by heat. Longchamp has 
further remarked that the precipitation by heat is rendered less com-
plete in proportion as the carbonate, sulphate or hydrochlorate of the 
alkali is in excess, and that the precipitate at first formed under these 
circumstances is redissolved on cooling. I have verified this last 
observation in the case of these natural waters, from which the mag-
nesian carbonate is only separated when they are evaporated to a 
small volume. When thus evaporated, even at a very gentle heat, 
these mineral waters yield large quantities of granular carbonate of 
magnesia, often nearly pure.

With these facts in view, it is very easy to trace a relation between 
the saline waters containing carbonate of magnesia, and another class 
of springs in which the predominant element is carbonate of soda 
with small quantities of common salt, borax, and earthy carbonates. 
These waters, although wanting in the west, are very abundant in 
eastern Canada, and rise from the same formations as the saline 
springs, but are most abundant in the argillaceous strata immediately 
overlying the lower limestones, which appear to be the source of the 
salines. These alkaline waters probably owe their origin to the slow
decomposition of felspathic débris in presence of earthy carbonates. By the mingling of these solutions of carbonate of soda with the bitterns of the limestones, the carbonate of lime would be precipitated, except so far as an excess of carbonic acid were present, while carbonate of magnesia would remain in solution, as in the Carlsbad waters. The mixture of these alkaline springs with sea-water would yield similar results.

From my analyses of more than sixty of the different mineral springs of Canada, to be found in the published reports of the Geological Survey, I select a few characteristic waters of each class, giving here only approximatively the determinations of the principal ingredients for 1000 parts.

A. Saline waters, containing little or no earthy carbonate.
B. Alkaline waters, feebly saline.
C. Saline waters, holding abundance of earthy carbonates.
   a. Neutral, containing earthy chlorides.
   b. Alkaline, containing carbonate of soda.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>A. Whitby</td>
<td>46·30</td>
<td>...</td>
<td>17·53</td>
<td>9·54</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>&quot; Hallowell</td>
<td>68·00</td>
<td>15·90</td>
<td>12·90</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>&quot; Hallowell</td>
<td>36·00</td>
<td>9·20</td>
<td>9·40</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>B. Chambly</td>
<td>2·13</td>
<td>1·06</td>
<td>...</td>
<td>...</td>
<td>0·04</td>
<td>0·07</td>
</tr>
<tr>
<td>&quot; Nicolet</td>
<td>1·56</td>
<td>1·13</td>
<td>...</td>
<td>...</td>
<td>0·17</td>
<td>0·13</td>
</tr>
<tr>
<td>&quot; Saint-Ours</td>
<td>0·53</td>
<td>0·13</td>
<td>...</td>
<td>...</td>
<td>not determined</td>
<td></td>
</tr>
<tr>
<td>&quot; Jacques-Cartier</td>
<td>0·34</td>
<td>0·19</td>
<td>...</td>
<td>...</td>
<td>0·07</td>
<td>0·03</td>
</tr>
<tr>
<td>&quot; Joly</td>
<td>0·75</td>
<td>0·23</td>
<td>...</td>
<td>...</td>
<td>0·06</td>
<td>0·02</td>
</tr>
<tr>
<td>C. a. Caledonia (V.)</td>
<td>14·64</td>
<td>...</td>
<td>0·28</td>
<td>1·03</td>
<td>0·12</td>
<td>0·86</td>
</tr>
<tr>
<td>&quot; Saint-Léon</td>
<td>13·83</td>
<td>...</td>
<td>0·07</td>
<td>0·66</td>
<td>0·35</td>
<td>0·94</td>
</tr>
<tr>
<td>&quot; Caxton</td>
<td>13·65</td>
<td>...</td>
<td>0·05</td>
<td>0·37</td>
<td>0·21</td>
<td>1·06</td>
</tr>
<tr>
<td>&quot; Plantagenet</td>
<td>13·15</td>
<td>...</td>
<td>0·13</td>
<td>0·21</td>
<td>0·05</td>
<td>0·89</td>
</tr>
<tr>
<td>&quot; Sainte-Geneviève</td>
<td>20·99</td>
<td>...</td>
<td>0·60</td>
<td>2·05</td>
<td>0·01</td>
<td>0·75</td>
</tr>
<tr>
<td>&quot; Berthier</td>
<td>9·06</td>
<td>...</td>
<td>0·04</td>
<td>0·08</td>
<td>0·05</td>
<td>0·83</td>
</tr>
<tr>
<td>C. b. Varennes</td>
<td>9·58</td>
<td>0·32</td>
<td>...</td>
<td>...</td>
<td>0·35</td>
<td>0·35</td>
</tr>
<tr>
<td>&quot; Fitzroy</td>
<td>8·34</td>
<td>0·59</td>
<td>...</td>
<td>...</td>
<td>0·15</td>
<td>0·78</td>
</tr>
<tr>
<td>&quot; Caledonia (1)</td>
<td>7·75</td>
<td>0·05</td>
<td>...</td>
<td>...</td>
<td>0·15</td>
<td>0·52</td>
</tr>
</tbody>
</table>

Few of the above waters contain sulphates, but baryta and strontia are present in very many of them; the amount of these two bases in the Varennes spring is equal to 0·016, while in that of Lanoroie, a water of the class B, containing 12·88 of solid matters, there were found 0·030 of baryta and 0·021 of strontia. Small quantities of silica, alumina, phosphoric acid, manganese and iron are present in all of these springs, and in the alkaline and many of the saline waters a portion of boracic acid; the borate is included with the carbonate of soda in the above analyses. Bromine and iodine are found in all the saline waters. I have shown, in my analyses of five alkaline saline waters from Caledonia and Varennes, that the amount of
carbonic acid is much less than is required to form bicarbonates with the soda, lime and magnesia which these waters contain, so that the magnesia must be held dissolved as a mono-carbonate. In the water of Chambly, on the contrary, there is no deficiency of carbonic acid, and the bases exist as bicarbonates. The temperatures of these springs range from 46° to 53° F.; some of them are therefore to be regarded as slightly thermal.

Interstratified with the shales and sandstones of the Quebec division of the Lower Silurian rocks, which immediately overlie the strata yielding these alkaline waters, are found thick beds of pure limestone, sometimes presenting the agatized structure and semi-translucency which characterize certain travertines, but at other times opake, homogeneous, and including remains of orthoceratites, trilobites and other fossils. Associated with these beds of pure carbonate of lime are others which are magnesian, and contain considerable quantities of carbonate of iron, which causes them to weather reddish brown. These beds are always granular in texture, and contain a variable portion of siliceous sand; they often become conglomerate, enclosing pebbles of quartz and schist, or more frequently fragments of a pure compact limestone, seemingly identical with that of the beds just described. Thin layers of the ferruginous magnesian rock sometimes separate beds of the pure carbonate of lime, or form lenticular masses in its midst, and seem to replace its fossils. The pure limestones also sometimes form the base of a conglomerate, or are mixed with sand and argillaceous matters.

These magnesian rocks, like the pure limestones of this formation, occur in irregular and interrupted beds; they often attain a thickness of many yards, are destitute of fossils, and contain from ten to forty per cent., and even more, of sand or clay. The portion soluble in acids is sometimes a dolomite with carbonate of iron; at other times the lime is wanting, or present only in traces, and we have a ferruginous magnesite. In two previous notes presented to the Society, I have already explained the manner in which I suppose these siliceous carbonates to have been, in some parts of the formation, transformed into silicates, such as serpentine, talc, chlorite and pyroxene, by the subsequent intervention of heated solutions of alkaline carbonates.

It appears to me that we may explain the origin of these magnesian deposits, by the spontaneous evaporation of magnesian waters. If the waters of Carlsbad were to become stagnant above their deposited travertine, they would yield by evaporation beds of ferruginous dolomite, and waters like those of Caxton, Plantagenet, and Sainte-Genève would furnish carbonate of magnesia nearly free from lime. Nothing forbids us to suppose the existence of waters more highly charged than these with magnesian carbonate, formed perhaps by the action of carbonate of soda upon lagoons of seawater, whose lime may be removed as carbonate, or by previous evaporation as sulphate. The lagoons in Bessarabia, supplied with the waters of the Black Sea, deposit annually large beds of rock-salt; and it would require only the intervention of waters like those of the
natron lakes of Hungary and Egypt, to produce deposits of magnesian carbonate.

The conditions of these deposits at Pointe-Lévis and elsewhere in the Quebec group, seem to point to the existence of basins along an ancient sea-shore, which probably marked the first upheaval of the older Silurian strata. Beds of travertine were there formed, and then the sea flowed over these deposits and gave rise to the fossiliferous limestones; but there were intervals of disturbance, indicated by the conglomerates, and these movements, or the deposition of bars along the shore, gave rise to lagoons or basins cut off from the sea, where, by evaporation under the conditions which we have supposed, magnesian precipitates would be deposited. The absence of fossils in these beds is probably connected with the peculiar composition of the waters.

"A Fourth Memoir upon Quantics." By Arthur Cayley, Esq., F.R.S.

"A Fifth Memoir upon Quantics." By Arthur Cayley, Esq., F.R.S.

"On the Tangential of a Cubic." By Arthur Cayley, Esq., F.R.S.

"On the Constitution of the Essential Oil of Rue." By C. Greville Williams, Esq., Lecturer on Chemistry in the Normal College, Swansea.

The essential oil of rue and its products of decomposition have been examined by several chemists. Will analysed it many years ago, and deduced the formula $C_{20}H_{32}O_4$ as the result of his analyses. The principal investigation of it was made by Gerhardt, who regarded it as the aldehyde of capric acid. The production of capric acid from it by the action of nitric acid, as observed by Gerhardt and also by Cahours, has been considered as corroborative of the 20 carbon formula. It is evident, however, that the formation of capric acid merely indicates the aldehyde to contain _not less_ than 20 equivalents of carbon.

Some experiments made with a view to the production of certain new derivatives of capric aldehyde, led the author to believe the ideas generally entertained regarding the formula of the oil to be erroneous. Before continuing his experiments, he has therefore reinvestigated the nature of the oil itself.

In order to obtain the aldehyde in a state of purity, advantage was taken of the tendency of the aldehydes to combine with the alkaline bisulphites. The oil obtained from the ammoniacal bisulphite of the aldehyde was carefully analysed. The mean of eight very coincident analyses gave,—

\[
\begin{array}{c|c|c}
\text{Mean} & \text{Calculation} \\
\hline
\text{Carbon} & 77.71 & C^{22} \\
\text{Hydrogen} & 13.07 & H^{22} \\
\text{Oxygen} & 9.22 & O^2 \\
\hline
\text{100.00} & 170 & 100.00
\end{array}
\]
Mr. Williams on the Constitution of the Essential Oil of Rue. 381

The mean of two determinations of the density of the vapour gave,—

<table>
<thead>
<tr>
<th>Experiment (mean)</th>
<th>Theory C^{22}H^{22}O^{2} = 4 vols.</th>
</tr>
</thead>
<tbody>
<tr>
<td>5·870</td>
<td>5·874</td>
</tr>
</tbody>
</table>

The aldehyde, purified as above, was again converted into the ammoniacal bisulphite, from which the oil was a second time obtained. It gave on analysis,—

<table>
<thead>
<tr>
<th></th>
<th>Calculated.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbon...</td>
<td>77·67</td>
</tr>
<tr>
<td>Hydrogen.</td>
<td>12·93</td>
</tr>
<tr>
<td>Oxygen...</td>
<td>9·40</td>
</tr>
<tr>
<td>100·00</td>
<td></td>
</tr>
</tbody>
</table>

It is plain, therefore, that oil of rue contains an aldehyde of the formula C^{22}H^{22}O^{2}. Recent researches having demonstrated that no acid of the series C^{n}H^{n}O^{4} with 22 equivalents of carbon has yet been isolated, and no other derivative with a 22 carbon formula being known, the author has given the name enodyl to the radical homologous with acetyl contained in this substance.

Enodic aldehyde is a colourless fluid of a fruity odour, quite different to that of the rue plant. Its density is 0·8497 at 15°. Agitation will cause it to solidify at 7° into a snow-white mass resembling camphor. Its boiling-point is 213°.

Rue oil yields a small portion of fluid boiling at 232°, containing the aldehyde of lauric acid. It was not obtained absolutely free from the first fluid. It contained:—

<table>
<thead>
<tr>
<th>Experiment.</th>
<th>Calculation.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbon......</td>
<td>C^{24} 144</td>
</tr>
<tr>
<td>Hydrogen....</td>
<td>H^{24} 24</td>
</tr>
<tr>
<td>Oxygen......</td>
<td>O^{2} 16</td>
</tr>
<tr>
<td>100·00</td>
<td>184 100·00</td>
</tr>
</tbody>
</table>

The oils accompanying the aldehydes, but which refuse to combine with the alkaline bisulphites, are of the terebinthinate class. The more volatile are composed chiefly of an isomer of oil of turpentine; the less volatile are hydrates apparently homologous with an isomer of borneol.

March 25.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—


After describing the apparatus employed, and the process followed to determine the conductibility of metals and alloys, the authors give the chemical means by which they purified the metals used in the experiments. Taking silver, which is the best conductor, as 1000, they have obtained the relative conducting powers of the following metals:—

* In order to prevent oxidation of the oil, the balloons were filled with hydrogen previous to immersion in the bath.
Silver | 1000  
Gold, | 981  
Gold, | 840  
Copper, rolled | 845  
Copper, cast | 811  
Mercury | 677  
Aluminium | 665  
Zinc, forged | 641  
Zinc, cast vertically | 628  
Zinc, cast horizontally | 608  
Cadmium | 578  
Forged iron | 436  
Tin | 422  
Steel | 397  
Platinum | 379  
Sodium | 365  
Cast iron | 359  
Lead | 287  
Antimony, cast horizontally | 215  
Antimony, cast vertically | 192  
Bismuth | 61

The precision obtained by this process is such, that the authors were able to determine the different conducting powers of the same metal, when rolled or cast, as shown above. They were also able to appreciate the influence of crystallization on conductivity; for they found that the conducting power of a metal was different when it was cast horizontally or vertically, from the different directions which the axes of crystallization took under these circumstances.

The importance of having the metals as pure as the resources of chemistry allow, is shown by the action which one per cent. of impurity exerts on the conductivity of a metal, in some cases reducing it one-fifth or one-fourth. Copper alloyed with one per cent. of various metals gave different conducting powers, in the same manner as Mr. Thomson has shown that the conduction of electricity by the same metal is affected by a similar amount of impurities.

Alloying a metal with a non-metallic substance also exerts an influence, as is shown in the case of the combination of iron with carbon, thus—

Forged iron | 436  
Steel | 397  
Cast iron | 359

Similar results were obtained by combining small proportions of arsenic with copper.

The authors, with a view of ascertaining whether alloys are simple mixtures of metals, or definite compounds, made a large number of alloys of various metals, using equivalent proportions, and determined their conducting powers. The general result obtained is, that alloys may be classed under the following heads:

1st. Alloys which conduct heat in ratio with the relative equivalents of the metals composing them.

2nd. Alloys in which there is an excess of equivalents of the worse conducting metal over the number of equivalents of the better conductor, such as alloys composed of 1Cu and 2Sn; 1Cu and 3Sn; 1Cu and 4Sn, &c., and which present the curious and unexpected result that they conduct heat as if they did not contain a particle of the better conductor; the conducting power of such alloys being the same as if the square bar which was used in the experiments were entirely composed of the worse conducting metal.

3rd. Alloys composed of the same metals as the last class, but in
On the relative power of Metals to conduct Heat.

which the number of equivalents of the better conducting metal is greater than the number of equivalents of the worse conductor; for example, alloys composed of 1Sn 2Cu; 1Sn 3Cu; 1Sn 4Cu, &c.; in this case each alloy has its own arbitrary conducting power, and the conductibility of such an alloy gradually increases and tends towards the conducting power of the better conductor of the two metals composing the alloy.

Experiments were also made with bars composed of various metals soldered together, in order to compare the results obtained with alloys with those afforded by the same metals when mixed.

The first part of the paper concludes with the conducting power of several commercial brass alloys.

The second part, which will shortly be published, will contain the conduction of heat by amalgams.

"On the Surface which is the Envelope of Planes through the Points of an Ellipsoid at right angles to the Radii Vectores from the Centre." By Arthur Cayley, Esq., F.R.S.

The consideration of the surface in question was suggested to me some years ago by Professor Stokes; but it is proper to remark, that the curve which is the envelope of lines through the points of an ellipse at right angles to the radius vectors through the centre occurs incidentally in Tortolini's memoir "Sulle relazione," &c., Tortolini, vol. vi. pp. 433 to 466 (1855), see p. 461, where the equation is found to be

\[ 4(a^4 + b^4 - a^2b^2) - 3(a^2x^2 + b^2y^2) \]

\[ = 9a^2(2b^2 - a^2)x^2 + 9b^2(2a^2 - b^2)y^2 - 4(a^2 + b^2)(2a^2 - b^2)(2b^2 - a^2) \]

an equation which is obtained by equating to zero the discriminant of a quartic function. Tortolini remarks that this equation was first obtained by him in 1846 in the 'Raccolta Scientifica di Roma,' and he notices that the curve is known under the name of Talbot's curve.

According to my method, the equation of the curve is obtained by equating to zero the discriminant of a cubic function, and the equation of the surface is obtained by equating to zero the discriminant of a quartic function.

The paper contains a preparatory discussion of the curve, and the surface is then discussed in a similar manner, viz. by means of the equations

\[ x = X \left( 2 - \frac{1}{a^2} (X^2 + Y^2 + Z^2) \right) \]

\[ y = Y \left( 2 - \frac{1}{b^2} (X^2 + Y^2 + Z^2) \right) \]

\[ z = Z \left( 2 - \frac{1}{c^2} (X^2 + Y^2 + Z^2) \right) \]

which determine the coordinates \( x, y, z \) of a point on the surface in terms of \( X, Y, Z \), the coordinates of a point on the ellipsoid. The surface, which is one of the tenth order, is found to have nodal conics in each of the principal planes, and also a cuspidal curve. The case more particularly considered is that for which \( a^2 > 2b^2, b^2 > 2c^2 \), and
\[ a^2 + c^2 > 3b^2 \], and the memoir contains a figure showing the form of the surface for the case in question. The equation of the surface is obtained by the elimination of \( X, Y, Z \) between the above-mentioned equations and the equation \[ \frac{X^2}{a^2} + \frac{Y^2}{b^2} + \frac{Z^2}{c^2} = 1 \], as already remarked. This is reduced to the determination of the discriminant of a quartic function, and the equation of the surface is thus obtained under the form \( I^3 - 27J^2 = 0 \), where \( I \) and \( J \) are given functions of the coordinates.


The famous poison-tree of Madagascar was described for the first time by Aubert du Petit Thouars in his 'Genera Madagasarensia,' under the name of Tanghinia venenifera. At a later period, Sir W. Hooker published a good description, with a figure of this tree, named by him Cerbera Tanghin (see Botanical Magazine, pl. 2968); so that nothing is wanted with regard to the botanical knowledge of this plant. On the other hand, the physiological effects of its poisonous parts have not been hitherto investigated. All we know is, that the fruit of the Tanghinia is a strong poison, and is used in Madagascar as an ordeal poison in the most strange and revolting way. The only experiment on animals made by Ollivier, showed that 12 grains kill a dog in some hours; but this experiment gave no further insight into the real action of the Tanghinia. We hope, therefore, that the Royal Society will take some interest in the experiments which we undertook with this poison, of which the following is a short abstract.

The poison used by us was the alcoholic extract of the leaves and small stems of the Tanghinia, prepared from dried specimens, which Prof. Pelikan had received from Count Seyderitz of Mecklenburg. About one centigram of this extract was sufficient to show the full effect of the poison on frogs, when introduced into a wound of the back. It acted also when given by the mouth, but in this case a somewhat larger dose was required to produce a full effect.

The observed symptoms were the following:—

1. First of all, viz. in about 5 to 15 minutes, the heart was affected and stopped in its action, in such a way that the ventricle became contracted and very small, whilst the auricles remained dilated, but were also paralysed.

2. The voluntary and reflex movements were at first not at all affected; but some time—from half an hour to one hour—after the paralysis of the heart, they became weaker and weaker, and gradually ceased totally without any sign of spasms or tetanus.

3. In the third place, the Tanghinia has a great influence upon the voluntary muscles, which become paralysed. This action begins very soon, and we have been able to show, with the aid of the myographion of Volkman, that as soon as the heart is paralysed, the muscles also begin to lose their force. Nevertheless, the total paralysis of
these organs is not observed till after six hours and more, that is to say, when the muscles have been preserved in a temperature of 14° to 16° R. In a temperature of 4° to 6° R., the irritability of the poisoned muscles may last for double this time, as is usual with all poisoned muscles and nerves; but even in this case it disappears long before that of the non-affected muscles.

4. If muscles which have lost their irritability through the Tanghinia are put into a solution of common salt of from ½ to 1 per cent., their power of contraction reappears after a certain time, but only when they have been preserved at the lower temperature of 3° to 6° R.

5. Lastly, the nerves also are paralyzed by the Tanghinia, and, as far as we were able to pursue this question, under the same circumstances as the muscles, only perhaps a little earlier.

From all this it follows that the Tanghinia is a paralyzing, and above all, a muscular poison. As far as we have been able to follow its action, it resembles very much, the Upas Antiar; only its power would seem to be a little less strong.

April 22.—Major-General Sabine, Treasurer and V.P., in the Chair.

The following communication was read:—

"On the Differential Stethophone, and some new Phenomena observed by it." By S. Scott Alison, M.D.

Engaged for some years in investigations into the phenomena of audition, I have become cognizant of some facts which I believe have hitherto remained unnoticed, and which are certainly not generally known to physicists and physiologists.

The first of which I shall treat is the restriction of hearing external sounds of the same character to one ear, when the intensity is moderately, yet decidedly greater in one ear than in the other, the hearing being limited to that ear into which the sound is poured in greater intensity. The sound is heard alternately in one ear and in the other, as it is conveyed in increasing degrees of intensity, and hearing is suspended alternately in one ear and in the other, as the sound is conveyed in lessening degrees of intensity.

Sound, as is well known, if applied to both ears in equal intensity, is heard in both ears; but it will be found, if the intensity in respect to one ear be moderately yet decidedly increased, by bringing the sounding body nearer that ear than the other, or otherwise, as by the employment, in respect to one ear, of a damper or obstructor of sound, or in respect of the other ear, by the employment of some intensifier, or good collector or conductor of sound, the sound is heard in that ear only which is favoured and has the advantage of greater intensity.

There is little doubt that this law holds with regard to sounds passing through the air, and carried to the ear in the ordinary manner, without the aid of any mechanical contrivance, as for instance those of a watch placed in front of the face; but as the restriction of hearing to one ear, and its suppression in the other, admit of being rendered more obvious by an apparatus that shall collect sound, prevent its diffusion through the air, and carry it..."
direct to the ear, I propose to give the results of experiments made with an instrument which I have invented for hearing with both ears respectively, and which, as it is specially adapted for the auscultation of differences in the sounds of different parts of the chest, I have named the Differential Stethoscope, or Stethophone.

The results thus procured will be more satisfactory than those obtained by ordinary audition; a sound will be increased as a visual object is magnified by the microscope, and as both ears are similarly dealt with, a perfect parity of conditions will hold in respect of both ears.

The differential stethoscope (see figure) is simply an instrument consisting of two hearing-tubes, or trumpets, or stethoscopes, provided with collecting-cups and ear-knobs, one for each ear respectively. The two tubes are, for convenience, mechanically combined, but may be said to be acoustically separate, as care is taken that the sound, once admitted into one tube, is not communicated to the other. The tubes are composed of two parts nearly equal in length, one near the ear-knob, made of metal (C); while the other part, near the collecting-cup, is made of metal wire (B), to impart flexibility. The ear-end is curved, so as to approach the ear, and is supplied with an ivory knob (D) for insertion into the meatus externus. The other end of the tube, being intended to collect sound, is supplied with a hollow cup, or receiver (A) made of wood, or some such material.

The mechanical construction of this instrument is borrowed from the stethoscope contrived by Dr. Caman of New York, and intended by its inventor for the purpose of hearing with both ears sounds emanating from one point; and collected into one cup. The two tubes are brought near together, a few inches in front of the face, by means of a connecting-bar (E), but calculated to prevent the transmission of sound from one tube to the other. This bar is supplied with a joint, which permits the tubes to be freely moved, as is necessary in applying the knobs to the ears. The two knobs are kept steadily in the ears by means of an elastic band (F) connecting the two tubes near the bar, already described.

The instrument being fitted into the ears, with the knobs directed upwards, and the cups being applied equally near to, or upon a sounding body, say the inflating lung, or a watch, and the conditions for collecting sound being the same, the sound is heard with
both ears, as in ordinary hearing. But if one cup be removed a little, say a half or a quarter of an inch from the watch (for we shall now adopt it), and the other cup be left upon the watch, the sound is heard with that ear only which is connected with the cup placed upon the watch, and the sensation in the hearing ear is so marked, as to leave the mind in no doubt whatever that it is through that ear we become conscious of the sound. If the cup placed upon or nearer the watch, be removed a little further than the other cup, so as to be less favourably situated for collecting sound, say one inch from the watch, the ear connected with it becomes totally unconscious of sound, and the sensation of hearing is most unequivocally felt in the ear (and in that ear only) which but a moment before was utterly deaf to it. If one cup be placed upon the middle of the watch, and the other on the edge, the watch sound is heard in that ear only which is connected with the cup placed upon the middle.

These experiments may be thus varied; and the result will in reality be the same, though apparently more remarkable. The watch, being held in the air, at the distance of about an inch from one ear, is heard distinctly beating into that ear only; but if the watch be now connected with the collecting-cup of the tube of the stethoscope, inserted into the other ear, the sound, being greatly magnified, is heard in this ear, and in it only, the ear in which the sound had been primarily heard being now altogether insensible to it, or unaffected by it as far as our consciousness is concerned. The sensation of sound is transferred from one ear to the other, although the watch is allowed to remain in close proximity to the ear that is now deaf to its sound.

A watch placed upon or inside the cheek, is heard to beat in that ear which is nearer; but if the opposite ear be connected with it by means of one of the arms of the stethoscope, or by a common flexible stethoscope, the watch sound is no longer heard in the ear nearer the watch, but in the ear further from it, which is now in reality brought into nearer connexion with it, by means of the hollow tube.

Sounds, produced in whatever material, are alike subject to this law, so far as my experiments have yet been made.

The medium in which sounds are produced does not alter this law. A watch ticking, or a bell ringing, either in the air or under water, affords the same results.

Sounding bodies give the same results when covered with soft or hard materials. A watch placed in one corner of a box, a few inches square, and an inch deep, is heard to beat in that arm of the stethoscope only which is near it. By this means, and by successive movements of the instrument, and by attending to degrees of intensity, the exact position of the watch may be with certainty indicated. Or this may be effected by successively excluding those parts which fail to cause hearing in one of the ears.

The interposition of a body calculated to obstruct the sound at its entrance into one of the cups of the stethoscope, causes the sound to be heard in that ear only which is connected with the cup which remains free from obstruction. This admits of ready proof, by
applying the two cups as much as possible equally on the middle of a watch about an inch above it, and by placing two fingers held together between one cup and the watch. When this is done, the watch is heard to tick into the ear that remains free from obstruction.

The removal of an obstructing body from one cup, while it is allowed to remain in operation with the other, causes sound which had been equally heard with both ears, to be heard in that one only which is connected with the cup freed from the obstructing body. Thus, if the fingers be interposed between the watch and the cups held equally over it, and the fingers be separated under one of the cups, so as to permit of atmospheric communication, the sound is heard in that ear only which is connected with this cup, and not at all in the other.

The effect of intensification of a sound in one ear depriving the other ear of all sensation of that sound, is interestingly shown by placing the tubes of the instrument across a block of wood with the cups hanging in the air. While both cups are left open, and a tuning-fork in vibration is placed between the two tubes, the sound is heard with both ears; but if one cup be closed with the hand, or with leather, and the other be left open, the sensation of sound is restricted to that ear connected with the closed cup. The sound in the tube connected with the closed cup is rendered more intense by the closure, the escape of sound is obstructed, and reverberation takes place. By virtue of the intensification, sensation is monopolized by one ear, and is lost in the other. The result and the mechanical conditions are much the same as in the experiments of Mr. Wheatstone with a tuning-fork held upon the head, presently to be referred to.

It is worthy of observation, that in order that a sound previously heard with or in both ears, as in the above experiments, may be appreciated or felt in one ear only, it is not necessary that the stethophone, or other conducting instrument, be placed in the cavity of the meatus externus. It is sufficient for this result that the instrument be placed near the meatus, so as to give it an advantage of intensity over the opposite cavity. When the instrument is to be held only near the meatus, care should be taken not to touch the external ear, so that there may be no conduction by that part, from contact, which would vitiate the experiment. The result is perfectly satisfactory and conclusive, although the remarkable sensation of pouring in of sound into the ear is less marked,—a fact sufficiently intelligible from the diffusion of sound which must take place outside the ear, when the extremity of the tube is held there, and is not inserted into the meatus. It is therefore obvious that the restriction of hearing to one ear, under the conditions specified, is not due to closure of the meatus externus, the cause of the augmentation of sound in some experiments of Mr. Wheatstone, to be shortly referred to.

The remarkable phenomenon of the restriction of hearing to one ear, above described, seems not to be without important signification. It holds apparently in virtue of a law seemingly established for the purpose of enabling man and the lower animals to determine the
direction of the same sound, with more accuracy than could be done
had a judgment to be formed between the intensity of two similar
sensations in the two ears respectively. All source of error is
removed by there being only one sensation, although there may be
two impressions. This law of a stronger impression in one ear,
rendering us unconscious of a weaker but similar impression in the
other, has an analogue, though perhaps an imperfect one, in the
sense of touch. Very strong impressions upon one part of the
body cause such acute sensations, that minor impressions of the
same kind upon another part are frequently not felt, in fact, produce
no sensation.

The only observations bearing upon this law which I have been
able to discover, are some by Mr. Wheatstone, in a paper entitled
"Experiments on Audition," published in the Quarterly Journal of
Science, Art, and Literature, vol. ii. New Series, 1827. These expe-
riments are intended to show the augmentation which the sensation
of autophonic sound, and the sounds of a tuning-fork applied to the
head, acquires when the ear is closed, although the perception of
external sounds is diminished. Mr. Wheatstone shows that a vocal
sound is heard louder in that ear that is closed, say with the finger,
than in the other. He also shows, that the sound of a tuning-fork
placed upon the head is heard louder in that ear which is closed than
in the other which remains open, even though the tuning-fork may
be brought nearer the open ear than the closed one. These experi-
ments, Mr. Wheatstone says, prove that "sounds immediately com-
municated to the closed meatus externus are very greatly magni-
ified;" and he adds, "it is an obvious inference, that if external
sounds can be communicated to act on the cavity in a similar man-
ner, they must receive a corresponding augmentation."

This distinguished philosopher constructed the instrument named
a Microphone, for the purpose of augmenting weak sounds, upon this
principle, i.e. the augmentation of sound by closure of the ears;
and he informs us that it "is calculated for hearing sounds when it
is in immediate contact with sonorous bodies," and that "when they
are diffused by their transmission through the air; this instrument
will not afford the slightest assistance." This instrument is spoken
of in connexion with the augmentation of sound, and not in reference
to the limitation of sound to one ear, or to the comparison of sensa-
tions in the two ears. The remarkable, and, to the uninitiated mind,
the wonderful fact, made known more than thirty years ago by
Mr. Wheatstone, that a tuning-fork held upon the head close to an
open ear is not heard in this ear, but in the opposite ear, provided it
be closed with the finger, or by some other means, proved that
sounds communicated to the skull were exclusively heard in the
closed ear. In the case of the tuning-fork, the fact made known by
Mr. Wheatstone is undoubted. The rationale of the phenomenon
appears to be this:—The vibrations of the tuning-fork are commu-
nicated to the bones of the head, and through them to the ears
including their bones, cartilages, and contained air; but in the case
of the closed ear, the vibrations are permitted no egress or escape
as in the open ear; reverberations take place, and the consequence is, that the sound is not duly moderated; and in virtue of the law I have just enunciated, the sensation of sound is restricted to the closed ear. When the tuning-fork, duly sounding, is held in the air, and not connected directly with the head, the closed ear remains insensible to it, and the sound is heard exclusively in the open ear.

Mr. Wheatstone's interesting observation relates to a head-sound not duly moderated, as in the opposite and open ear, and virtually more intense, and comes within the general law advanced in this paper, which embraces all sounds, whether internal or external, viz. that a sound of the same character in the presence of both ears, if conveyed by any means to one ear, or to the nerve of that ear, more intensely than to the other, is heard in the more favoured ear only.

It seems necessary, in Mr. Wheatstone's experiments, that the bones of the head shall vibrate freely; weak sounds, such as gentle blowing, will not succeed; and if the tuning-fork be placed immediately under the open ear, and passed upon the soft parts (little fitted for vibration) between the mastoid process of the temporal bone and the lower jaw, the sound is heard in this ear, and not in the closed ear.

It may perhaps be well, before proceeding further, to acknowledge that I am well aware it has been long known that a very loud sound conveyed into one ear will render the other ear insensible to sound of a weak or low character. But the phenomenon which I have ventured to bring under the consideration of the Royal Society differs from this well-known and readily admitted fact in this important particular, that no very great loudness is required, and that no very great augmentation of sound in one ear over that in the other is necessary in order to restrict the sense of hearing to one ear, and to deprive the less favoured ear of the sense of hearing which it had previously enjoyed. A moderate, yet a decided increase of intensity is all that is required to remove the sense of hearing from the less favoured ear, and to cause the more favoured organ to be alone sensible to the sound.

When sound is proceeding into the two ears, but in consequence of its reaching one ear in greater intensity than the other is heard only in one ear, the sensation of hearing in the favoured ear, though strictly limited to it, is augmented by the sound entering the less favoured ear, although it entirely fails to cause a sensation there, or to produce a consciousness of sound in that organ. The more sound collected by the less favoured ear, as long as the amount is less than that conveyed to the other ear, the more the sensation of sound is augmented in the more favoured ear. The intensity of sensation in the more favoured ear increases in a ratio with the increase of sound in the less favoured ear, until the intensity of sound is the same, or nearly the same, in both ears, when the sensation experienced is the ordinary one of hearing with two ears.

This fact admits of satisfactory proof in this way:—A watch is placed on a table equidistant from both ears. The stethoscope is applied to the ears; one cup is placed within an inch of the watch, while the other is turned away from it, at the distance of some
Dr. Alison on the Differential Stethophone. 391

inches. As the further cup is brought nearer and nearer the watch, the sound, always confined to the more favoured ear, is gradually and steadily intensified, until the two cups are, or are about to be, similarly placed, at which moment the sensation ceases to be restricted to one ear, and has acquired its greatest intensity. This fact proves, that though the sensation of hearing be confined to the ear to which sound is communicated with greater intensity, we profit by the sound which is conveyed into the other ear, though failing to produce a sensation or a consciousness of sound there, by its serving to augment very materially the sensation of sound in the more favoured ear. The less favoured ear thus augments the sensation which we experience, at the same time that it fails to interfere with the aid which the sensation confined to one ear affords us as to the direction of external sounds.

The sounds of which we have been treating as differently affecting the two ears, according to the intensity with which they are respectively communicated, are of the same character, though differing in intensity. It is sounds of the same character only which exhibit the phenomenon of restriction in virtue of moderately different intensity. The sounds must emanate from the same sounding body, or from bodies sounding similarly. A little difference in character will cause the experiment of restriction to fail.

Thus, if two bells, differing considerably in character, be rung respectively in the two ears, one louder and graver than the other, the louder and graver sound does not render the other ear insensible to the weaker sound of the weaker bell. Both ears hear perfectly, but the loud, grave sound is heard in one ear, and the weak sound is heard in the other.

If, instead of one watch, we place two together, having sounds of different character, as for instance one low and grave, and another loud and sharp, and the two arms of the stethophone be placed over them respectively, the sounds of both watches are heard, but the sound of one is heard in one ear, and the sound of the other is heard in the other ear. The loudness of the sound in one ear does not increase the weakness of the sound in the other; or, in other words, the intensity of the sensation produced by the weak watch in the one ear is not reduced by the sensation produced by the loud watch in the other ear.

The sound of a watch ticking continues to be heard in one ear, although a large-sized bell is made to ring at the other; and I have not perceived that the sensation produced by the watch is at all impaired by the bell. A whistling lung-sound heard in one ear, is not rendered less obvious by a loud blowing lung-sound in the other. A hissing murmur at the apex of the heart conveyed into one ear, and a rasping sound at the base conveyed into the other, are both heard without alteration in the ears to which they are respectively conveyed.

By virtue of these two laws—1st, that sounds of the same character are restricted to that ear into which they are conveyed in greater intensity, and 2nd, that sounds differing in character may
be heard at the same time in the two ears respectively, even if they
be made to reach the ears in different degrees of intensity—it is pos-
sible to analyse a compound sound, or one composed of two sounds,
and to divide it into its component parts. In order to effect a divi-
sion of a compound sound, it is only necessary that the two sounds
of which it is composed may respectively be heard at certain points,
in greater and lesser intensity, and that the respective cups of the
stethophone be placed at these points. The ear connected with the
cup placed where one half of the sound is in greater intensity, hears
that half sound only, and the ear connected with the cup placed
where the other half of the sound is in greater intensity, hears that
half sound only. The sound is divided into two parts; and one is
heard in one ear, and the other part in the other ear. For example,
a compound sound composed of the two sounds of two watches
placed together upon a table, with the unassisted ear is distinctly
heard in its compound state, and cannot be divided into its
two constituent parts. With the stethophone this is readily done.
One cup is placed where the sound of one watch is in greater in-
tensity, and the other is placed where the sound of the other watch
is in greater intensity; and the result is obtained of one watch only
ticking in one ear, and of the other watch only ticking in the other
ear. The greater intensity of each watch-sound in one ear has ren-
dered all hearing of it in the other ear impossible, and as each watch-
sound in its greater intensity is conveyed to different ears, one is
heard in one ear only, while the other is heard in the other ear
only. Without the stethophone, or some such instrument, this
analysis could not be made: the ordinary stethoscope will not suc-
cceed; for wherever it is placed it conveys the mixed or compound
sound to the ear. If the naked ear be applied over or upon the
watches, the same result follows; and it is the same if instead of two
arms of the stethophone we employ only one. This remarkable
separation of the components of a sound may be effected also when
the sounding bodies are enclosed in a box capable of transmitting
sound, or when separated from us by the interposition of materials
capable of conducting sound; and by successive trials and comparisons
of intensity at different places, and by a process of exclusion of those
parts which fail to cause sensation, the respective positions of two
adjacent sounding bodies may be predicated. If, for example, we have
two watches, A and B, enclosed in a box, and through one cup, A,
we hear watch A, and with the other cup, B, we hear watch B, we
may conclude that cup A is nearer watch A than cup B is, and so on.
In the same manner we may auscultate the morbid sounds of the heart.
By cup A, placed at the apex, and cup B placed at the base, we hear
separately the morbid sounds of the two parts,—for example, a blow-
ing murmur at the apex in one ear, and a rasping murmur at the
base in the other ear. This we are enabled to do, although at any
intermediate point with the single ear, either with or without a
stethoscope, we hear the conjoined two sounds. It is obvious that
with the stethoscope we not only succeed in separating sound, but
that this instrument, or some similar contrivance, affords the only
possible means of hearing, with two ears at once, sounds emanating from the same region or surface, for the sides of the head can be applied, of course, to the same sounding surface only in turn or succession. With this instrument we, as it were, place our ears in our hands, apply them where we choose, and listen with them both at adjacent or distant points of the same surface, at one and the same instant of time.

It is not unlikely that the property which the stethophone possesses of pointing out with precision where sound is most intense, may be very usefully employed. It seems possible that it might be turned to account in discovering the points where operations in military mining may be going on.

It is, however, in the practice of medicine only that the differential stethophone has been hitherto applied; and it may be here permitted to me to point to some of the chief purposes for which it is adapted, and for which it has been employed.

In respect to respiration, we may compare at once, and without the inconvenience of moving the head, or the ordinary stethoscope, from place to place, the extent of the respiratory sounds in different parts, so that a very minute difference, an excess in one part or a deficiency in another, may with certainty be discovered. Differences in quality, such as softness or roughness, are readily recognized. The increased length and loudness in one part is accurately contrasted with the healthy conditions of another part. In cases where the inspiration has been very full in one place, in order to compensate for deficiency in another place, and where the expiration was long and coarse in the deficient part, I have heard the inspiratory sound only in one ear, and the expiratory sound in the other ear. The sounds were respectively restricted to the two parts; and they alternated in a very marked manner. One part has remained silent while the other has been heard to sound; and this has been silenced when the other has awaked the ear.

The diagram represents the sounds occurring alternately in two sides of the chest in a consumptive patient. The dark spots represent the sounds.

<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Inspir. 1.</td>
<td>Inspir. 1.</td>
</tr>
<tr>
<td>Expir. 1.</td>
<td>Expir. 1.</td>
</tr>
<tr>
<td>Inspir. 2.</td>
<td>Inspir. 2.</td>
</tr>
<tr>
<td>Expir. 2.</td>
<td>Expir. 2.</td>
</tr>
</tbody>
</table>

The influence which the acts of respiration exert in heightening and lowering the murmurs in veins, say of the neck, in persons affected with a thin and watery condition of blood, is well exhibited.
by placing one arm of the stethophone on the chest and the other upon the veins.

When the respiration in two parts is alike in character, but decidedly louder in one part than in another, the sound in the weak side is lost. While this loss proves, in a very emphatic manner, the important fact of deficiency; it of course for the time deprives us of the opportunity of judging of the quality of the deficient inspiration: but this is readily obviated by removing the cup of the instrument from the full respiring part; and then the deficient respiration is immediately heard through the other cup. Thus, while the two sounds, being of like character, and one being more intense than the other, can be heard only in one ear at the same time, an admirable opportunity is obtained for contrasting the extent, and some of the qualities, of the sounds of the two parts, by placing the cups alternately and rapidly upon the two spots respectively. Vocal extusive resonance in two parts of the thorax, is well contrasted with the two tubes employed at once, or in immediate succession.

The sounds of the two sides of the heart, and of the valves of the two great arteries proceeding from that organ, are, by means of the stethophone, very advantageously dealt with. By placing it over the two sides of the heart, or the origin of the two arteries, we ascertain the character and loudness of the sounds of these parts. One cup being placed over the aorta, and the other over the pulmonary artery, if the sounds they collect differ in character, one sound is heard in one ear, and another in the other ear. We may have at the same moment an aortic murmur and a healthy pulmonary artery sound, one sound in one ear, and another sound in the other ear. But when it is desired to listen to each sound singly and in succession, the instrument will still be available; for the cups may be applied singly and in succession, thus affording ample means for contrast.

In cases of disordered heart, in which it is desired to discover whether the sounds of the two sides of the heart are synchronous, the stethophone affords the most satisfactory mode of investigating the fact. With it, we virtually place our two ears over the two sides of the heart; and if one side sounds at all after the other, the fact is made known, and the end of one sound and the beginning of another are clearly and distinctly defined. With the ordinary stethoscope this is impossible; for where one sound is heard, the other may be inaudible, and long before the head or stethoscope can possibly be adjusted at another part, the second sound has taken place, and is long since over.

In conclusion, I may perhaps be permitted to say that the differential stethoscope proves a great auxiliary in examining the heart with the cardioscope or sphygmoscope, which I had the honour to exhibit to this Society two years ago. While the latter instrument exhibits the movements of the heart, the stethoscope informs us of their sounds, in a more complete manner than can be otherwise effected; and from the stethoscope permitting of auscultating two parts at once, and with the eyes directed to the chest, the relation of the movements and of the sounds, normal or abnormal, of this most important organ is very fully and satisfactorily made out.
Postscript.

In connexion with that part of my paper which treats of the restriction of hearing to the closed ear, I desire to add the fact which I have ascertained within the last few days, that if one ear be closed wholly or partially at its external part, i.e. at the meatus externus, by disease or by congenital malformation, while the other ear is healthy, the sound of the tuning-fork, applied to any part of the head, is heard only in the closed ear. This fact holds, although the closed ear is totally unaffected by sounds conveyed through the external air.

I have further to mention the fact, that all persons, deaf in one ear, whom I have lately examined, with one exception, hear the sound of the tuning-fork applied to the head in that ear only that is deaf to external sounds. A man who has been totally deaf in one ear for thirty years, in consequence of a violent blow upon the head, had the tuning-fork applied over the forehead. He started, and said that he heard only in the ear which had been deaf during that long course of time. In such cases I have been disposed to believe that, amidst other lesions of the organ of hearing, there may be present an obstruction or closure, that a reverberation takes place, and that thus a restriction of hearing is secured for the diseased organ.

---

Geological Society.

[Continued from p. 238.]

June 23, 1858.—Prof. Phillips, President, in the Chair.

The following communications were read:—

1. "On some points in the History and Formation of Etna." By Dr. H. Abich, For. Memb. G.S. (In a letter to Sir C. Lyell, F.G.S.) In this letter the author explained his views of the stratification of the rocks in the Val di Bove, of the distribution, nature, and effects of the dykes, and the different characters of the lavas of successive ages in Etna, and stated his belief that the Val di Bove had originated in subsidence; and, pointing out the value of the peculiar outline of the mountain as illustrative of the double-coned origin of Etna, he described some dykes and rents that have been connected with the catastrophe, followed by the formation of the Val di Bove, which gave the chief features to Etna in its modern form.

2. "On the Lacustrine or Karéwah Deposits of Kashmir." By H. H. Godwin-Austen, Lieut. H.M. 24th Reg., Kashmir Survey. Communicated by R. Godwin-Austen, Esq., F.G.S. The author, in going up the Jhelum to Baramula, observed great quantities of granitic boulders at Kuthai, Oorie, and Gingle, where the river-valley widens out after having been narrowed by gorges. These blocks occur throughout the whole thickness of the Alluvium, sometimes upwards of 200 feet, as seen in the sections where the nullahs cut through it. Mr. Vigne had supposed that these granite-boulders were originally brought across the Valley of Kashmir from Haramuk by ice or some other transporting agency, but the author
observed no trace of such fragments along the supposed line of transport, except at the isolated localities above-mentioned. In pursuing his journey up the Jhelum by Baramula and Sopur through North Kashmir to the North Punjab Range, which he was occupied in surveying, the author was enabled to study the nature of the great alluvial deposits of Kashmir, constituting a formation of great thickness, and of which the Karewah Hills, sometimes upwards of 300 feet in height, are formed. This alluvium or "Karewah formation" is believed by Mr. H. Godwin-Austen to be purely of lacustrine origin. He found that it is always composed of detritus of local origin, containing granitic, basaltic, or calcareous fragments, according to the nature of the mountains in the background, from which the materials had been derived; and that the coarser constituents lie near the mountains, whilst finer and finer detritus is discernible in the beds towards the plains. Subsequently examining the heights above the Jhelum outside the Baramula Pass, the author found the granitic rocks in place from whence the granite-boulders of the valley-deposits at Kuthai, Oorie, and Gingle had been derived. A journey through the Valley of Kashmir, past the old buried city, and by Islamabad and Shahbad, to survey the country traversed by the Chunab and Kishtwar Rivers, and overlooked by the Brahma Mountain, 22,000 feet high, still further illustrated the author's views of the local origin of the great alluvial or lacustrine deposits of the country,—which, whether formed in the outer and smaller basins, or in the great Kashmir Valley, appear to have been the slow result of atmospheric agencies, operating on this very ancient land, from the time of its first exposure as a highly dislocated tract of tertiary and secondary strata entangled in an irregular trough or basin of crystalline and granitic rocks, until the period when the gradual disintegration of the surface had filled up the step-like cavities with local lacustrine deposits. Subsequently the drainage of the country has not only shaped the Karewah Hills out of these sediments, but has cut through these deposits, often deep into the underlying rock, and, clearing out the gravels and boulders from the choked gorges of the Jhelum at Baramula, has reduced the waters of the old lake of Kashmir to its present narrow limits. Hence the buried condition of the old city and its temple, and other local phenomena, may be accounted for, without recourse being had to the supposition of successive subsidences and upheavals which has been sometimes advanced.


The black mica accompanying the white margarodite of the Leinster granite, similar mica at Ballyellin, Carlow, and the black mica found in the Poisonglen, leading to the pass of Ballygihen, in Donegal, have been carefully examined by the author, and he regards the black mica of Donegal as certainly identical with that of Carlow and Leinster, and probably the same as the black mica from Petersberg, Wermland, described as Lepidomelane by Soltmann.
4. "On an Outlier of Lias in Banffshire." By T. F. Jamieson, Esq. In a letter to Sir R. I. Murchison, V.P.G.S.

In a cutting of the Banff and Turriff Railway, about four miles to the north of Turriff, there has been exposed a thick mass of tenacious blue clay, containing Ammonites, Belemnites, Gryphaea, Plagiostomatata, and other fossils of Liassic character.

The author explained his reasons for regarding this clay as being a fragment of the Lias in situ, and noticed the interest belonging to it as being perhaps the most eastern Liassic outlier in Scotland.


By the examination of a series of mammalian fossils sent from the Condamine River and Darling Downs, and now in the Worcester Museum, and of casts of the cranium, upper jaw, and teeth of Macleay’s "Zygomaurus," communicated by the Trustees of the Sydney Museum, Professor Owen has been able to demonstrate that this cranium belongs, as he suggested in a paper lately read before the Society, to his genus Nototherium, and to the species which he had dedicated to the late Sir T. Mitchell. A smaller species, provisionally named Nototherium inerme, was also established by Professor Owen on some of the specimens examined; but he thinks it not improbable that with additional materials it might be found that these two forms may represent the male and female of one species.


These fossils were found by Mr. Brown in a small exposure of sand and clay beds, in a garden on a hill-side in the parish of Chislet, Kent. The beds would appear, according to Mr. Prestwich’s sections of that county, to belong to his "Lower London Tertiaries;" but of the 36 species of Shells, Cirripeds, and Foraminifera met with—13 are forms found also in the Crag; 9 are English Lower Tertiary forms; 2 are Belgian Tertiary forms; and 4 are new species.

7. "On the Fossil Crustacean found by Mr. Kirkby in the Magnesian Limestone of Durham, and on a new species of Amphipod." By Spence Bate, Esq. Communicated by Dr. Falconer, F.G.S.

In this paper Mr. Bate described a new recent Amphipodous Crustacean, which he believes to represent some of the fossil crustacean remains lately described by Mr. Kirkby in the Society’s Journal, under the name of Prosoponiscus problematicus.


The author gave a history of what is known about this genus,—described several new or little known species,—and observed that the range of the genus is confined to the Ludlow, Devonian, and Lower Carboniferous strata. The new species described were—Eurypterus Symondsii, E. pygmaeus, E. megalops, E. acuminatus, E. linearis, E. abbreviatus, and E. Aquila-chatracea. E. Scouleri, Hib-
Intelligence and Miscellaneous Articles.

9. "Description of a New Fossil Crustacean from the Lower Greensand of Atherfield." By Charles Gould, Esq. Communicated by Professor Huxley, F.G.S.

This is a macrourous crustacean, of rare occurrence, named by the author *Mithracites Vectensis*, and is related to the equally rare *Mithracia* of Bell (tertiary).

ON THE RELATIONS BETWEEN THE TENSIONS AND TEMPERATURES OF VAPOURS. BY J. A. GROSHANS.

ON comparing the numbers in the Table calculated by Regnault from his experiments on the tension of aqueous vapour at different temperatures, I have found the following formula,—

\[
\frac{t^a-100}{t^b-100} = \frac{\sqrt{a} - \sqrt{760}}{\sqrt{b} - \sqrt{760}}
\]

(1)

in which \(a\) and \(b\) are two tensions expressed in millimetres, and \(t^a\) and \(t^b\) the corresponding temperatures.

To see how this formula would agree with Regnault’s results, I calculated the following Table:

<table>
<thead>
<tr>
<th>(t^b)</th>
<th>Tension (b)</th>
<th>(t^a) calculated</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>4.600</td>
<td>121.36</td>
</tr>
<tr>
<td>+25</td>
<td>23.550</td>
<td>120.89</td>
</tr>
<tr>
<td>+50</td>
<td>91.982</td>
<td>120.63</td>
</tr>
<tr>
<td>75</td>
<td>288.517</td>
<td>120.54</td>
</tr>
<tr>
<td>95</td>
<td>633.788</td>
<td>120.59</td>
</tr>
<tr>
<td>105</td>
<td>906.41</td>
<td>120.59</td>
</tr>
<tr>
<td>120</td>
<td>1491.28</td>
<td>120.61</td>
</tr>
<tr>
<td>121</td>
<td>1539.25</td>
<td>120.60</td>
</tr>
<tr>
<td>122</td>
<td>1588.47</td>
<td>120.64</td>
</tr>
<tr>
<td>150</td>
<td>3581.23</td>
<td>120.77</td>
</tr>
<tr>
<td>175</td>
<td>6717.43</td>
<td>120.97</td>
</tr>
<tr>
<td>200</td>
<td>11688.96</td>
<td>121.22</td>
</tr>
<tr>
<td>230</td>
<td>20926.40</td>
<td>121.13</td>
</tr>
</tbody>
</table>

Average ............... 120.81

The numbers \(t^b\) and \(b\) are derived directly from the Table; \(a\) is always equal to two atmospheres = 1520 millims.

The numbers 100 and 760 in the formula might of course be replaced by any numbers \(t^c\) and \(c\) taken at pleasure from Regnault’s Table.

If we take, for example, \(t^a=150\), \(t^b=200\), and \(a\) and \(b\) from the Table, place these values in the formula (1), make \(t^c=0\), and solve the equation as regards \(c\), we find \(c=4.221\) millims. Regnault's Table gives 4.600 millims, for the tension of aqueous vapour at 0° C.
If, on the other hand, we make \( c = 0 \)° C., whilst \( a, b, t^a \) and \( t^b \) remain unaltered, we find \( t^c = -79.39^* \); this last number is therefore the lowest limit of evaporation of water.

The formula (1) may therefore be converted into the following:

\[
\frac{t^a + 79}{t + 79} = \frac{\sqrt{a}}{\sqrt{b}}.
\]

(2)

The temperatures (counted from the lowest limit of evaporation) are consequently related as the sixth roots of the pressures, or the pressures stand in the proportion of the sixth powers of the temperatures.

In 1849 I found the formula

\[
\frac{273 + 100}{273 + e} = \frac{273 + T}{273 + t}.
\]

(3)

which expresses that, if all temperatures be counted from \(-273^0\) C., all the corresponding temperatures for any two fluids are proportional.

(100 is the boiling-point of water or of any fluid; \( T \) and \( t \) are two corresponding temperatures of water and the other fluid.)

This formula I have been able to verify from the experiments of Regnault and Faraday, for alcohol, carbonic acid, and nitrous acid gas; other fluids, such as ether, but especially sulphuret of carbon, exhibit considerable differences in opposition to the formula; I shall hereafter publish the results of my calculations upon them.

If we assume that the formulæ (3), (1), and (2) are in general applicable to all fluids, and indicate by \( t^c \) the lowest limit of evaporation of a fluid boiling at \( e \), this limit may be calculated by the formula

\[
t^c = -273 + \frac{273 + e}{273 + 100} (273 - 79).
\]

(4)

The formulæ (1) and (2) then become

\[
\frac{t^a - e}{t^b - e} = \frac{a^0 - 760.1}{b^0 - 760.1},
\]

(5)

and

\[
\frac{t^a - t^c}{t^b - t^c} = \frac{a^0}{b^0}.
\]

(6)

Poggendorff’s Annalen, 1858, No. 8. p. 651.

ON INTERMITTENT FLUORESCENCE. BY J. MÜLLER.

In a strip of paper which was washed over with a solution of platinocyanide of barium, so that after the evaporation of the water the paper was covered with a layer of delicate greenish crystals, I observed the following singular phenomenon, which I may characterize as intermittent fluorescence. The spectrum produced in a dark room by a flint-glass prism was projected by means of a

* Of course somewhat different values are found if \( a \) and \( b \) be altered; for \( a = 25^0 \) and \( b = 0 \), we find \(-79.92\); but we also find numbers such as \( 74.75, \&c. \)
lens of long focus, so that Fraunhofer's lines were visible on a paper screen. When the paper covered with the platinocyanide of barium was substituted for the ordinary paper screen, nearly the whole portion on which the blue rays fell appeared of an unaltered blue colour; but in this blue part of the spectrum, three isolated green fluorescent bands appeared. One of these fluorescent bands falls with its middle upon Fraunhofer's line G; the other two lie between G and F, and the middle of these bands corresponds with the wave-lengths 0.000462, 0.000446, and 0.000430 millim. The rays of light of the lengths of undulations just mentioned consequently produce a green fluorescence upon platinocyanide of barium; whilst the intervening lengths of undulations, 0.000454 and 0.000438 millim., as well as rays of the length of undulation, 0.000420 millim., produce no fluorescence upon this substance.

An uninterrupted green fluorescence only begins at that spot of the spectrum which nearly corresponds with a length of undulation of 0.000410 millim. I am unacquainted with any analogous phenomenon.—Poggendorff's Annalen, 1858, No. 8. p. 649.

ON THE INCREASE OF THE ELECTRICAL RESISTANCE OF THE METALS WITH THE TEMPERATURE. BY R. CLAUSIUS.

In the May Number of Poggendorff's Annalen there is an interesting memoir by Arndtsen upon the resistance of metals at various temperatures, in which the author arrives at the result, that in the simple metals investigated by him, with the exception of iron, the resistance increases uniformly with the temperature, and moreover, that the proportional increase in the different metals varies but little. If the resistance at the freezing-point be called 100, the numbers representing the increase for 1°C. (1°.8 F.) in the six simple metals investigated, lie, with the exception of iron, between 0.327 and 0.413; and Arndtsen adds, that if absolutely pure metals were employed, and the investigations were carried on still more carefully, perfectly accordant numbers would probably be obtained.

On glancing at these numbers, it struck me that they closely approach the coefficients of expansion of permanent gases. If we leave out of consideration the quadratic member occurring in iron, and take the mean of the whole of the first coefficients, we obtain for the resistance \( w \) at the temperature \( t \), compared with the resistance \( w^0 \) at the freezing-point, the formula

\[
w^t = w^0 (1 + 0.00366 \cdot t),
\]

from which it would follow that the resistance of the simple metals in the solid state is nearly in proportion to the absolute temperature.

Although the number of metals investigated by Arndtsen is still too small, and the agreement of the numbers too imperfect to enable us to arrive at a safe conclusion, still I think that this observation may perhaps be of some interest, and furnish an inducement to further investigations.—Poggendorff's Annalen, 1858, No. 8. p. 650.

* The numbers found by Matthiessen for potassium and sodium in the solid state also fall between these limits.
IT is not many days since I had the opportunity of seeing for the first time the Notices of the Royal Astronomical Society for January 9, 1857, which contain a paper with the following title, "An Examination of the Figure of the Indian Meridian as deduced by Archdeacon Pratt from the two Northern Indian Arcs; with a proposition for testing that form by Astronomical Observations, by Lieut. J. F. Tennant, Bengal Engineers, F.R.A.S.;" and also a continuation of that paper, read before the Astronomical Society in June of last year by the same author. The calculation here referred to by Mr. Tennant was made by me while at the Cape of Good Hope in 1854, and is published in the Philosophical Transactions of the following year. As the calculations and results of that paper do not appear to have been altogether understood by Mr. Tennant, I trouble you with these remarks*.

2. The general figure of the earth has been determined to be an oblate spheroid of ellipticity about $\frac{1}{307}$. This has been arrived at by four distinct processes. (1) Upon the hypothesis that the earth was once fluid, and by assuming a (very probable) law of density of its mass, the depression has been brought out

* I am indebted to Mr. Tennant for pointing out a numerical error near the close of my paper. Others also, which I have detected myself, I have corrected in another communication which I have recently transmitted to the Society in the hope that they may consider it worthy of a place in their 'Transactions.' These errors do not, however, at all affect my main results.

as above. (2) By pendulum experiments made in many parts of the earth it is \(\frac{1}{300}\) th. (3) From the effect of the protuberant parts of the earth’s mass on the motion of the moon in latitude and longitude, Laplace made the depression very slightly less than \(\frac{1}{300}\) th. (4) By the measurement of arcs of meridian in different parts of the world and observing the latitudes of their extremities, and comparing arcs in high latitudes with arcs in low latitudes (which has always been considered necessary to eliminate certain errors of observation), the depression has been found to be slightly less than \(\frac{1}{300}\) th of the equatorial radius.

3. But it will be observed that these methods all regard the earth as a whole: they do not determine anything regarding the separate parts. Indeed, as I have already intimated, it has long been acknowledged that the fourth method fails, when arcs near each other, and therefore appertaining to one portion only of the earth’s surface, are compared. The fact is, that physical and practical geology teach us, that the surface of the earth has for ages been undergoing changes of level in such a way as most materially to disturb the exact spheroidal form. The thickness of the crust, which Mr. Hopkins in his ‘Physical Researches’ shows to be very great, is a proof that ages must have elapsed since the earth’s form ceased to be affected by hydrostatic principles. Sufficient time has therefore elapsed for the exact spheroidal form to be pretty nearly obliterated. The spheroid (of \(\frac{1}{300}\) th depression) is the average spheroid, or the spheroid which more nearly represents the earth’s irregular form than any other—some parts being slightly above it, and some slightly below it.

4. It is therefore perfectly gratuitous to assume, as has generally been done, that the form of the earth is near an exact spheroid; that is, that the meridians are all equal ellipses, and the arcs of longitude circular. But this is assumed in Mr. Tennant’s examination of my results. I show that, if no compensating cause sufficient to counteract the attraction of the Himalayas can be found, the ellipticity of the ellipse most nearly representing the Indian arc between Kaliana (north lat. 29° 30’ 45”) and Damargida (north lat. 18° 3’ 15”) is \(\frac{1}{120}\) th, and not the mean value \(\frac{1}{300}\). Mr. Tennant’s object is to test this. But how does he test it? Not by examining my calculation, but by comparing my result with the curvature of other arcs: (1) with the prolongation of the arc down to Punnae (north lat. 8° 9’ 32”), and (2) with an arc of longitude running west from Kalianpur to Karachi. The only question, therefore, which he can solve is, whether his arcs and mine belong to one and the same spheroid or not; not whether my calculation is right or
wrong*. He also applies a third test, viz. a comparison of the computed and observed azimuth of Kalianpur and Karachi. But the same objection applies to this also. His process goes wholly upon the untenable hypothesis, that every meridian is an ellipse and the same ellipse, and that every arc of longitude is circular.

5. There are other indications that Mr. Tennant has taken a wrong view of the subject. For example (paragraph 13), "The attraction is so enormous, if Mr. Pratt's values hold good near the mountains . . . . ." But I particularly specify, and the whole line of reasoning shows, that my calculation is not made for such places (see p. 66, note, of my paper). And in the continuation of the note in the next three pages, I point out a method for places in and near the mountains; so that the wish expressed by Mr. Tennant in paragraph 17 was met in the paper before him, viz. "If an estimate of the attraction at Benog [in the mountains] could be made . . . ." Then in Mr. Tennant's second paper, there are other expressions which show that he has mistaken the matter. He says, "Mr. Pratt has failed in satisfying the geodesical data of the great longitudinal series." But I did not make the attempt. Mr. Tennant must mean that, on applying my calculation to the great longitudinal series, he failed to show that the results tally, on the supposition that the earth's figure is a perfect spheroid. This is what I should have expected, and quite confirms my general view. He says, further on, "If [the ellipticity I deduce for the great arc of 800 miles long] is useless for geodesical purposes." Of course it is, if the geodesical operations are carried on with an assumed, and most probably wrong, ellipticity. If the mean ellipticity be not the right one for that arc, then not that ellipticity, but some other, ought to be used in computing the latitudes of places in the neighbourhood of the arc, otherwise the geodesical operations of the Great Survey will be "useless" for the purpose of attaining to that accuracy which it is supposed to be capable of ensuring. I have read through both Mr. Tennant's papers and his interesting calculations with great care. They cannot disprove the results of my paper, for the reason I have stated. The only method will be to point out where my calculations are wrong, or to discover some other cause which, on calculation, is found to nullify mountain-attraction.

6. There is one of Mr. Tennant's calculations which I will make use of to illustrate this subject more at length.

Let O be the ocean, P C B A H a vertical section of Hindostan from Punna on the Great Arc (close to Cape Comorin), through

* It is worth observing that, in the first of these cases, Mr. Tennant does pronounce mine to be more correct than the mean.
the three stations C or Damargida (18° 3' 15''), B or Kalianpur (24° 7' 11''), and A or Kaliana (29° 30' 48''), and cutting the Himalayas in H. (N.B. The heights in this diagram are enormously exaggerated in order to make them visible.)

Suppose a canal is cut from the ocean at P up to the Himalayas, so that the sea-water may flow up it. When it is at rest the surface will mark the actual level. Let O c b a be this level. Draw through C, B, A the lines mCc, n Bb, r Aa, in which the plumb-line hangs at the three stations. The Survey makes Cc = 2090.5, Bb = 1878.2, Aa = 942.3 feet (see Colonel Everest's volume of 1847). The portion c b a of the sea-level is the Arc of the Meridian which the Survey measures. It is assumed in Colonel Everest's volume to be part of an ellipse of which the ellipticity is \( \frac{1}{300\,} \), the centre being at the centre of the earth, and the minor axis in a line with the earth's axis. Proceeding upon this hypothesis, he finds that the computed length of c b is 63.73 fathoms longer than the length as measured by the Survey; and that of ba 88.15 fathoms shorter. But the fact is, it is perfectly gratuitous to assume that c b a is part of such ellipse, or of any ellipse. An ellipse of the kind described is, we know, the form which the ocean would assume if H (and every similar disturbing cause) did not exist and modify, as it does, the effect of gravity; and therefore there is an a priori argument against c b a being elliptic. I can, however, prove it to be a fact.

In my paper in the Phil. Trans. of 1855, I show that the deflec-
tion of the plumb-line at stations between C and A, caused by H, varies inversely as the distance from a certain point Q about 260 miles north of A or Kaliana, and that at a distance of 1000 miles it equals 7".235. Mr. Tennant makes use of this law to determine the effect the mountain-attraction has in altering the curve of level. He calculates it from the equator (supposing that my law extends so far), and shows that the curve ascends continually higher and higher; so that O c b a being the curve as affected by mountain-attraction, O' c' b' a' is the curve which would mark the level if H did not exist. Mr. Tennant makes $cc' = 327$ feet. This is not to be depended upon, because it is calculated on the assumption that my law applies between the equator and C, which is not proved. The other results are correct,—viz. that, if c d e be drawn parallel to c' b' a', $d b = 98$, and $e a = 271$ feet.

These measures enable me to show that c b a and c d e cannot both be portions of ellipses of the kind described. For when two concentric and coaxial ellipses of small ellipticity cut each other in a point of which $l$ is the latitude in either ellipse, the distance between the ellipses at another point of which $\lambda$ is the latitude $= \frac{1}{2} c(\cos 2l - \cos 2\lambda)$, where $c$ is the difference of the minor axes. Hence if both the curves are ellipses,

$$\frac{bd}{ae} \text{ would } = \frac{\cos 36^\circ 6' 30^\prime - \cos 48^\circ 14' 22^\prime}{\cos 36^\circ 6' 30^\prime - \cos 59^\circ 1' 36^\prime} = 0.48,$$

whereas this ratio $= \frac{98}{271} = 0.36$.

It is clear that, if either be elliptic, it must be c d e, which is that obtained by eliminating the effect of H; and the curve c b a cannot be elliptic, as assumed in the Great Survey.

This shows the absolute necessity of calculating and allowing for mountain-attraction. If after making this allowance the curve does not become part of the mean ellipse, it shows that there is some other cause of derangement which we have not detected.

7. There is another point I would mention as occurring to me. We have been accustomed to think that, however much the contour of the continents may vary and depart from the mean form, the ocean must possess that figure. But I infer from the following approximate calculation that this is not certainly the case.

I have shown that the Himalayan attraction may fairly be considered to vary inversely as the distance from Q for all stations between A and C. Beyond C the law has not been proved. In fact it is clear that, from that point southwards, the attraction of the mass will be better represented by the inverse square
of the distance, for the following reason. The Himalayan mass attracts very much as if it were a huge prism running not far from latitude $33^\circ$ at $Q$, and not far from east and west. Now the attraction of a long uniform prism upon a point opposite its middle equals the mass of the prism divided by the product of the point's distances from the middle and either of its extremities. If therefore the point be near (compared with the prism's length), the force will nearly vary inversely as the distance; but if it be more distant, the force will approach much more to the inverse square. I will therefore suppose that below $C$ the force thus varies. For points of the ocean due south of Hindostan, we may suppose the force to act in parallel lines (that is, at right angles to a line east and west through $Q$), and inversely as the square of the distance from that line. The distance of $C$ from $Q = 1061$ miles, and the attraction at $C = \text{gravity} \times \tan 7^\circ$.

Hence $\frac{m}{(1061)^2}$ or force at $C, = \frac{M}{a^2} \tan 7^\circ$, $M$ being the mass of the earth, and $a$ its radius;

\[ m = (0.00000024)M. \]

If $u$ be the distance of any point of the ocean from the line through $Q$, $\theta$ its latitude, then $u = 2a \sin \frac{1}{2} (33^\circ - \theta)$, and $-\int \frac{m}{u^2} du$ is a term which must be added to the potential $V$ in the equation of the surface given by fluid equilibrium. This equation is

\[ \text{const} = V + \frac{1}{2} \omega a^2 \cos^2 \theta + \frac{m}{u}, \]

or \[ \text{const} = \frac{M}{r} (1 - \epsilon \sin^2 \theta) + \frac{m}{u}, \]

$r$ being the radius vector of the ellipse when $m=0$;

or \[ 1 = \frac{a}{r} (1 - \epsilon \sin^2 \theta) + \frac{m}{M} \frac{a}{u}, \]

or \[ r = a \left(1 - \epsilon \sin^2 \theta + \frac{m}{M} \frac{a}{u}\right), \]

$\epsilon$ being the ellipticity.

From this, by differentiating, we have

\[ \frac{1}{r} \frac{dr}{d\theta} = -\epsilon \sin 2\theta + \frac{m}{M} \cos \frac{1}{2} (33^\circ - \theta). \]

Hence if $\Psi$ be the angle through which the normal to the ocean-surface is thrown back by the attraction,

\[ \text{arc } \Psi = \frac{m}{2M} \frac{d \cos \sec \frac{1}{2} (33^\circ - \theta)}{d\theta}. \]
Hence the total rise of the ocean from the equator to this point, owing to the attraction,

\[ \int \psi \, d\theta = \frac{m \cdot a}{2M} \csc^{\frac{1}{2}}(33^\circ - \theta) = \frac{m \cdot a}{2M} (4.620 - 3.521) = 28 \text{ ft.}, \]

taking the limits from the equator to Cape Comorin. This shows that the attraction has a considerable influence, even at that distance, upon the ocean. The fact is, that though the force is small it acts horizontally in the nearer and most important parts of the ocean, and, owing to the peculiar property of fluid pressure, accumulates the effect*. Had I supposed that the law of the inverse distance had prevailed below C, the result would have been still larger.

8. An inference I would draw from this calculation is, that the sea-level affords us no real standard from which to measure heights, and thence determine the ellipticity of the earth with extreme precision. Had I calculated the height of the ocean at Karachi, it would have come out much more than 28 feet, so as quite to derange the perfect spheroidal form. The height in the latitude of C would be 105 feet, and at Karachi 225 feet. Moreover, had I integrated from the antipodes instead of from the equator, all these elevations would be larger by about 67 feet: but my object has been to refer the change of sea-level to the equator.

9. In the paper to which I allude, in a previous note I have shown that it is not impossible, nor perhaps improbable, that a cause does exist, which, if not altogether, yet nearly counteracts the effect of the Himalayan Mass. At the same time it will not explain the anomalies of the Indian Arc. There must be some other cause besides Himalayan attraction to account for them—either some local cause in the neighbourhood of one or both of the extreme stations where the astronomical amplitude is observed, or in the interior of the earth. Unless these disturbing causes can be detected, and their effects calculated, the errors are sure to be reproduced in the mapping of the country. The measurement of the lengths of arcs has been carried out by the Survey with wonderful precision, as the tests show. It is when the degree of bending (the knowledge of which is essential to

* The moon's maximum power to disturb the ocean = \( \frac{1}{110000000} \)th part of gravity. (See Herschel's 'Astronomy,' p. 496.) The Himalayan mountain-attraction at the equator and at Cape Comorin equals (according to the above) \( \frac{1}{145000000} \)th and \( \frac{1}{560000000} \)th part of gravity respectively, and is therefore very much greater than the force producing the tides.
marching) is sought that the difficulty appears, because it is in
that part of the problem that the errors in the vertical line have
any influence, and in no other part.

I am, Gentlemen,

Your obedient Servant,

Calcutta, September 15, 1858.

J. H. Pratt.

XLVI. Observations on the Electrical Discharge through Rarefied
	Gases. By M. Plücker*.

[Continued from p. 135.]

Magnetic Light.

47. The behaviour towards magnetism of that light which,
proceeding from the negative electrode, spreads out
in all directions, is so remarkable that I shall in the first place
recur to it again. We can best illustrate this behaviour by con-
sidering the well-known fact, that when iron filings are strewn
upon a piece of stiff paper covering the pole of a magnet, they
arrange themselves in curves which have been called magnetic
curves, or lines of magnetic force. Such curves render the dis-
tribution of the power of a magnet visible even when analysis is
unable to determine their form. In every such curve the sepa-
rated particles of iron having, under the influence of the magnet,
become themselves little magnets, arrange themselves with their
attracting poles together so as to form a chain. Could we remove
the particles of iron from the influence of gravitation and distri-
bute them through the whole space surrounding the magnetic
pole, then such chains assuming the form of magnetic curves
would traverse the whole magnetic field, and furnish a visible
image of the distribution of the magnetic force. The links of
every chain would be in a condition of stable equilibrium; every
portion of every chain, if removed by an external force from its
position, would, when left to itself, return to its original place
again. Never more than one single magnetic curve passes
through a given point; and the position which any one link of
the chain occupies in space determines the form of the whole
chain; so that if we were to fasten such a link rigidly, and then
give the chain any other form, it would, on being again left to
itself, resume the original form of the magnetic curve. Let
us imagine the links of a chain, thus withdrawn from the influ-
ence of gravity, still to adhere together after having lost their
magnetism in consequence of the cessation of the inductive action

* Translated by Dr. F. Guthrie, from Poggendorff's Annalen, vol. civ.
p. 113.
of the magnet. Let us further suppose that any number of such curves of any form spread out in all directions from one given point of space, then, on exciting the magnet, all such chains would collect together into one and the same curve—in that magnetic curve, namely, which is completely determined by the given point.

48. The hypotheses conditioning such a phænomenon are such as can scarcely be realized; so that the phænomenon itself will probably remain a merely imaginary one. If, however, in place of the linked iron chain, we suppose rays of magnetic light, the phænomenon is converted into one which actually exists.

49. On leading an electrical discharge through a Geissler’s tube provided at its extremities with bulbs to the centres of which the electrodes penetrate, such magnetic light radiates from every point of the negative electrode, and spreads through the whole interior of the bulb containing this electrode. If all the negative electrode except its extremity be isolated by a fused coating of glass, then the above radiation is confined to the free point. The rays proceeding from this point collect in one single line of light, which coincides with the magnetic curve passing through the end of the negative electrode, and which by its luminosity renders such magnetic curve visible.

Thus every ray which is bent in this magnetic curve, forming a portion of the arc of light, behaves exactly as if it consisted of little magnetic elements placed with their attracting poles in contact. In other words, such a ray behaves as a magnetic thread of perfect suppleness, and which accordingly, on being rigidly held in any one point (the extremity of the negative electrode), assumes the form of the magnetic curve passing through this point, or, what comes to the same thing, as an electrical current twisted in an infinitely thin spiral.

50. By the above illustrations I have merely sought to make the nature of the phænomenon intelligible, without in the least attempting to describe the nature of the magnetic light itself*.

* I have nothing further to add to what I have already remarked (38) concerning the formation of the luminous surfaces which take the place of the magnetic curves of light when the electrode, no longer isolated, radiates magnetic light throughout the whole of its length. I may take this opportunity of mentioning a wider tube of about 32 centims. in length, into which the one electrode penetrated a distance of 10 centimetres. If, in passing the electricity through, this electrode was made the negative one, the tube was filled, as far as the electrode penetrated, with magnetic light. When the tube was placed in an equatorial position upon the approximated armatures, this light contracted to a beautiful, single, dome-shaped light-surface. On placing the tube in an axial position, the magnetic light collected in the plane passing through the wire, and formed in this plane a splendid arch upon the glass, extending from one armature to the other. The inner
51. What are the conditions for the formation of that magnetic light which has hitherto only been observed at the negative electrode, being there sharply separated from that light which forms the electric light-current, and which suffers by the magnet the deflection of such a current, that is, an electro-magnetic one? What is the difference between these two kinds of light, upon which the magnet exerts so different an action?

Small particles of platinum are generally torn off from the negative platinum electrode and deposited upon the internal surface of the glass bulb enclosing the electrode*. A consequence of this is, that this bulb becomes gradually blackened, and after a long-continued action, the bulb, if not too great, becomes coated internally with a beautiful metallic mirror. By chemical analysis we may prove the presence of platinum in the deposit. It is clearly most natural to imagine the magnetic light to be formed by the incandescence of these platinum particles as they are torn from the negative electrode. We must not, however, forget the fact that the bulbs do not blacken in all cases. Even in such cases (as, for instance, in tubes containing fluoride of boron) where the magnetic light is very beautiful, and the influence of the magnet upon it very great, no deposit upon the bulb is observable, although the action goes on for a long time. The deposit has a very different appearance according to the nature of the gas in the tube. The nature of the gas has, further, an effect upon the warming of the negative electrode, which, under circumstances otherwise identical, sometimes glows brightly, and sometimes not at all. The radiation of luminous particles of platinum must appear especially strange in the case where the spark of a very moderate-sized electrical machine is discharged through the tube (42), and where, under ordinary circumstances, an elevation of temperature of the negative electrode would be scarcely perceptible.

The increased temperature of the negative electrode is communicated to the bulb surrounding it, probably principally by means of the platinum particles projected from the pole.

52. It would scarcely require a special experiment to prove that the bulb would only be blackened at the place where the disc of light came into contact with its internal surface, if a fresh tube side of this arch was the brightest and the most sharply defined, and touched the wire above the middle point between the two armatures. This arch remained unmoved and unchanged, on slipping the tube along, in the direction of its length, until the extremity of the electrode passed beyond the middle between the two armatures. The phenomenon then underwent a change, which need not be here further considered.

* I have never observed a tendency of these atoms to move towards the other electrode.
were so brought in equatorial position between the two approximated armatures that the bulb containing the electrode chosen as the electro-negative one touched them from the outside (to take one particular case), the electric current being then led through the tube, and the magnetic light becoming concentrated into a sickle-shaped disc.

53. The colour of the magnetic light appears to depend upon the nature of the gas in the tube. This light is in most cases violet, of a more or less red tinge; it is often accompanied by an intense green light (35); frequently it is of a beautiful blue; and under otherwise unfavourable circumstances I have observed an intense yellow flame of magnetic light.

Hitherto we have assumed the use of platinum electrodes. In the phenomena under consideration, the nature of the metal of the electrode appears nevertheless to exert no influence. I have already mentioned the apparently perfect similarity in the behaviour of strongly cuppered, silvered, and gilded platinum electrodes (37). A sewing-needle as negative electrode gave the same appearance, as also electrodes of lead and tin. (The portion of the electrode fused into the glass can of course be nothing but platinum. The metal under investigation was fastened appropriately to the platinum.)

54. From these experiments we are, it seems, justified in concluding that the diamagnetic or paramagnetic nature of the particle torn from the electrode need not be taken into account in considering the magnetic light. If, on the other hand, we were to assume that these particles, retaining the electricity of the electrode, gave rise by their motion to elementary currents, and further assuming their motion to be a spiral one, we should certainly have a sort of explanation for the phenomena observed. Such an explanation, however, is not as yet sufficiently justifiable.

55. Magnetic light under the influence of magnetic excitation gives no trace of polarization.

56. In consequence of our want of knowledge about magnetic light, and of the total want of analogous phenomena, I performed many experiments in order to obtain magnetic light under other circumstances. All such attempts, however, were fruitless; it may suffice to mention one of them here.

I directed my attention especially to that remarkable fluorescent light, for the more exact knowledge of which we are indebted to Mr. Stokes. In order to produce this light I employed a narrow Geissler tube, which showed an intense blue light on the passage of the current; this was fastened to the middle of a wider tube containing an aqueous solution of aesculine. On exciting the current, the fluorescent light in the water formed
a cylindrical envelope of light of the well-known light blue colour, surrounding the narrower tube. When this system of tubes was laid in an equatorial position upon the two approximated armatures, and the electro-magnet excited, no action was observed upon the cylindrical envelope of fluorescent light.

As yet, therefore, the magnetic light appears only to occur under very limited circumstances; and electrical conditions appear essential to its production.

**Electro-magnetic Action upon the Electric Light-current.**

57. In the observations already described (9 to 15), it has been shown that, apart from secondary phænomena, the magnet acts upon the electric light-current in tubes of rarefied gases in the same manner as upon the electric current in metallic conductors. For although hitherto practical difficulties have prevented the construction of apparatus to reproduce with the electric light the rotation of Barlow's wheel and similar phænomena, still, even in the incomplete results obtained, we recognize a new confirmation of the above assertion.

58. The following observation may be not unworthy of being mentioned. The fact that, in consequence of the magnetic attraction and repulsion of the light-current, the latter was sometimes increased, sometimes diminished, in intensity in different places, was at once and clearly recognized by me. Subsequently, however, by the approximation of a tube through which the discharge passed to the magnet, I have observed the light-current become weaker and entirely disappear, and at the same time the beating of the hammer in Ruhmkorff's apparatus underwent a change, showing an alteration in the conditions of conduction.

59. Further, the different kind of action upon differently-coloured light, which is originally united in the electrical light-current, deserves our attention. This occurs, for instance, in tubes containing hydrobromic or hydriodic acids; and the different lights correspond probably with the different ponderable substances in the tubes. The original light is thereby decomposed into light of different colour, which, under certain circumstances, appears as variously-coloured flashes of lightning (18). Dependent upon this is also the phænomenon, to be afterwards described, that in narrow tubes the colour of the discharge-current is temporarily changed by the magnet.

60. The electric light of the discharge-current in Geissler's tubes extends also to those parts which do not lie in the path of discharge. This occurs, for instance, in the long narrow tubes which, having been fused on to any point of the principal tube, and having been used as evacuating tubes, have been fused off
the chief tube. The following observation shows that this light is not a stationary one, like the light of a flame or like the magnetic light, but it is light which passes to the end of the tube and back again to form again a portion of the general electric current.

61. Amongst the tubes constructed by M. Geissler for the sake of effect, there was a cylindrical one of about 25 centimetres in length, and 3 to 4 centimetres in thickness. Inside this was a narrow tube bent several times, its whole length exceeding a metre. One end of this narrow tube carried a platinum electrode, and was fused into the first end of the wider external tube. The other and open extremity of the narrow tube terminated near the end of the wider tube, at a place where the latter was much contracted. The wide tube, after being again widened, received the second electrode. As the electric current was discharged out of the narrower into the wider tube, only a portion of it passed, through the contraction of the latter; another portion was spread out backwards in the wider tube, forming beautiful rings round the narrower one. On laying the tube in an equatorial position upon the magnet, the light became disposed about its upper and under parts. The current descended from the opening of the narrow tube, passed backwards along its lower side, and then rose, proceeding to the upper side of the tube towards the contraction. On altering the direction of the current and inverting the magnetic polarity, there resulted the modifications of the phenomena which were to be anticipated. It was remarkable that, whatever might be the direction of the electric discharge, magnetic light always occurred at the second end of the wider tube.

**Deviation of the Electrical Light-currents by the approximation of Conductors.**

62. A remarkable circumstance is the different kind of deviation of the electrical light-current on bringing the hand or other conductor into its neighbourhood. This deviation is very various, according to the different nature of the ponderable substance in the tubes: it is especially remarkable when the tubes contain traces of fluoride of silicium. I shall here describe an observation made by M. Geissler, in that gentleman's name.

63. Amongst the diverse forms which M. Geissler has lately given to his tubes, one form is especially remarkable, on account of the varied nature of the phenomena which it offers. These tubes, 50 to 70 centimetres in length, are composed of several pieces: the middle piece has an ellipsoidal or cylindrical widening. The ends are composed of wider and shorter cylinders, bearing spheres into which the platinum electrodes are fused.
The middle piece is connected with the end ones by means of two tubes of different diameter, the narrower being a rather wide thermometer-tube.

64. One of the tubes just described contained fluoride of silicium. The electrical light-stream was diffused through the wide middle cylinder with a faded colour, while the light in the connecting tubes was far more intense, and more intense in the narrower than in the wider of these tubes. In the bulb containing the negative electrode, a beautiful atmosphere of magnetic light was diffused.

65. On bringing the hand towards the middle cylinder, the light was deviated even while the hand was at a considerable distance. On touching the cylinder with the finger, the light was sometimes attracted to the point touched; sometimes it was repelled from this point, the light-current in the cylinder being broken by dark strata. On inverting the Ruhmkorff's apparatus, an inversion of the phenomena invariably occurred.

The very remarkable phenomena described would be contradictory if everything had been symmetrical on both sides of the middle cylinder. The free tension at the two ends of the induction wire is very different; but it is easy to convince oneself that the cause could not consist in this. Nothing therefore remained but to seek the cause in the difference of width of the two connecting tubes; and it was actually found that the light in the middle cylinder was either attracted or repelled by the hand, according as the positive light-current passed into this cylinder by the wider or narrower of the connecting tubes.

Spectra of the Electrical Light-currents.

66. It was to be confidently expected that, on looking through a prism at a tube which showed clearly the black lines of Ruhmkorff, these lines would be elongated to longitudinal ones traversing all the colours of the spectrum. This conjecture I immediately found confirmed on trying the first Geissler tubes in the physical cabinet of this town (Bonn). I convinced myself also on the same occasion that such tubes show beautiful spectra of the most varied kind, according to the nature of the traces of gases or vapours which they contain. All these spectra have this in common, that the colours do not merge into one another as in the ordinary solar spectrum. They are, on the contrary, sharply demarcated; and the separate spaces of colour again are also divided into well-defined lighter and darker strips. Each gas, moreover, has a characteristic spectrum.

67. On discharging Ruhmkorff's apparatus through one of the tubes before described (63), not only the intensity, but the colour of the light is different in different parts of the tube.
The eye perceives, for instance, in one part of the tube red, in another violet, and in the middle cylinder a fainter colour; so that one would be inclined to imagine that the ponderable matter which becomes luminous is differently distributed through the tube. In addition to this, it happens in many cases that the colour of the electric light undergoes a change in its passage through the narrow tube on the excitation of the great electro-magnet (upon the approximated armatures of which we shall suppose the tube placed, in equatorial position); so that if the light were, for instance, in the first place of a beautiful red or violet, it becomes of a fine green or blue colour (59). But in all cases, whatever may be the colour-impression produced on the eye, the distribution of the colours in the spectrum remains for the same gas entirely of the same kind; it is the intensity of the colours alone which changes in different degrees in different portions of the spectrum: so that when the eye (whose judgment is, moreover, considerably influenced by the external illumination) is at fault, still the nature of the gas or vapour contained in the tube is unfailingly determined by means of the spectrum.

68. If, for instance, we take a narrow tube with platinum electrodes whose capacity is only a fraction of a cubic centimetre, and, having filled it with hydrogen, so far exhaust it that the tension is reduced to two or three millims., then the traces of gas are so small that they may be said to be inappreciable by chemical reaction, and the most delicate chemical balance is unable to recognize their weight, being, as it is, a fraction of the thousandth of a milligram. On passing an electric current through the tube, however, these traces of the gas becoming luminous, are rendered visible to and recognizable by the almost dazzled eye by means of a beautiful characteristic spectrum.

69. As yet I have only observed these spectra by means of a telescope (such as that employed by Fraunhofer in the observation of the lines of the spectrum), without angular measurement. The refracting angle of the flint-glass prism, which was placed immediately before the object-glass of the telescope, was 45°. The eye-glass of the telescope was, however, generally replaced by one of less magnifying power. The narrow tube through which Ruhmkorff's apparatus was discharged, was suspended vertically at a distance of 4 millims.

70. Hydrogen gas gave a comparatively simple spectrum, in which five bright bands of almost equal breadth were especially prominent:—A, a bright violet band beyond the limit of the spectrum; three bands in the green, of which the one, B, bounded the green towards the violet, D forming the boundary on the other side, and C between these last about twice as far from B
as from D; finally, a beautiful yellow band, E. Of the three bands in the green, D is especially bright, being of a yellowish tinge; then follows C in respect of intensity, and finally B. The red is very prominent, spreading over an extensive surface. A well-defined thick black line occurs near its further boundary. On the other side it is sharply separated from the yellow band E by a broad black line: the whole of the yellow light is concentrated upon this band. The band D is separated from the band E by a grey interval, almost equal in breadth to that separating D and C. The violet light is confined to the band A. The space between A and B is divided into a completely black space, and one of an indefinite dark colour. The black space sharply bounding band A, is about three times as broad as each of the five bright equally broad bands, and about a third of the breadth of the whole space between A and B, or between B and D.

71. As another example, I shall take the beautiful spectrum of gaseous fluoride of boron as one totally different from that just described. In this spectrum, which shows bright colours from one extremity to the other, red, together with orange and yellow, takes up about a fifth of the whole space; of the other four-fifths, two are occupied by the green, and the remainder by the violet. It is remarkable that the eye does not perceive any transition between the violet and the green, the blue appearing to be entirely wanting. Yellow and orange form two sharply bounded bright bands of about equal breadth, both together being about half as wide as the red, from which the orange is separated by a strong black line. Near the boundary of the red there is a second such black line. The space of the red between these two black lines is divided into six equal parts by five fine black lines; and on the other side of the strong black line first mentioned, there is in the orange and yellow also the same number of fine black lines at the same distance from one another. In the green, about twice as far from the violet as from the yellow boundary, there is a bright green band about as broad as the yellow band. This green band divides the green space into two parts, which differ importantly from one another. That part which lies next to the yellow, has in the middle a bright shining band; and the green on either side of this band is not uniform, for it becomes continually darker towards its two extremities. The remaining portions of the green and of the violet have quite a characteristic appearance. This space appears divided into sixteen bands, ten of which belong to the violet. Each separate band is bright towards the red, becoming gradually darker in the opposite direction. The breadth of these bands is not quite
uniform; they are broadest towards the middle of the violet. The broadest of these bands is on the one side of a bright shining violet; on the other side it is completely black. At this part the spectrum presents in the telescope the appearance of a strongly illuminated fluted column.

72. For the present I shall confine myself to the description of these two spectra. The most difficult question which arises on the discharge of electricity through rarefied gases, is the chemical nature of the ponderable substance which gives rise to so infinitely varied phenomena of light. This question can only be safely discussed in connexion with the prismatic analysis of the light which is produced,—the more so as by this means every sudden or gradual chemical change in the substance is recognized.

73. In confirmation of this assertion I shall adduce the following example. It appeared to me to be eminently desirable to determine the spectrum of oxygen gas, and to draw it, like the other spectra, in colours. M. Geissler, with his known skill, prepared the tubes necessary for this purpose, employing thereby chlorate of potash for the preparation of the oxygen. The pale flesh-coloured light in the narrow part of the tube, gave a spectrum characterized by a remarkably bright band at the end of the red, and by two beautiful orange-coloured bands, which were separated by a narrower and perfectly black one. In the green there also appeared bright bands; the violet was very dark.

I and my pupil, M. Lick, who purposed making a drawing of the spectrum for me, on looking at it, the one after the other, could not agree as to its appearance. The cause of this I soon discovered to consist in the continual change of the spectrum. The violet became more intense, black streaks appearing in it; the bright line to which the red was originally confined, became paler and paler. Bright red bands appeared over a wider space, alternating with dark ones. In short, the entire spectrum had undergone an alteration. The light in the narrow tube became continually greener, increasing in brightness. I increased the induction current, by adding two to the three Grove's elements used in exciting the Ruhmkorff's apparatus, allowing it to traverse the tube continually in the same direction. The spectrum remained for a time so constant, that it might easily be drawn; it then commenced to diminish rapidly in intensity, the light in the narrow tube becoming gradually quite violet. The discharge through the tube began to be discontinuous, till at last only single separate discharges occurred; finally the current entirely ceased to traverse the tube.

I consider the original spectrum to be the spectrum of the oxygen gas, which evidently loses gradually its free condition. If we do not consider the possible formation of ozone, the second
spectrum is probably to be attributed to the traces of some other gas not entirely driven out of the tube, making their effects felt. Decisive experiments have still to be tried concerning this point. After the complete disappearance of the free oxygen gas, the tube contains too little gas to admit the passage of the stream, in complete accordance with the older experiments of Mr. Faraday, and the later ones of M. Masson; the interior of the tube behaves as a complete and non-conducting vacuum.

Before the entire extinction of the current in the tube, I in vain endeavoured, by inverting the poles, to restore the continuity of the discharge. By such inversion, the bulb about the negative pole became filled with the already mentioned enigmatical beautiful green light (35); but this light was also extinguished as soon as the current ceased to pass through the tube.

The current had passed through the tube altogether for about $1\frac{1}{2}$ hour.

74. I shall here break off, because, apart from the chemical question, investigations of the spectra of different gases cannot have the requisite degree of accuracy. These spectra are essentially different from those belonging to the electrical arch of light in the air, and from metals glowing or burning in it. I doubt whether the particles carried off from the electrodes exert any influence upon the spectra above described (51): I think rather that these spectra belong entirely to the rarefied gases. On the other hand, the electric arch of light in the air is never free from matter, which is carried over (carbon or metal), whose incandescence gives rise to new bright lines in the spectrum, peculiar to each substance*.

75. In connexion with the chemical question, I propose recurring to the question of the spectra. The subject is one belonging, if I may use the expression, to Micro-chemistry. Conditions occur in it which differ from those under which chemical actions usually take place. It is only on the successful solution of these questions, that many not unimportant points for the molecular theory will be satisfactorily solved, such as—

How may the spectrum of a mixed gas be derived from the spectra of its constituents?

How are the spectra of a compound gas related to one another before and after its chemical decomposition by the current?

How does the chemical combination which the gas effects with the electrode influence the spectrum?

Do isomeric gases give rise to similar spectra?

Bonn, March 30th, 1858.

* Compare the interesting investigation of M. Masson on electrical photometry.
XLVII. On the Properties of Compound Double-refracting Rhombs. By Professor Potter, A.M.*

IN the Philosophical Magazine for December 1857, I entered into some discussions of the properties of Nicol's rhomb, and also of other forms of rhombs, for procuring a beam of plane-polarized light. There stated that I believed the explanation of the manner in which Nicol's rhomb furnishes a beam of plane-polarized light by one of the double-refracted rays undergoing total reflexion, had not been published before it appeared in my experimental treatise on Physical Optics. I have found, since writing that paper, that the credit of first giving that explanation belongs to M. Radicke, who gave it in his Handbuch der Optik, vol. ii. p. 369; and he has a discussion of the subject also in the Annalen der Physik und Chemie for 1840.

With respect to the computations given in my paper of December 1857, those for Nicol's rhomb are upon the only method we can follow until we know the specific refraction of the two double-refracted rays on passing from calc-spar into Canada balsam, and they are sufficient for the approximate solution. The method followed in that paper for the case when air is left between the component prisms is, however, so defective for the extraordinary ray, that the results must be rejected as erroneous. A strict numerical computation might be obtained without a very great deal of trouble if the subject were worth it.

I have now to describe the practical results afforded by the rhomb of calc-spar and glass described in the paper, which is similar in form to the common double-image rhomb, but with air between the component prisms in place of Canada balsam, and especially the particular form described in the last sentence of the paper. In that form a rhomb of calc-spar is cut in a plane through the optic axis, and this is made one of the outer faces of the compound rhomb, with a polished natural cleavage plane for the inner face next that of the glass, as at $ab$ of fig. 4 of the paper. When we place a rhomb thus constructed before the eye with the calc-spar nearest to it, and look directly towards the daylight, we see two boundaries of total reflexion, one for the ordinary, and the other for the extraordinary rays, with the interval of $8^\circ$ or $9^\circ$ between them, which is of plane-polarized light; and this is the analogous beam to that which is employed when Nicol's rhomb is used, where a plane-polarized beam of $27^\circ$ is obtained. When the glass portion is nearest the eye, only one boundary of total reflexion is seen; but the two beams having an angular separation of about $11^\circ$, another boundary of

* Communicated by the Author.
Dr. Genth's *Contributions to Metallurgy.*

A transmitted beam can be made by an opaque screen, and so a beam of plane-polarized light can be obtained. When the common double-image rhomb is placed in the same manner before the eye, no boundaries of total reflection are seen.

I have tried the rhomb above named, with air between the component prisms, in various experiments where Nicol's rhomb would generally be used, and found it succeed; amongst others, I have used it as polarizer and analyser in the microscope: but I have not found any advantages to recommend it in preference to Nicol's rhomb, except a saving of the material, calc-spar; and the smallness of the angle gives some disadvantages. The only cases in which it would have a decided advantage, are those in which it was proposed * to be used where a large breadth of pencil was required with small angular divergence or convergence; and then convergence in the transmitted beam may be produced by a convex lens. In this way an analyser may be formed so as to throw images of selenite designs upon a screen for exhibition to a number of persons better than by any other method for brightness and distinctness; and there may be found other peculiar cases where it would have an advantage over other rhombs.

---

**XLVIII. Contributions to Metallurgy.—No. I.**

By Frederick A. Genth, Ph.D.†.

The difference in the appearance of a lot of Chinese coins which were lately offered for sale in this city by a native of the Celestial Empire, and the little knowledge which we have on the subject of Chinese metallurgy, suggested the idea that, by a careful examination of the different kinds, some new and serviceable alloys might be added to those already used in the arts, or, at least, that by such an investigation some information might be obtained of the Chinese ores, and the manner of their reduction. With the exception of three or four alloys, which have been repeatedly analysed, we know hardly anything of the metallic compositions used by the Chinese. I have endeavoured to learn from the works written on China, how their alloys were manufactured, what kind of ores they use for that purpose, whether they prepare their alloys directly from the ores, or by mixing the metals in the proper proportions, &c. Unfortunately, I have been able to consult but few books; and these have frequently given statements which, on account of the want of familiarity with metallurgy of their authors, are unquestionable.

* Potter's 'Physical Optics,' p. 31.
† From the Journal of the Franklin Institute, S. 3. vol. xxxvi. p. 261.
ably erroneous; so we find that different authors use not un-
fre-quently the same name for altogether different substances.

The white copper of the Chinese, more than any other metal
or alloy, appears to have attracted the curiosity of the writers on
China. John Francis Davis*, who generally seems to be very
well informed, speaks of it as an alloy of copper, zinc, and iron,
with a little silver, and occasionally some nickel. The process
of its manufacture, as he gives it, however—directly from the
powdered ore, mixed with charcoal dust, and placed in jars over
a slow fire, and the vapours condensed in water—leaves no
doubt that the Chinese furnished him with their method of distil-
ling zinc.

Of the same substance speaks undoubtedly Father Mailla†,
who observes that the Chinese, besides the common copper, have
another one of a white colour, which is obtained directly from
the ore, and is called Pé-tong‡. He describes it as being whiter
on the fresh fracture, but very brittle and hard, and mentions it
as being frequently used in China,—that it is necessary, however,
to add tutanegue or some similar metal to it, in order to render
it soft and less brittle. The meaning of Father Mailla's tuta-
negue is unintelligible. The word tutanegue, or tutenag, means
zinc§; but it is frequently, although erroneously, used for an
inferior alloy of copper, zinc, and nickel. The richest alloy
composed of these three metals, the real pack-fong, or, more
correctly, pack-tong, is undoubtedly the most interesting of all.
The best kind, which contains nearly 32 per cent. of nickel, has
the colour of silver of 18 carats; this alloy is sometimes called
electrum. There exists, however, in China a good many varieties
which contain a great deal less nickel. They are too well known
to require repetition.

Another very interesting alloy is that from which the gongs
are made, the so-called tam-tam metal. It contains about 80
per cent. of copper, and 20 per cent. of tin. This composition
seems to have been in use for many hundreds, perhaps thousands
of years, for an ancient cast coffin, found on the cast side of the
Altaï mountains, near the boundary line of China, gave on analysis
the same substances, in almost exactly the same proportion. Of the
other alloys of tin and copper, that which is called Chinese silver,
and which is used for mirrors, is the most interesting. We have

† Histoire générale de la Chine; traduite du Tong-kien-kang-mon. Par
le Père Mailla.
‡ According to Stan. Julien (Comptes Rendus, vol. xxiv. p. 1069), Pé-
tong is an alloy of copper and arsenic.
§ Handbuch der Technischen Chemie von Ernst Ludwig Schubarth.
Berlin, 1851.
no analysis of it; but it has in all probability the same composition as our speculum-metal, which is used for the construction of mirrors of telescopes or similar purposes: Davis says that it contains tin and copper, and perhaps a portion of silver.

Father Mailla states that the alloys used for coins are made from the materials furnished, since a very early period, from the provinces Yun-nan and Koc-tcheou. The coins are called Tschen, and are, according to Davis, composed of copper and zinc, with perhaps some lead. These are the only coins issued by the Chinese government; and their value should be equal to one-thousandth of the Chinese silver ounce or tael, which latter is equal to 1.40 dollar of United States money. Their market value, however, when Davis made his observations, was equal to 1.240 tschen for one tael fine silver, although the expense of their manufacture was frequently greater to the government, so that the Viceroy of Fokien, in a memoir to the Emperor, suggested to close the mint of that province, and suspend the coinage altogether, until the relative value of tschen and silver would approach nearer to a par. Although the value of one tschen is not more than about one-tenth of one cent of our money, and the inducements to counterfeit the same cannot be very great, still the Chinese cannot resist their unconquerable propensity for substituting something of an even smaller value, that they forge them and smuggle casts of lead into circulation. The tschen have an inscription on both sides: on one the name of the reigning emperor, with two words denoting "current value;" on the reverse, a Tartar inscription. They have a square hole in the centre, through which they are strung together by hundreds to save counting. Most of the coins which I have seen were of this description; and they are certainly the most common. However, there are two other kinds, which are not mentioned by Davis, and of which I am therefore in doubt whether they are government's issue, viz. of cast iron, and of an alloy principally of lead and copper. Of the cast-iron coins I have seen only one. It had a square hole in the centre, like the tschen, and similar inscriptions on both sides. It is very hard, but was so much oxidized that it was impossible to examine more closely into the nature of the metal. The other coin appears to be the same of which Renouard de St. Croix speaks*, and of which he says that it is called Patéc, is made of an alloy of copper and lead, has a square hole in the centre, and an inscription only on one side. The analyses Nos. 7 and 8 are in all probability this coin: it was impossible, however, to observe any inscription on either side; but as they are made of a very soft alloy, this may have been worn off.

* Renouard de St. Croix, Voyage, vol. iii.
All the Chinese coins are cast, and are not struck or stamped. Many of those which I have examined showed the impression of the fine sand used for the moulds; others were hollow, others did not fill the mould completely, or were full of very minute cavities and air-holes; hence the specific gravity was in some found to be considerably smaller than it ought to have been.

A pupil of mine, Mr. George J. Pöpplein, of Baltimore, has at my suggestion made in my laboratory the following very careful analyses of the various Chinese coins.

The analytical methods adopted did not materially differ from those generally in use. It was found, however, that tin cannot be accurately determined by dissolving in nitric acid, &c. It was therefore always separated in the usual way; but the binoxide of tin was afterwards decomposed by fusion with sulphur and carbonate of soda, by which method the small quantities of lead, silver, copper, and iron were converted into insoluble sulphides, whilst the bisulphide of tin and sodium was dissolved by water, &c. All the necessary precautions were also taken to ensure a complete separation of the zinc from the copper. The following are the results of Mr. Pöpplein's analyses:

No. 1. Tschen.—Colour, pale brass-yellow, greyish on the fresh fracture; very good cast, with very fine grain, and but very few air-holes inside; exceedingly brittle. It contains—

<table>
<thead>
<tr>
<th>Element</th>
<th>Quantity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper</td>
<td>63.94%</td>
</tr>
<tr>
<td>Tin</td>
<td>2.29%</td>
</tr>
<tr>
<td>Lead</td>
<td>6.02%</td>
</tr>
<tr>
<td>Zinc</td>
<td>26.24%</td>
</tr>
<tr>
<td>Cobalt</td>
<td>trace</td>
</tr>
<tr>
<td>Iron</td>
<td>1.35%</td>
</tr>
<tr>
<td></td>
<td>99.84%</td>
</tr>
</tbody>
</table>

No. 2. Tschen*.—Colour pale bronze-yellow; the composition was not homogeneous; and with the magnifying glass two alloys, one of a brass-yellow, the other of a pale copper-red colour, could be distinguished: the colour of the fresh fracture was greyish; medium grain, brittle. It contains—

<table>
<thead>
<tr>
<th>Element</th>
<th>Quantity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper</td>
<td>60.97%</td>
</tr>
<tr>
<td>Tin</td>
<td>0.05%</td>
</tr>
<tr>
<td>Lead</td>
<td>1.56%</td>
</tr>
<tr>
<td>Zinc</td>
<td>35.05%</td>
</tr>
<tr>
<td>Iron</td>
<td>2.37%</td>
</tr>
<tr>
<td>Silver and Cobalt</td>
<td>traces</td>
</tr>
<tr>
<td></td>
<td>100.00%</td>
</tr>
</tbody>
</table>

* H. Onnan (Scheik. Onderz. vol. iv. p. 517 et seq.) has analysed a similar alloy from China, of a copper-red colour.
Dr. Genth's Contributions to Metallurgy.

No. 3. *Tschen.*—Thick brass-yellow coin, very good cast of fine grain; not easily tarnishing. Sp. grav. at 25° Cels. = 8.497. It contains—

<table>
<thead>
<tr>
<th>Substance</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper</td>
<td>55.53</td>
</tr>
<tr>
<td>Tin</td>
<td>0.33</td>
</tr>
<tr>
<td>Antimony</td>
<td>3.21</td>
</tr>
<tr>
<td>Arsenic</td>
<td>3.44</td>
</tr>
<tr>
<td>Lead</td>
<td>1.03</td>
</tr>
<tr>
<td>Zinc</td>
<td>32.74</td>
</tr>
<tr>
<td>Nickel</td>
<td>0.54</td>
</tr>
<tr>
<td>Iron</td>
<td>2.38</td>
</tr>
</tbody>
</table>

No. 4. *Tschen.*—Brass-yellow with a greenish tinge, a very good cast; fine grain, brittle; easily tarnishing. It contains—

<table>
<thead>
<tr>
<th>Substance</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper</td>
<td>59.14</td>
</tr>
<tr>
<td>Tin</td>
<td>2.71</td>
</tr>
<tr>
<td>Lead</td>
<td>3.40</td>
</tr>
<tr>
<td>Zinc</td>
<td>29.62</td>
</tr>
<tr>
<td>Iron</td>
<td>4.83</td>
</tr>
<tr>
<td>Nickel</td>
<td>0.17</td>
</tr>
<tr>
<td>Cobalt and Silver</td>
<td>traces</td>
</tr>
</tbody>
</table>

No. 5. *Tschen.*—Bronze-yellow, greyish on the fresh fracture. Spec. grav. = 8.552. It contains—

<table>
<thead>
<tr>
<th>Substance</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper</td>
<td>59.983</td>
</tr>
<tr>
<td>Tin</td>
<td>1.204</td>
</tr>
<tr>
<td>Silver</td>
<td>0.068</td>
</tr>
<tr>
<td>Lead</td>
<td>3.977</td>
</tr>
<tr>
<td>Zinc</td>
<td>32.114</td>
</tr>
<tr>
<td>Nickel</td>
<td>0.552</td>
</tr>
<tr>
<td>Iron</td>
<td>2.102</td>
</tr>
<tr>
<td>Arsenic</td>
<td>1.34</td>
</tr>
</tbody>
</table>

No. 6. *Tschen.*—Colour bronze-yellow, the fresh fracture brass-yellow; the grain is coarse and crystalline, with many cavities; did not fill the mould well; very brittle. Spec. grav. = 8.166. It contains—

<table>
<thead>
<tr>
<th>Substance</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper</td>
<td>60.19</td>
</tr>
<tr>
<td>Tin</td>
<td>1.81</td>
</tr>
<tr>
<td>Lead</td>
<td>5.83</td>
</tr>
<tr>
<td>Zinc</td>
<td>31.57</td>
</tr>
<tr>
<td>Iron</td>
<td>1.34</td>
</tr>
</tbody>
</table>

100.74
Dr. Genth's Contributions to Metallurgy. 425

No. 7. Patéc.—Not homogeneous, but a mixture of a grey and pale copper-red alloy; full of very small cavities, cuts easily with the knife; brittle. Spec. grav. = 8.617. It contains—

<table>
<thead>
<tr>
<th>Element</th>
<th>Quantity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper</td>
<td>=59.88 per cent.</td>
</tr>
<tr>
<td>Tin</td>
<td>= 7.90</td>
</tr>
<tr>
<td>Lead</td>
<td>= 31.42</td>
</tr>
<tr>
<td>Zine</td>
<td>= 0.59</td>
</tr>
<tr>
<td>Nickel</td>
<td>= 0.18</td>
</tr>
<tr>
<td>Iron</td>
<td>= 0.35</td>
</tr>
<tr>
<td>Silver, Arsenic, and Antimony</td>
<td>traces</td>
</tr>
</tbody>
</table>

Total = 100.32

No. 8. Patéc.—Similar to No. 7, but of a coarser grain. Spec. grav. above 9. It contains—

<table>
<thead>
<tr>
<th>Element</th>
<th>Quantity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper</td>
<td>= 51.20 per cent.</td>
</tr>
<tr>
<td>Silver</td>
<td>= 0.03</td>
</tr>
<tr>
<td>Tin</td>
<td>= 4.81</td>
</tr>
<tr>
<td>Lead</td>
<td>= 42.25</td>
</tr>
<tr>
<td>Cobalt</td>
<td>= 0.23</td>
</tr>
<tr>
<td>Iron</td>
<td>= 1.36</td>
</tr>
<tr>
<td>Zinc and Nickel</td>
<td>traces</td>
</tr>
</tbody>
</table>

Total = 99.88

From these analyses it is difficult to come to any general conclusions, because the Chinese are not guided by certain and invariable rules in the manufacture of their coins, although they generally use a composition which might pass under the name "brass." This brass is certainly made by mixing copper and zinc together, and only seldom directly from the ore as the ancients did. We know, from the descriptions given by Father Mailla and J. F. Davis, that they make zinc, and also know that long before the manufacture of zinc came into use in Europe, the Dutch brought it from China. The copper which is used for their coins appears to be always very impure, and is evidently made from ores containing iron, sometimes with small quantities of nickel and cobalt; analyses Nos. 3 and 7 indicate the occurrence of antimonial and arsenical grey copper (tetrahedrite) in China. It seems that the Chinese frequently take at hap-hazard whatever alloys and metals they can get, melt them together, and cast their coins from the mixture. We have seen, above, that alloys of copper and tin are frequently used in China; now it seems that they add invariably a small quantity of such alloys for the manufacture of their coins, because every analysis shows the presence of tin; we know, however, that ores of copper, containing tin at the same time, are not frequently met with.
It is very probable that the Chinese have had the same kind of alloys in use for perhaps more than two thousand years; it was of interest, therefore, to compare the composition of some ancient Roman coins with that of Chinese coins. I have been permitted by the Rev. H. S. Osborn, for whom I have made analyses of coins of the emperors Hadrian and Trajan, for his work on 'Palestine, Past and Present,' to insert these in this paper.

No. 9. Hadrian Coin.—Of a bronze-yellow colour; the colour on the fresh fracture is inclining to brass-yellow; grain very fine. Spec. grav. = 8.778. It contains—

\[
\begin{align*}
\text{Copper} & = 86.92 \text{ per cent.} \\
\text{Silver} & = 0.30 \text{ } \\
\text{Tin} & = 0.72 \text{ } \\
\text{Lead} & = 1.10 \text{ } \\
\text{Zinc} & = 10.97 \text{ } \\
\text{Iron} & = 0.18 \text{ } \\
\text{Arsenic and Antimony} & = \text{traces}
\end{align*}
\]

No. 10. Trajan Coin.—The colour is bronze-yellow, inclining to brass-yellow; the fresh fracture is greyish; very fine grain. Spec. grav. = 8.745. It contains—

\[
\begin{align*}
\text{Copper} & = 88.58 \text{ per cent.} \\
\text{Silver} & = 0.21 \text{ } \\
\text{Tin} & = 1.80 \text{ } \\
\text{Lead} & = 2.28 \text{ } \\
\text{Zinc} & = 7.56 \text{ } \\
\text{Iron} & = 0.29 \text{ } \\
\text{Arsenic and Antimony} & = \text{traces}
\end{align*}
\]

Both coins were stamped, and not cast like those from China; and both were pretty tough, which is certainly owing to the smaller per-centage of zinc and iron in the same. These alloys were beyond question made directly from the ore, and only the small per-centage of tin appears to have been added afterwards.

XLIX. On the Influence of Light on the Polarized Electrode.

By W. R. Grove Esq., Q.C., V.P.R.S. &c.*

Soon after the experiments of Daguerre were published, it occurred to me that the galvanometer might be used as a test for the chemical effects of light; and I succeeded in obtaining a deflection of the needle by allowing a beam of light suddenly to impinge on a daguerreotype plate in a trough of water—the

* Communicated by the Author, having been read at the Meeting of the British Association, Leeds, September, 1858.
plate being connected with one extremity of a galvanometer, and
a gridiron of silver-wire placed in front of the plate with the
other. This experiment I showed at a lecture at the London
Institution in 1843; and it was subsequently used as an illustra-
tion of the convertibility of force, in my essay on the 'Correlation
of Physical Forces.'

I tried some further experiments at the time without obtaining
results of any importance, but as galvanometers at that period
had not reached the degree of delicacy they have since attained
in the hands of M. Ruhmkorff, I determined this summer to
resume the inquiry; and the results which I have obtained I
now proceed to describe. The galvanometer used in the following
experiments is by Ruhmkorff, formed of 544 feet of fine copper
wire, and though not as delicate as the very long-wired instru-
ments used by M. DuBois Reymond and others, it has proved
sufficiently delicate for most of the effects I aimed at.

The idea with which I started, was to arrange two plates of
platinum in an electrolyte in such manner that a bright beam
of light should impinge on one while the other was in darkness,
and yet to allow free eletrolytic communication. After making
a somewhat complex apparatus which did not answer the purpose,
the following simple means of effecting my object was adopted.
In a cell similar to those used for the nitric-acid battery, the
outer cell being of thin glass and the inner one of porous ware,
two platinum plates were placed, each of 6 inches by 2,—4 inches
by 2, or the immersed portions of the plates, being platinized or
coated with a deposit of black platinum. Both the outer and
inner cell were filled with distilled water slightly acidulated with
sulphuric acid; and some tow steeped in the same solution was
stuffed into the upper part of the porous cell around the pla-
tinum, so that this latter plate was perfectly excluded from light.
The extremities of the two plates weremetallically connected.
A brass cylinder, covered at the top, was placed over the whole,
its lower circumference resting on a circular pad of paper so as
to exclude light.

The apparatus, thus disposed, was set aside for ten days so as
to allow the local currents to subside. At the expiration of this
period the apparatus was taken into bright sunlight, the position
of the plates so arranged that the one in the outer or glass cell
should be opposite the sun, the terminals connected with the gal-
vanometer, and the temporary deflection occasioned by polariza-
tion allowed to subside, or rather to reach a fixed point, for there
was always a slight deflection.

The brass cylinder which excluded light from the apparatus
was now removed; and the galvanometer needle instantly deviated
to 10°, the platinum exposed to sunlight being positive to that in
the dark, or as zine to copper. The platinum plates were now reversed, that which had been in the outer cell placed in the porous cell, and vice versa, and the apparatus again set aside for ten days: at the end of this period it was again taken out, the experiment repeated, and the same result obtained; i.e. on removing the brass cylinder there was a deflection of 12°, the platinum exposed to light being positive to the sheltered one.

This identity of electrical effect taking place with the reversed plates seemed so strongly in favour of the impact of the solar rays having an initiatory effect in producing a voltaic current, that the only remaining point seemed to be to ascertain whether it was due to light or heat, to the chemical or calorific rays of the sun; yet the conclusion I then came to was erroneous, as will presently be seen.

In order to ascertain how far the effect was due to heat, I arranged, in a room lighted by a small candle, the same apparatus over a fire of asbestos heated by coal-gas, so that both radiant heat and an ascending current of hot air impinged on the side of the glass in which was the exposed platinum, while the opposite side was entirely sheltered from the heat by a metallic shelf on which the cell rested: this experiment was continued until one side of the cell was uncomfortably hot to the hand, while the other side was quite cool; but not the slightest deviation of the galvanometer took place.

I now repeated the former experiment with sunlight, changing the liquid each time. In three successive experiments the deflections on the impact of light were in the same direction, the exposed platinum being positive; but in a fourth the deflection was in the reverse direction, the exposed platinum being negative: in several subsequent experiments there was always a notable deflection which ensued on the impact of light; but it was sometimes in one direction and sometimes in the other. I ultimately discovered that, in the deflection produced by light, the needle of the galvanometer deviated in the same direction which it took upon the first contact of the wires connected with the platinum plates. The effect of light was therefore to increase the deflection occasioned by the polarization of the platinum plates; and this my subsequent experiments have, I think, fully established. Although the experiment on the impact of heat seemed to show that the heating effect of the solar rays was not the cause of the phenomena, yet it might well be that the solar rays absorbed by the platinum black would produce a greater heating effect at the actual point of contact of the platinum and liquid than any non-luminous heat would produce; and I was therefore anxious to ascertain whether the different-coloured rays of light showed any difference in their effects. To this end I procured three
plates of coloured glass, one blue, the second yellow, and the third red; a strip of thick brown paper was pasted to the opposite sides of each of these plates of glass, so as to form a nearly cylindrical chamber cut by the plane of the glass. A cover was placed over each of them; and the chambers so formed could be placed over the cell containing the platinum plates, the coloured glass plates intervening between the sun and the platinum in the outer cell. A great number of experiments were made with these apparatus; and in all the deviations of the galvanometer were notably greater with the blue glass than with the yellow or red, and, of the latter two, the yellow gave slightly greater deflections than the red glass.

This result is, I think, conclusive in favour of the effect being due to the chemical, not to the calorific rays of the sun, the more so when we consider that the yellow allowed a far larger quantity of light to pass than the blue glass. I may also add that I have obtained a slight galvanometric deflection when diffused daylight was allowed to impinge on the platinum plate, and when there was no perceptible difference of temperature between the illuminated and the non-illuminated plates.

The superiority of the yellow over the red was not so strongly marked; and, considering that the yellow glass allowed much more light to pass than the red, I am not disposed to think that there was any actual superiority in the former: the effects observed with these two colours are, however, corroborative of the effects not being due to the red or heating rays of the sun.

I substituted for the water acidulated with sulphuric acid (which may be regarded electrically as pure water with its conducting power improved), muriatic and nitric acids; the effects were the same, but less marked with the nitric acid, probably from its more completely depolarizing the plates.

In a small number of experiments the following effect took place. After a certain time of connexion with the galvanometer, the cover being over the apparatus, the sign of polarization changed; i.e. supposing the needle of the galvanometer to deviate to the left, and indicate that the exposed platinum was positive, the needle would gradually return, pass the zero-point, and be deviated to the right: when this was the case, on removing the cover, the effect occasioned by the impact of light was a return of the needle towards the zero-point, indicating an influence in the direction of the original polarization. On setting aside the apparatus for twenty-four hours with the plates in metallic connexion, and then repeating the experiment, the deviation of the galvanometer was in the direction of the final polarization. This apparent anomaly may have arisen from a conflict of two classes of currents, the one arising from imperfect mixing or want
of homogeneity in the liquid, and the other from the state of surface of the platinum; the latter would most probably be the current affected by light.

As the general effect of light was to increase the deflections occasioned by polarization, whatever direction this assumed, it seemed probable that the exclusion of one of the plates from the light, which I had commenced with in the hope of obtaining currents initiated by light, was unnecessary, and that the observed effects would be rather increased by exposing both plates to light. I therefore arranged two platinized plates in a cell without a porous diaphragm, inclined to each other like the letter V, but without contact, and allowed the light to impinge on the interior surfaces, or those opposed to each other; but, to my surprise, the effect was very trifling, the needle deviating only one or two degrees, and that in a sluggish and irregular manner.

When, however, the two plates were arranged parallel, the one shading the other from the light, as good deflections were produced as with the porous cell,—the more so if the back or shaded plate was of polished, and the front plate of platinized platinum.

Why light should produce a greater augmentation of the current when impinging on one than on the two plates, I cannot well understand, and therefore will not attempt any hypothetic explanation, but leave it for further experiment.

In all the experiments I have made on the subject of this paper, the most marked effect upon the galvanometer is produced when the polarization causes a small permanent deflection of from 5° to 10°; when the polarization of the plates is extremely slight, the effect of light is very feeble; and when the polarization is considerable, so as to deflect the galvanometer to 20° or 30°, the increased force required to produce a small increase of deflection is too great to afford notable results.

I have used the term polarization, having no better word to indicate the feeble currents which are always observed when two platinum plates immersed in a liquid are connected with a delicate galvanometer. The electrical currents which would ensue if the plates were polarized by connecting them with a voltaic battery and then detaching them, would be far too powerful for the delicate indications which I have been examining. There can be no doubt, at least to those who adopt the chemical theory of the voltaic pile, that both these classes of polarization are due, when one homogeneous liquid is employed, to slight deposits on the plates, either of films of gas, or of some substance which acts chemically on the liquids; and the effect of light would seem simply to be an augmentation of the chemical action taking place at the surface of the electrodes, which is the locus
where the chemical changes producing or produced by voltaic currents are always observable.

With more sensitive galvanometers, and with a greater variety of solutions, this class of experiments may, I venture to hope, be found important in further investigating the effects of light on chemical actions; and the pure coloured rays of the spectrum may be employed.

There can, I think, from analogy, be little doubt that light would influence those actions of surface which are comprehended among the various effects to which the term catalysis is applied. In an experiment I made in the month of September 1851, two similar glass tubes, containing each 15 grains of water, were placed, the one under an opake porcelain, and the other under a glass vessel of the same size, with capsules of sulphuric acid by their sides: I found that evaporation took place much more rapidly in the one exposed to light, though it was in a room with a northern aspect, on which the sun never shone. In twelve days the water under the glass vessel had lost 6.3 grains, that under the porcelain 5.4, showing a difference of nearly a sixth part in the evaporation in favour of the tube exposed to light. I mention this experiment here as showing a probability that the liberation of vapour or gas may be accelerated by light, as M. Donny's remarkable experiments seem to show that evaporation is a surface action; and the effect of light on polarized plates may be somewhat of the same nature. I will not, however, enter into theories, which must be necessarily vague on such a novel subject, but conclude by giving a Table of some of the most trustworthy results which I have obtained when the sunlight has been most steady, in order to show the extent of deflections and differences with coloured light: they were all made between the hours of 10 A.M. and 12, and on the finest days I could select. My first experiments were made in London, in the months of June and July. I was then absent for some time on circuit; and I have resumed and continued them during my vacation in the months of August and September.

June 21, apparatus having been arranged on June 12. Solution, water with a few drops of Sulphuric Acid.

Deflection by sunlight 8°, or, from 12°, the deflection by polarization, to 20°.
Exposed platinum positive.

July 4. Same solution, platinum having been changed on the 24th.
Deflection by sunlight of 9°, or from 1° to 10°.
Exposed platinum positive.
On the Influence of Light on the Polarized Electrode.

August 24. Solution, water with Sulphuric Acid.

| Deflection by polarization | 1 |
| Sunlight                  | 5 |
| Blue glass                | 3 |
| Yellow glass              | 2 |
| Red glass                 | 2 |

Exposed platinum positive.

August 25. Solution, water with Sulphuric acid.

| Deflection by polarization | 2 |
| Sunlight                  | 7 |
| Blue glass                | 4.5 |
| Yellow glass              | 3 |
| Red glass                 | 3 |

Exposed platinum positive*.

September 4. Very dilute Muriatic Acid.

| Deflection by polarization | 6 |
| Sunlight                  | 14 |
| Blue glass                | 9 |
| Yellow glass              | 7.5 |
| Red glass                 | 7.5 |

Diffuse daylight . . . 8

Exposed platinum negative.

September 6. Very dilute Nitric Acid.

| Deflection by polarization | 1 |
| Sunlight                  | 7 |
| Blue glass                | 3 |
| Yellow glass              | 2 |
| Red glass                 | 1 |

Exposed platinum negative.

September 8. Dilute Nitric Acid.

| Deflection by polarization | 3 |
| Sunlight                  | 6 |

Exposed platinum negative.

September 10. Dilute Nitric Acid.

| Deflection by polarization | 1 |
| Sunlight                  | 5 |

Exposed platinum positive.

Dilute Muriatic Acid. Polished platinum plates.

| Deflection by polarization | 4 |
| Sunlight                  | 10 |
| Blue glass                | 7 |
| Yellow glass              | 6 |
| Red glass                 | 5.5 |

Exposed platinum negative.

September 14. Two sets, one of polished, the other of platinized platinum plates, in dilute sulphuric acid without porous diaphragm.

The polished plates gave—

| Deflection by polarization | 2 |
| With sunlight              | 3 |

Platinized plates gave—

| By polarization            | 3 |
| With sunlight              | 4 |

The pair with black platinum was then tried with a porous cell; and as it was not set aside for more than a few minutes to allow the polarization to subside, the

| Deflection by polarization was | 15 |
| Increased by sunlight to      | 20 |

Exposed platinum positive.

* In a second cell with the same solution the exposed plate was negative, and the deflection increased by sunlight in the same direction as the polarization, from 2° to 10°.
This being covered, but kept in connexion with the galvanometer, the polarization gradually diminished, passed the zero-point, and took up an opposite deflection of 7°'5; the outer platinum being now negative, the effect of sunlight was now to reduce the

| Deflection to | 3 |
| Red glass, to | 6'5 |
| Yellow glass, to | 6'5 |
| Blue glass | 5'5 |

On September 16, this same cell having been set aside in the interim, with the plates metallically connected, the deflection by polarization was increased by sunlight, the exposed platinum being negative.

September 15. Black and polished platinum in glass cell without porous cell, the black platinum being towards the light, and the plain platinum shaded by it; the black platinum was positive to the plain.

| Deflection by polarization | 10 |
| Sunlight | 18 |
| Blue glass | 12 |
| Yellow glass | 11 |

After half an hour in connexion with a galvanometer,—

| Deflection by polarization had sunk to | 4 |
| Sunlight | 12 |
| Blue glass | 6 |
| Yellow glass | 4 |
| Red glass | 4 |

---

I. **On Monoacetate of Glycol, and on the Preparation of Glycol.**

By Dr. E. Atkinson*.

In making some experiments with bromide of ethylene, I had occasion to notice the great facility with which that substance acts on certain potash salts soluble in alcohol; and at the instance of my friend Dr. Debus, I tried its action on acetate of potash, expecting to find therein a ready method of preparing acetate of glycol, from which glycol may easily be obtained.

\[
\text{Bromide of ethylene} + 2\left(\frac{C^2H^4O}{K}\right)O = 2\left(\frac{C^2H^4}{O}\right)O^2 + 2\text{BrK.}
\]

This method would be quite analogous to that by which this

---

* Communicated by the Author.

interesting body was first obtained by Wurtz, its discoverer—that is, by the action of iodide of ethylene on acetate of silver; but an easier method is a desideratum, for Wurtz's method is unfortunately attended with many disadvantages. The preparation of iodide of ethylene, especially in large quantities, is difficult and tedious, the other substance concerned in the reaction is expensive,—while, from the high equivalents of iodine and of silver, the proportion of acetate of glycol obtained must in any case be small, and this is materially lessened by the fact that the acetate of glycol is not the only substance produced in the reaction.

The expectation that the above-mentioned reaction would furnish a ready means of obtaining glycol, has not been disappointed. The following method, which leads to the preparation of glycol, is easy and expedients of execution; the bromide of ethylene is readily obtained pure, and is comparatively inexpensive; and the reaction is remarkably precise and definite.

About 60 grammes of pure bromide of ethylene are placed in a soda-water bottle, along with an equal weight (very nearly two equivalents) of fused acetate of potash, and 4 fluid ounces of 80 per cent. alcohol. The bottle, well corked and tied down, is placed in a water-bath and heated to 100° C. for two days, and from time to time is well shaken. The bottle now contains a liquid and a large quantity of crystals of bromide of potassium, which latter are filtered off and washed on the filter with alcohol. The filtrate is then distilled in a water-bath until nothing more passes over at 100° C. This distillate consists of alcohol containing acetic ether, and a small quantity of free acetic acid. The residue is then transferred to a smaller retort in which a thermometer is inserted, and rectified, the portions passing over below 180° C., and above that point, being collected separately. Occasionally, owing to the presence of some bromide of potassium, the ebullition becomes so violent that it is necessary to stop the distillation*. In that case the residue in the retort is digested with its bulk of absolute ether, and filtered. From this ethereal solution the ether is first distilled off in the water-bath, and then the residue rectified, the distillate at and above 180° being collected separately.

The distillate above 180°, on being again rectified, consists almost entirely of a substance boiling at 181°-182° C. This is the

**Monoacetate of Glycol.**

It is a colourless, limpid, oily liquid, with a faint odour and a peculiar taste. It is perfectly neutral to litmus and turmeric

* When absolute alcohol is taken, this is much less likely to occur.
papers. It leaves an oil-spot on paper, which disappears after some time. It is a little heavier than water, and dissolves in it in all proportions. It is readily decomposed by potash and by baryta into acetic acid and glycol. Its boiling-point is from 181°-182° C. The following analytical determinations were made:—0·308 grm. gave 0·511 grm. carbonic acid and 0·2185 grm. water, which agrees best with the relation $C^4H^8O^3$, as is here seen:—

<table>
<thead>
<tr>
<th>Calculated.</th>
<th>Found.</th>
</tr>
</thead>
<tbody>
<tr>
<td>$C^4 = 48$</td>
<td>45·1</td>
</tr>
<tr>
<td>$H^8 = 8$</td>
<td>7·7</td>
</tr>
<tr>
<td>$O^3 = 48$</td>
<td>46·3</td>
</tr>
<tr>
<td><strong>104</strong></td>
<td><strong>100·0</strong></td>
</tr>
</tbody>
</table>

The formula $C^4H^8O^3$ is that of a monoacetate of glycol, $C^2H^4$; $C^2H^3O$ $O^2$. In order to control this, the following acetic acid determinations were made:—2·027 grms. of substance were enclosed in a tube with some moist hydrate of baryta, and the sealed tube heated in a paraffine bath for twenty-five hours to a temperature of 120°-130° C. The contents of the tube were then transferred to a flask, and carbonic acid passed through to saturation; the mixture was then heated for some time and filtered; and in the filtrate, which contained acetate of baryta, the baryta was determined as sulphate. In this way 2·136 grms. of sulphate of baryta were obtained, which correspond to 2·33 grms. acetate of baryta, or to 1·1 grm. acetic acid. According to the formula of monoacetate of glycol, $C^2H^3O$ $O^2$, it would yield, by decomposition with hydrate of baryta, one equivalent of acetate of baryta, thus:—

\[
\begin{align*}
C^2H^4 & \quad \text{Hydrate of baryta.} \\
C^2H^3O & \quad \text{Acetate of baryta.} \\
& \quad \text{Glycol.}
\end{align*}
\]

And 2·027 grms. of substance ought to give 2·485 grms. of acetate of baryta; the quantity obtained, 2·33 grms., corresponds therefore to 0·94 of an equivalent of acetic acid. The diacetate, $2(C^2H^4O)$ $O^2$, would have given 3·49 grms. acetate of baryta. In a second determination made by the same method, but with substance of a different preparation, 1·825 grm. gave 2·027 grms. 2 F 2
Dr. Atkinson on Monoacetate of Glycol,

sulphate of baryta, which corresponds to 0.996 of an equivalent of acetic acid*.

These results, together with its physical properties, leave no doubt that the substance in question is the monoacetate of glycol. The analytical results would correspond equally well to a mixture of equivalent quantities of glycol and acetate of glycol; but, on the other hand, its evidently homogeneous nature, its complete solubility in water (in which it differs from acetate of glycol), its boiling-point, which, in various preparations made with various quantities, was found to be constant between 181° and 182° C. (which is 5 degrees lower than that of diacetate of glycol and about 13 degrees lower than that of glycol), and the agreement in the quantity of acetic acid found in specimens of different preparation are all inconsistent with this assumption. In its formation, the elements of water take part, thus:—

\[ C_2H_4Br_2 + 2\left(\frac{C_2H_3O}K\right)O + H_2O = 2BrK + C_2H_3O + C_2H_4O^2. \]

Bromide of ethylene. Acetate of potash.

And part of this acetic acid forms acetic ether with the alcohol present.

The diatomic glycol stands in the same relation to the monoacetate of glycol as a bibasic acid (for example, oxalic acid) does to its acid salts, thus:—

\[ C_2H_4O^2 + C_2H_3O^2 = C_2H^4O + C_2H^4O^2. \]

Glycol. Monoacetate of glycol.

And monoacetate of glycol stands to the diatomic alcohol, glycol, in a similar relation to that in which Berthelot's monoacetine and diacetine stand to the triatomic alcohol, glycerine.

* The chloride may be substituted for the bromide of ethylene in the preparation of monoacetate of glycol; but as its action on the potash salt is less energetic, it is necessary to continue the heating in the water-bath somewhat longer. In other respects the preparation is the same. The experiment was not made on a sufficiently large scale to enable me to state with certainty what amount of product was obtained; but as the equivalent of the chloride of ethylene is little more than half that of the bromide (99 : 188), its employment would, ceteris paribus, be more advantageous. Bromide of ethylene yields nearly three-fourths of the theoretical quantity of pure monoacetate of glycol required by the equation.
It is isomeric with butylactic acid, $C_4H_9O_3$, the homologue of lactic acid obtained by Wurtz by the oxidation of amyle-glycol*.

From the distillate which passed over below 180°, a further quantity of monoacetate of glycol may be thus obtained. It is again rectified, and the portion distilling at and above 180° collected separately. The distillate below this point, which consists of acetic acid and monoacetate of glycol, is neutralized with baryta, somewhat evaporated in the water-bath, and then digested with ether. From this ethereal solution the monoacetate is obtained by rectification.

Some glycol was prepared from the monoacetate by Wurtz's method—distillation with hydrate of potash:—

$$\text{C}_2\text{H}_4\text{O} + \text{KIIO} = \text{C}_2\text{H}_3\text{KO}_2 + \text{C}_3\text{H}_4\text{O}_2.$$  

Monoacetate of glycol.

Monoacetate of potash.

To 14 grms. of monoacetate the equivalent quantity of fused and powdered potash was gradually added. The evolution of heat showed that an action was at once set up. The mixture was allowed to stand some time, and then distilled, at first in a sand-bath, and then with the heat of a spirit-lamp. The crude product thus obtained was rectified; the greater part of it had the constant boiling-point 193° C. Its identity with glycol is evident from the following combustion:—

0·170 grm. gave 0·2385 grm. carbonic acid and 0·147 grm. water.

<table>
<thead>
<tr>
<th></th>
<th>Calculated</th>
<th>Found</th>
</tr>
</thead>
<tbody>
<tr>
<td>$C^2$</td>
<td>24</td>
<td>38·70</td>
</tr>
<tr>
<td>$H^6$</td>
<td>6</td>
<td>9·67</td>
</tr>
<tr>
<td>$O^2$</td>
<td>32</td>
<td>51·63</td>
</tr>
</tbody>
</table>

The description of the properties of glycol given by Wurtz I

* Phil. Mag. October 1858.
can confirm. It is an oily liquid, heavier than, but soluble in water in all proportions, has a sweet taste, which its aqueous solution also shares. A glass rod dipped in the liquid takes fire with great difficulty, burning with a pale blue flame like that of alcohol. It is perfectly neutral to litmus and turmeric papers; it leaves an oil-spot on paper, which disappears after some time.

When bromide of ethylene is boiled with an alcoholic solution of acetate of potash in an open flask, an action takes place, and bromide of potassium is formed, but the ebullition soon becomes so violent that it is necessary to discontinue the operation.

Iodide of ethylene, when treated with acetate of potash in a closed vessel, undergoes a different decomposition. A large quantity of iodide of potassium is formed, and some iodoform, but no glycol compound.

Chloroform enclosed in a tube with acetate of potash had only a slight action when heated for some time to 100° C. On being heated to 150° C. the tube exploded. Bromoform and iodoform have also very little action at 100° C., and were not tried at a higher temperature.

These experiments were made in the laboratory at Queenwood College.

Queenwood College, near Stockbridge,
Hants, October 1858.

---

LI. On the Action of Caustic Potash on Glycerate of Potash.
By Dr. H. Debus*.

I HAVE shown on another occasion† that some of the salts of glyoxylic acid are decomposed, by treatment with solution of potash or caustic lime, into combinations of glycolic and oxalic acids‡:

\[ 2(C_3H_5KO_4) + KHO = C_3K_2O_4 + C_3H_3KO_3 + 2H_2O. \]


Glyceric acid, which is homologous with glyoxylic acid, might under similar conditions be resolved according to the equation

\[ 2(C_3H_5KO_4) + KHO = C_3H_2K_2O_4 + C_3H_5KO_3 + 2H_2O, \]

Glycerate of potash. Laetate of potash.

into lactate of potash, and the potash salt of an acid \((C_3H_4O_4)\) homologous with oxalic acid.

* Communicated by the Author.
† Phil. Mag. November 1856, and January 1857.
‡ C=12, H=1, O=16.
In order to decide this question, the following experiments were made:—

Glycerate of baryta was boiled with a concentrated solution of caustic baryta for several hours. On examination of the liquid, the substances employed did not appear to have undergone any perceptible change. 25 grms. of glycerate of potash were dissolved in a solution consisting of 25 grms. of caustic potash and 50 grms. of water, and the liquid evaporated at a high temperature until a small portion of it, after neutralization with acetic acid, gave a white precipitate on the addition of acetate of lead. The residue, which assumed the consistency of glycerine or strong syrup, as soon as it became cold was dissolved in water slightly acidulated with acetic acid, and acetate of lead added as long as a precipitate was formed. The examination of the filtrate from this precipitate will be described presently.

The lead precipitate, well-washed, was mixed with water and decomposed by sulphuretted hydrogen.

The solution separated from the sulphuret of lead gave, on evaporation, only crystals resembling oxalic acid. The latter dissolved easily in water, and produced in a solution of sulphate of lime a white precipitate, insoluble in an excess of acetic acid. With sulphuric acid the well-known reaction of oxalic acid was obtained. Some of the crystals, dissolved in water, neutralized with ammonia, and added to chloride of calcium, gave a white precipitate, which was well washed and dried at 100° C.

0.649 grm. of the substance thus obtained yielded, according to the usual method, 0.439 grm. of carbonate of lime. If it be supposed that the precipitate was oxalate of lime (C₂Ca₃O₄ + II₂O), 0.444 grm. of carbonate of lime ought to have been obtained.

The liquid from which the oxalate of lead had been precipitated was freed from lead by means of sulphuretted hydrogen, evaporated on the water-bath to the consistency of syrup, and mixed with as much dilute sulphuric acid as was necessary to saturate the potash which it contained.

Three times its volume of strong alcohol was now added, and the whole allowed to stand for some hours. The sulphate of potash which had separated was collected on a filter, washed with spirits of wine, and the alcohol distilled off from the filtrate. To remove the last traces of sulphate of potash from the acid residue left in the retort, it was mixed with absolute alcohol, filtered, and the filtrate again distilled. The retort contained an acid liquid, which was diluted with water, and neutralized with carbonate of lime at 100° C. After filtering off from a little sulphate and carbonate of lime, the liquid was concentrated by evaporation and left to stand. A large quantity of crystals, of the appearance of lactate of lime, separated. The latter were,
On the Action of Caustic Potash on Glycerate of Potash.

after recrystallization, dissolved in water and freed from the lime by the careful addition of oxalic acid. Carbonate of zinc was now added to the filtrate from the oxalate of lime, as long as an effervescence took place, and after some boiling the undissolved carbonate of zinc removed by filtering. The liquid gave on evaporation crystals resembling lactate of zinc, which served for the following determinations:

I. 0·384 grm., dried over sulphuric acid, lost at 100° C. 0·070 grm. water.

The same quantity of substance dissolved in water and precipitated with carbonate of potash, gave, according to the usual method, 0·108 grm. oxide of zinc.

II. 0·430 grm. from another preparation lost at 100° C. 0·080 grm. water. The residue, dissolved in water and precipitated with carbonate of potash, gave 0·119 grm. oxide of zinc.

Hence in 100 parts,—

<table>
<thead>
<tr>
<th></th>
<th>I.</th>
<th>II.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Water</td>
<td>18·23</td>
<td>18·60</td>
</tr>
<tr>
<td>Zinc</td>
<td>22·57</td>
<td>22·21</td>
</tr>
</tbody>
</table>

100 parts of lactate of zinc, prepared from the acid obtained by fermentation, contain—

\[ C^6 H^8 Zn^2 O^6 + 3H^2 O. \]

Water . . . . . 18·16
Zinc . . . . . . 21·93

The zinc-salt examined is therefore lactate of zinc; the shape of the crystals, as well as their other physical and chemical properties, agree with this conclusion. 5·244 grms. of a watery solution saturated at 16° C., left, after evaporation and exposure to a temperature of 100° C. till the weight of the crucible remained constant, 0·118 grm. of residue. 100 parts of water at 16° C. dissolve, therefore, 2·30 parts of the dry lactate of zinc prepared from glyceric acid. The Handbooks on organic chemistry state that the \( b \)-lactate of zinc requires 58 parts of cold water for its solution. The temperature at which the experiment was made is, however, not mentioned.

In the mother-liquors from the lactate of lime and zinc no other substances could be discovered.

On the supposition that the glycerate of potash, under the conditions related in this paper, is changed in a similar manner by caustic potash as glyoxylate of lime is by slaked lime, we should have the following equation for its decomposition:

\[ 2(C^3 H^5 KO^4) + KHO = C^3 H^5 KO^3* + C^3 H^2 K^2 O^4 + 2H^2 O. \]

* I employ for lactate of potash the formula \( C^3 H^5 KO^3 \), in order to express the decomposition in the simplest manner. The formula of lactic
C\(^3\)H\(^2\)K\(^2\)O\(^4\), which has the same composition as the malonate of potash of Dessaignes, is, according to this view, decomposed, at the high temperature by which the experiment was made, into oxalate and formiate of potash:

\[
C\(^3\)H\(^2\)K\(^2\)O\(^4\) + KHO + H\(^2\)O = C\(^2\)K\(^2\)O\(^4\) + CHKO\(^2\) + 4H.
\]

Oxalate of potash. Formiate of potash.

Oxalic, lactic, and formic acids were found amongst the products of decomposition of glycerate of potash.

According to an experiment made by my friend Dr. Atkinson, glycerate of potash is broken up by fusion with caustic potash into acetate and formiate of potash:

\[
C\(^3\)H\(^5\)K\(^4\) + KHO = C\(^2\)H\(^3\)K\(^2\)O\(^2\) + CHKO\(^2\) + H\(^2\)O.
\]


Queenwood College, near Stockbridge,
Hants.

---

LIII. On the Properties of Electro-deposited Antimony.

By G. Gore, Esq.*

1. WHILE engaged in depositing antimony by the electro-process in October 1854, I observed a remarkable phenomenon of development of heat in the deposited metal when gently struck, and published a brief account of it in the Philosophical Magazine for January 1855; since that period I have investigated the phenomenon, and have now the honour of laying the results before the Royal Society.

2. In depositing this metal by the electro-process, several solutions may be successfully used, consisting of compounds of the metal either with hydrochloric or tartaric acid; but those I have generally operated with have consisted,—1st, of ordinary chloride of antimony (as prepared for pharmaceutical purposes)

acid, as corresponding to its salts, is nevertheless C\(^6\)H\(^2\)O\(^6\); because it forms with bases two series of salts, yields at 130\(^\circ\) C. an anhydrous acid, C\(^6\)H\(^10\)O\(^3\), and with ammonia an amidogen acid, all derived from the formula C\(^6\)H\(^12\)O\(^6\). But lactic acid has also a second rational formula, C\(^3\)H\(^6\)O\(^3\), according to which it is also dibasic, as shown by the recent experiments of M. Wurtz. Lactic acid has two modes of entering into combination and decomposition: the one requires always one molecule, C\(^3\)H\(^6\)O\(^3\), the other two molecules, 2(C\(^3\)H\(^6\)O\(^3\)) = C\(^6\)H\(^12\)O\(^6\), for its realization, according to the conditions of the experiment.

All those who fancy that a chemical formula tells something about the arrangement of atoms, &c., must be, of course, of a different opinion.

* From the Philosophical Transactions, Part I. for 1858, having been read at the Royal Society December 10, 1857.
nearly saturated with antimony, by suspending a plate of that metal in it as an anode, and passing an electric current from several pairs of zinc and silver batteries through it until a copious deposit of bright metal occurred, and until its yellow colour had nearly disappeared; and 2nd, of five parts of tartar-emetic and five parts of tartaric acid, dissolved in a mixture of two parts of hydrochloric acid and thirty parts of water; each solution being filtered before using. A most excellent solution in lieu of the first may quickly be formed by saturating ordinary chloride of antimony with tartar-emetic, using about three or four parts of the former to one of the latter, or by dissolving two parts of tartar-emetic in three parts of hydrochloric acid. I have also in place of that liquid occasionally used hydrochloric acid saturated with antimony by the battery process, and sometimes a liquid composed of seven parts of tartar-emetic dissolved in a mixture of eight parts of hydrochloric acid and four parts of water. Substituting pure acid, distilled water, and pure antimony, for those of the ordinary quality, made no material difference in the results. Each solution requires to be electrolysed a short time before it yields a good and uniform deposit.

3. There are two distinct kinds of antimony deposit produced in these liquids, which I shall distinguish by the terms amorphous and crystalline; dark and gray: for the special production of the former I use the first liquid, or, better, that composed of chloride of antimony and tartar-emetic; and for the latter I use the second: the first solution will yield both varieties, but the second gives the crystalline variety alone. The line of demarcation between the two kinds of deposit is rigorously distinct; and there appears to be no transitional or intermediate gradation, at least not by this mode of procedure.

4. There are a great many circumstances which influence the production of those two kinds of metal in the first liquid; but the most important are the temperature and density of the solution, the strength of the battery, the state of the depositing liquid, and the physical condition of the receiving surface; if the battery is nearly exhausted in power, the solution unequal in density from long-continued working and neglected stirring, and the receiving surface rough or unclean, the deposited metal will be amorphous and smooth in the lower and denser part of the liquid, and crystalline and warty in the upper and lighter portion. If the liquid is boiling hot, or if it is very dilute or contains much free acid, the deposit will be gray throughout.

5. The production of either of these deposits is so easy that the simplest of conditions will suffice. To produce the amorphous variety, take a small battery consisting of one or two pairs of zinc and platinized silver plates, charged with a mixture of
one measure of sulphuric acid and twenty measures of water, and having prepared a small quantity of the mixture of chloride of antimony and tartar-emetic, connect with the terminal silver a piece of commercial antimony ("best French regulus") of the same amount of immersed surface as one of the battery-plates, and immerse it in the liquid as an anode, and for a receiving surface immerse a piece of clean polished silver or copper of about one-half the amount of surface of the antimony; connect it by a silver or copper wire with the negative pole of the battery; suspend it vertically in the lower part of the depositing liquid at a few inches from the anode, and protect the connecting wire from receiving a deposit near the surface of the liquid by a piece of gutta-percha tube, glass tube, or by a layer of varnish formed by dissolving pure gutta percha in bisulphide of carbon to the consistence of treacle. A perfectly bright deposit of amorphous antimony will immediately be formed, and will, if the electric power be rather great, exhibit a scaly surface after half an hour's action; but by further continuance the sealiness will cease, and a firm coherent deposit of perfect brightness, and of similar appearance to highly polished steel, will be produced. If the electric power is too great, the deposit will be granular and loose. Much impure matter will appear upon the anode during the process, especially if "common bowl regulus" is used, and may be removed by means of a hard brush and dilute hydrochloric acid. There is no depositing liquid with which I am acquainted, of any metal, which will bear so great a variation of electric power without deteriorating the quality of the deposit, as that composed of tartar-emetic dissolved to saturation in hydrochlorate of terchloride of antimony: good amorphous deposits have been obtained in it with batteries varying from one to thirty-six pairs in intensity.

6. To produce a crystalline deposit, prepare a similar quantity of the second liquid, and proceed in exactly the same manner, except that a much larger receiving surface should be used in consequence of the greater tendency to a sandy deposit, and the protecting tube or varnish may be omitted. In this case the deposit is gray, and frequently of a dull aspect, or even a dark loose granular powder if the power is too great; but when properly produced it has a silky lustre or semi-iridescent silver-gray appearance, especially when viewed within the liquid; and if the process is continued several days, or, better, for one week, the edges of the deposit assume a partly nodular form. The cleaning of the anode in this case may be of less frequency, and with water alone.

7. A speed of deposition, varying from 0·75 to 3·0 grains per square inch of receiving surface per hour, has repeatedly yielded
a good bright deposit with the first-named solution; and from
0.25 to 2.25 grains per square inch per hour has often given a
good crystalline deposit with the second liquid. By keeping
the cathodes in constant motion, much more rapid action may
be successfully ventured.

8. By similar means to those described, and without the aid
of motion, a solid bar of dark bright antimony, upwards of
1 inch in diameter and 3 inches long, was formed upon a piece
of copper wire \( \frac{1}{16} \) th of an inch thick and 2 inches long, in a
period of twenty-eight days, and with the aid of motion in about
half that time.

9. In depositing antimony from these solutions, a variety of
circumstances occur, a knowledge of which will greatly assist in
obtaining perfect specimens of the two varieties. In using the
ordinary chloride, after the process has been progressing one or
two days with continuous bright deposit, warty excrescences of
the gray variety will nearly always occur upon portions of the
receiving surface, the localities of their appearance differing
according to the shape of the cathode and its depth of immersion:
if it be within half an inch of the surface of the liquid, rough
gray metal will appear upon its upper parts. The presence of
these excrescences appears in nearly all cases to depend upon
accumulations of partly exhausted solution at the upper part of
the liquid, and at different parts of the receiving surface. The
solution, partly deprived of its metal by the action, and thus
rendered specifically lighter, ascends in a layer against the
cathode, and accumulates at its over-hanging parts and at the
surface of the liquid, and yields in those exhausted or acid por-
tions only the gray deposit; similar effects, but in a much less
degree, occur in the solution of chloride of antimony and tartar-
emetic. To obviate these effects, the solution should be fre-
cquently stirred; and to entirely prevent them, the cathode should
be immersed at least 1 inch below the surface of the liquid, and
be kept in a constant state of gentle swinging motion by attach-
ment to the moving pallets of an ordinary clock. Roughness or
want of cleanliness of the receiving surface also seems to favour
the production of these excrescences.

10. On every occasion with the first liquid, when the battery
power became nearly exhausted, gray nodules appeared; and when
once commenced, there was a strong tendency in them to con-
tinue; but by increased electric power, stirring the liquid, and
persistent action, they become covered with amorphous deposit.

11. If during the process of deposition the temperature of the
first solution was gradually raised, the deposited metal suddenly
changed from amorphous to crystalline at about 205° Fahr., and
less suddenly resumed its amorphous character on cooling, at
about 79° Fahr.; on interposing a galvanometer, the conductivity of the circuit was found to be gradually increased during the rise of temperature, and gradually decreased during the fall. These phenomena were repeatedly produced by reheating the same portion of liquid; and similar effects were obtained with the solution composed of tartar-emetic and dilute hydrochloric acid. The first liquid exhibited no material irregularity in its rate of cooling, indicative of molecular change, between 220° and 100° Fahr. By previously evaporating the same solution to one-half its original bulk, its tendency to yield a gray deposit by heat was considerably reduced; and by diluting it either with pyroxylic spirit, dilute hydrochloric acid, or solution of table-salt, its aptitude to give gray metal at ordinary temperatures was increased and its depositing quality injured.

12. Upon examining the electric relations of the depositing liquids with platinum plates in the apparatus described at page 1 of the Philosophical Magazine, January 1857, hot platinum was found to be increasingly electro-negative to cold platinum up to 210° Fahr. in the tartaric-acid solution, in accordance with the general rule there stated; but with the first liquid an unusual variation occurred, hot platinum being increasingly negative to cold platinum up to 110° Fahr., then decreasingly to 145° Fahr., and above that temperature increasingly positive to 210° Fahr. Similar effects were obtained with small square columns of Duboscq's carbon (made by him for electric light apparatus) immersed in those liquids in V-tubes. In each case the solution was previously boiled, and repeated experiments gave uniform results.

13. By separating the first and second solutions from each other by means of a porous diaphragm, and immersing two perfectly similar pieces, either of platinum or of Duboscq's carbon, one in each liquid, and connecting them with a galvanometer, that in the chloride solution was in each case found to be rather strongly electro-positive to that in the tartaric-acid liquid.

14. In depositing from either of the solutions which yield the amorphous metal, especially ordinary chloride of antimony nearly saturated with antimony by the battery process, there is a great tendency in the deposit to extend itself in the form of a thin sheet of gray metal upon the surface of the liquid: this lateral spread of the metal is unique, and increases rapidly; and, like the gray excrescences formed upon other parts of an amorphous deposit, its speed of increase is much greater than that of the dark metal near it (see 10); the reason of this appears to be that crystalline antimony is more electro-negative than the amorphous variety, and therefore receives the electric current with greater facility. I have found with perfect specimens of gray and dark
antimonous variety was electro-positive to the crystalline in the following liquids: — dilute phosphoric, sulphuric, hydrochloric, and nitric acids; in aqueous solutions of ammonia, potash, and carbonate of soda; also in the first-mentioned chloride solution. This difference of electric condition was in some instances of deposition so great as to generate a separate electric current, and completely redissolve some of the dark portions. The phenomenon of lateral surface deposit did not occur in the tartaric-acid solution. Antimony trees, consisting of branches of amorphous and crystalline nodules, were easily formed by suspending a horizontal copper wire as a cathode upon the surface of the perchloric solution.

15. Heat was perceptibly evolved in the first-named liquid during deposition; on one occasion, with a feeble battery, when the external air was at 66° Fahr., the coldest part of the liquid, viz. behind the anode, was at 67°.5 Fahr., and the warmest part in front of the anode 71° Fahr. And on another occasion, with a stronger battery (consisting of twenty-four pairs of zinc and silver plates) weakly excited, with an anode of moderate size, a thermometer in contact with the front of the anode rose from 74°.5 to 87°.5 Fahr.

16. Faint crackling sounds, audible at about 4 or 5 feet distant, frequently occurred in the depositing liquid during the process; they issued from the receiving surface, and appeared to be due to alterations in the cohesive state of the metal, being generally attended by the production of cracks, and occasionally of complete rents in the metal; they most frequently occurred when the temperature of the deposit was changed by removal from the liquid or by reimmersion. In one instance metallic sounds were definitely traced to bubbles of gas suddenly expelled with force from a small spot of a very firm deposit formed upon a bar magnet in the tartaric-acid liquid; but no fissure or orifice was observable at the parts; they were unattended by evolution of light.

17. In common with electro-deposits generally, the inner and outer surfaces of these deposits are in unequal states of cohesive tension, frequently in so great a degree as to rend the metal extensively and raise it from the receiving surface during the process in the form of a curved sheet with its concave side towards the anode. This separation is very apt to occur in depositing upon extended flat surfaces, and does not often occur when the form of the surface is unfavourable, as with wires, rods, bars, &c., where the deposit extends around the mass, or where it passes round projections, edges, &c., and retains a hold thereby. This state of tension is most manifest in thin deposits, and especially in the first-formed layer; for by depositing a thin layer
of amorphous antimony upon gold-leaf attached by varnish to a tube of glass, the film of gold was quickly raised from the surface, and curled into fantastic shapes by the cohesive action. Both varieties of the metal exhibit this phenomenon.

18. The fracture of amorphous antimony is conchoidal, smooth, and wavy; that of the gray variety is crystalline, radiating like hematite. The texture of the former is rather soft and weak, that of the latter is quite hard and strong. The amorphous metal files easily, the gray with more difficulty. The gray metal, if struck by a hard substance, emits a clear metallic sound, whilst the dark variety gives a more dull tone. The crystallization lines of gray antimony are at right angles to the receiving surface; and in several experiments of depositing upon magnets, the direction of those lines did not seem to be altered by the magnetism.

19. The amorphous variety, if gently struck by a hard substance, undergoes a rapid and intense change throughout its mass, attended by development of considerable heat; the crystalline kind undergoes no such change, apparently, under any circumstances. The change appears to be molecular in its character; and according to this view the crystalline variety of metal must be regarded as being in a state of comparatively stable molecular equilibrium.

20. The following are a few selected instances of this phenomenon. Antimony was deposited to the 10th of an inch thick upon a permanent bar magnet immersed vertically in the solution of tartar-emetic and dilute hydrochloric acid; the deposit was well washed, and after remaining in this state two days, I struck it gently with a small piece of iron; it at once shattered all to pieces with great evolution of heat and with a small cloud of vapour, emitting an adour of burnt tartrates and hydrochloric acid.

21. On another occasion a deposit of an inch thick, formed upon one side of a copper medallion in the same liquid, had been removed from the solution and lain in water thirty-six hours; I then wiped it dry and momentarily applied the flame of a candle to a very small portion of its edge; the whole at once changed with exceedingly great evolution of heat.

22. Once having by accident feebly struck a deposited bar of an inch thick against the glass containing vessel beneath the surface of the liquid, the metal was immediately shattered throughout, many pieces fell to the bottom of the vessel, and a powerful suppressed bubbling noise, as of heated metal plunged into water, occurred.

23. At another time, when examining the thermo-electric properties of a bar 4\(\frac{1}{2}\) inches long and 1\(\frac{1}{4}\) inch thick, on bring-
ing a heated brass wire near to a very small portion of one end of the bar, a sudden change or commotion of particles took place throughout the mass, passing rapidly from the heated part to the opposite end, attended by the production of a few very minute cracks, and with great evolution of heat, rapidly boiling a few drops of water placed upon it.

24. And on another occasion, on accidentally touching a cathode covered with perfect amorphous deposit against the anode, the change with considerable hissing noise occurred, the electric spark or current from the battery being in this case the apparent exciting cause of the phænomenon.

25. And, finally, with a rapidly-formed and perfect specimen of dark antimony deposited upon a swinging helix of fine copper wire, on washing it first in dilute hydrochloric acid and then in water, and gently striking it with a piece of glass, it at once shattered into small particles with considerable force, projecting some of them to a distance of 10 feet, apparently by converting into steam the adhering particles of water.

26. This thermic property of amorphous antimony is retained apparently unimpaired upwards of 2½ years; a piece deposited in October 1854 evolved considerable heat by slight percussion in May 1857. The change appears to be similar to the molecular changes of sulphur, selenium, iodide of mercury, &c., observed by Ritteroff, Regnault, Weber and others. I have never found that the phænomenon could be repeated with the same specimen.

27. The only observed exciting causes of the phænomenon have been ordinary heat, concentrated solar heat, friction, percussion, fracture, and the voltaic spark. Heat appears always to determine it, whilst, with thin deposits, slight percussion and even fracture have frequently failed. Thick pieces will bear gentle filing, but not violent fracture, at ordinary temperatures without the change occurring.

28. The production of the change and the rapidity of its action are considerably dependent upon the temperature of the mass; pieces which would easily fracture without further change in cold water would not break without change in the air at 60° Fahr.; the certainty and quickness of the action were also manifestly greater when the metal had been previously warmed.

29. Immersing the metal in water and gradually heating it to 212° Fahr. did not cause the change, destroy the property, or have any material effect upon the rate of heating of the liquid. Nor did sudden immersion in boiling water, or partial dissolving of the metal by warm aqua-regia, cause it, or destroy the property; but slight percussion on removing it from the hot water caused it powerfully.
30. At the suggestion of Mr. Faraday, and with the kind assistance of him and Dr. Tyndall, I have been enabled to reduce pieces of the metal of moderate thickness (\(\frac{1}{10}\)th of an inch) to a state of fine division without exciting the change or destroying the thermic property, by carefully triturating them in a mixture of broken ice and salt. Pieces of the thickness of an address-card were reduced to powder without the change occurring, by careful pulverization in water at 60° F.; the most ready plan adopted was by gently pressing the pieces in small quantity, under water, in a mortar, with a rolling motion of the pestle (not by blows or rubbing) until they attained the size of a pin's head, listening all the time to detect molecular changes, then reducing the particles to complete powder by gentle rubbing.

31. By placing small quantities of the dried active powder upon thin sheets of metal and gradually applying heat until the change occurred, sufficient heat was evolved by the molecular force to commence oxidation, which spread at a much lower speed throughout the mass, producing true combustion, and raising it to a temperature of dull redness visible in daylight. The molecular change in the powder is attended by a visible commotion amongst the particles. Portions of the gray variety reduced to powder and treated similarly, exhibited the same combustion.

32. It would appear from these experiments that amorphous antimony is susceptible of three different, distinct, and independent changes:—1st, the cohesive change of its mass, consisting of extensive fractures and bending; 2nd, the peculiar change, attended by evolution of heat; and 3rd, a chemical change, consisting of oxidation and true combustion; whilst the crystalline variety is susceptible of only two of these changes. Antimony, deposited in the state of a black powder upon a small cathode, by rapid action in the solution of tartar-emetic and chloride of antimony, exhibited no perceptible change of the second kind, but manifested rapid combustion by application of heat; and similarly with antimony powder deposited from a solution of antimonous oxide in pyrophosphate of soda. Deposits of metal, obtained from solutions of tersulphide of antimony in caustic potash, exhibited the phenomenon of unequal cohesion, but not the peculiar change. Fused terchloride of antimony, free from water, with an anode of antimony and cathode of copper, did not conduct, even in a small degree, the electric current from five pairs of zinc and silver batteries. By rotating a horizontal silvered disc of thin brass, 1½ inch diameter, in the solution of tartar-emetic and chloride of antimony, at a speed of 3500 revolutions per minute for thirty-three minutes, and simultaneously passing a current from five zine and silver batteries through it.
by means of a disc of antimony \(\frac{2}{3}\) inches diameter, a scaly deposit of bright black metal was obtained, possessing the usual thermic property.

33. In every case where the deposited metal was dark and shining, it evolved heat by the exciting causes, and under the conditions named; and in every case where it was gray and crystalline, it would not evolve heat.

34. The physical properties of the metal appear to be closely connected with the condition of the liquid; for in every case where the latter was sufficiently changed, whether by alteration of chemical composition, of density, of temperature, or of the electric power, a change occurred in the colour and brightness of the deposit; at the same time, by experimentally examining the influence of each of these circumstances separately, I did not find the peculiar state of the metal to be invariably connected with any one of them.

35. On immersing a piece of the unchanged metal in mercury, and applying heat very gradually, it suddenly changed with great force at a temperature of about 250° F.

36. A deposit, consisting of 279.5 grains of amorphous antimony and 101.5 grains of the gray variety, was formed upon a spherical bulb of a thermometer, and, after well washing and drying, was wrapped in cotton-wool and touched with a small heated wire: it at once changed; and the mercury rose from 75°.2 F. to 275°.2 F. in twenty-eight seconds. The diameter of the glass bulb was \(\frac{1}{3}\) in. of an inch.

37. A second deposit, consisting of 504.49 grains of dark antimony, formed upon the same bulb, and treated in a similar manner, raised the temperature of the mercury from 75° F. to 445°.0 F. in 1 minute 40 seconds. The thickness of the layer of metal was \(\frac{1}{4}\) th of an inch; and by the great heat evolved it acquired a dull gray aspect, similar to the gray variety.

38. A cylindrical bar of the amorphous metal, about \(\frac{1}{2}\) an inch in diameter, formed upon a rod of grain tin \(\frac{1}{4}\) th of an inch thick, when changed by the momentary application of a heated wire, evolved sufficient heat to melt the tin completely, which ran out through a crack in the antimony, and remained fluid a short time. A bar of bismuth, \(\frac{5}{16}\) ths of an inch thick, coated to about \(\frac{3}{4}\) ths of an inch in diameter, did not melt by the change.

39. By immersing a perfect bar of the dark variety, weighing 450.9 grains, in 836.6 grains of distilled water, contained in a large thin glass tube wrapped in cotton-wool, immersing the bulb of a thermometer in the water, and allowing the whole to acquire a uniform temperature, then inducing the change by momentary contact of a heated wire, and stirring the water to render it uniform, the temperature of the water rose from 63°
to 82° F. The cooling influence of the water, in this and in several similar experiments, appeared to prevent the metal undergoing its full amount of change.

40. The peculiar change is attended by alterations in the colour and fracture of the metal; from a bright steel colour and glassy fracture it passes to a dull gray colour and granular fracture, as if it progressed a stage towards the crystalline modification; this is particularly manifest in thick pieces gradually heated in the air until the change occurs. It is also attended by a change in the form of the metal: bars which were nearly straight before the action, were considerably curved afterwards; and the direction of the curvature was such, that the outer side, or that last deposited, invariably became more concave. Upon applying heat to a thin layer of dark antimony, formed upon a thin sheet of silver, it immediately underwent the change, and became exceedingly curved in a similar direction. This direction of curvature appears to be due to a cause already mentioned (see 16, 17), the unequal cohesion of the inner and outer surfaces of the metal.

41. By applying heat to the ends of deposits formed upon heliacal copper wires, the action was gradually propagated to the opposite ends at a speed varying from 12 to 30 feet per minute; and its rapidity of progress appeared to be dependent upon the temperature of the metal and the cooling influence of the enclosed wire, also upon the absence of gray nodules: the warmer the antimony, the greater its thickness; and the more free it was from gray metal, the more rapid was the action: and in cases where the deposit was thin in places, or had occasional gray nodules, the action was impeded at those parts, and sometimes totally arrested. Gray nodules invariably impeded the action, and caused it to progress by sudden starts. The progress of the change was rendered more or less visible by the evolution of a cloud of vapour at its advancing point; also in the case of fine copper wires, by a peculiar snake-like movement in the coils at that part, apparently produced by the cohesive alteration already mentioned (see 16, 17, 40). After the change, the layer of antimony contained numerous transverse and longitudinal fissures, as if it had contracted in those directions.

42. In a number of instances, by connecting the ends of the copper wires with a galvanometer, and inducing the change in the coating by a heated wire, a feeble electric current occurred in the enclosed wire whenever, from thinness of the coating, its termination, or other causes, the action ceased, and in every case passed in a direction opposite to that of the peculiar action. By touching the deposit with a heated wire some time afterwards at the point where the action ceased, a current of ordinary
thermo-electricity was in each case generated, and passed in a
similar direction.

43. By exciting the peculiar change in two pieces, weighing
upwards of 600 grains each, placed in the vicinity of the poles
of delicately suspended magnetic needles, no deflection of the
latter in either case occurred.

44. By taking two bars, each about 3 inches long, one of
amorphous antimony and one of the crystalline variety, connect-
ing their extremities with a galvanometer, and cautiously bring-
ing a small flame of a spirit-lamp within half an inch of their
remote and touching ends, the dark bar was found to be feebly
thermo-electro-positive to the gray one. A thin rod of changed
antimony was found, in a second experiment, to be weakly
thermo-positive to a large lump of gray metal. And in a third
trial, a large changed bar was positive to a bar formed by melt-
ing and casting deposited antimony.

45. Both the gray, the changed, and the unchanged metal
acquired a coating of dendritic crystals of silver by immersion
in a solution of nitrate of silver.

46. The following are some determinations of the specific
gravity of the deposits:

<table>
<thead>
<tr>
<th>Three specimens of unchanged antimony.</th>
<th>Five bars of unchanged antimony.</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. 1. 5·739 at 70° Fahr.</td>
<td>No. 4. 5·766 at 60° Fahr.</td>
</tr>
<tr>
<td>No. 2. 5·924 at 62</td>
<td>No. 5. 5·769 at 62</td>
</tr>
<tr>
<td>No. 3. 5·944 at 62</td>
<td>No. 6. 5·782 at 62</td>
</tr>
<tr>
<td></td>
<td>No. 7. 5·785 at 62·5</td>
</tr>
<tr>
<td></td>
<td>No. 8. 5·814 at 61·5</td>
</tr>
</tbody>
</table>

The bar No. 4 weighed 467·00 grains; and by immersing it in
750 grains of distilled water at 60° F. and inducing the change,
it evolved a little chloride of antimony, and acquired a density of
5·818 at 60° F. The bar No. 7 weighed 456·9 grains; and by
changing it in 836·6 grains of water at 63° F. (see 39), it ac-
quired a density of 5·862 at 62°·3 F. And the bar No. 8, changed
in the air, acquired a density of 6·186 at 61°·5 F.

<table>
<thead>
<tr>
<th>Four specimens of changed antimony.</th>
<th>Ten specimens of gray antimony.</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. 1. 5·748 at 75° Fahr.</td>
<td>No. 1. 6·369 at 75° Fahr.</td>
</tr>
<tr>
<td>No. 2. 5·818 at 60</td>
<td>No. 2. 6·444 at 72</td>
</tr>
<tr>
<td>No. 3. 5·880 at 62·5</td>
<td>No. 3. 6·497 at 72</td>
</tr>
<tr>
<td>No. 4. 6·029 at 64</td>
<td>No. 4. 6·522 at 73</td>
</tr>
<tr>
<td></td>
<td>No. 5. 6·557 at 75</td>
</tr>
<tr>
<td></td>
<td>No. 6. 6·616 at 72</td>
</tr>
<tr>
<td></td>
<td>No. 7. 6·616 at 75</td>
</tr>
<tr>
<td></td>
<td>No. 8. 6·662 at 75</td>
</tr>
<tr>
<td></td>
<td>No. 9. 6·671 at 75</td>
</tr>
<tr>
<td></td>
<td>No. 10. 6·673 at 75</td>
</tr>
</tbody>
</table>

Nos. 1 and 2 of the gray specimens were deposited from the
Electro-deposited Antimony.

upper part of the solution, composed of tartar-emetic and dilute hydrochloric acid; the remainder were deposited from the tar
taric-acid liquid, Nos. 9 and 10 being formed very slowly. The
greater specific gravity of the crystalline metal appears worthy of
consideration in connexion with similar differences of density of
amorphous and crystalline sulphide of antimony*.

47. The densities of both varieties changed to that of ordi
nary antimony by fusion; several specimens were fused under
tarsulphide of antimony, also under cyanide of potassium, and
their specific gravities were as follows:—6·67; 6·694; 6·71 at
76° F.; 6·75 at 73° F.; and 6·83 at 75° F.

48. I have endeavoured to determine the electro-chemical equi-
valent, or equivalents, of the two deposits; and by repeated expe-
riments with two perfectly similar batteries, one for depositing
the metal, and the other for determining the loss of zinc by ordi
nary chemical action, and deducting the average loss of zinc in
the cells of the idle battery from that occurring in those of the
working one, and excluding, as far as possible, all other sources
of error, I found in one experiment that for every 32·6 parts of
zinc dissolved, 43·38 parts of amorphous antimony were depo-
sited in the solution of tartar-emetic and dilute hydrochloric
acid; in a second experiment 43·61 parts of antimony were de-
posited; and in a third experiment 43·71 parts; and of gray
antimony, from the tartaric-acid solution, 41·1 parts.

To exclude the possibility of difference of local action in the
two batteries, I arranged three depositing liquids in succession: 
viz. 1st, the saturated solution of chloride of antimony (i. e. sa-
turated with antimony by means of a battery); 2nd, a solution
composed of 4 parts of sulphate of copper, dissolved in a mixture
of 1 part of pure sulphuric acid and 20 parts of distilled water;
and 3rd, the tartaric-acid solution of tartar-emetic. A current
from twelve pairs of zinc and silver batteries was passed through
these liquids by suitable electrodes of copper and antimony, and
the weights of the respective deposits, after washing and drying,
ascertained; and I found in the following experiments that for
31·7 parts of copper deposited in the sulphate solution, the weight
of perfect crystalline deposit varied from 40·41 to 10·797 parts,
and of perfect amorphous deposit, from 42·304 to 43·813 parts.

<table>
<thead>
<tr>
<th>Crystalline deposit.</th>
<th>Amorphous deposit.</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. 1. 40·11 parts.</td>
<td>No. 1. 42·304 parts.</td>
</tr>
<tr>
<td>No. 2. 40·612</td>
<td>No. 2. 43·112</td>
</tr>
<tr>
<td>A No. 3. 40·659</td>
<td>A No. 4. 43·160</td>
</tr>
<tr>
<td>B No. 4. 40·707</td>
<td>B No. 5. 43·199</td>
</tr>
<tr>
<td>No. 5. 40·797</td>
<td>B No. 6. 43·238</td>
</tr>
</tbody>
</table>

Nos. 3 of gray deposit, and 4 of amorphous deposit, were formed by the same current; and Nos. 4 of gray and 6 of amorphous deposit, were also formed by one current. Nos. 1 of gray, and 3 and 7 of amorphous deposit, were formed upon copper-wire helices kept in a state of constant motion, No. 7 being formed by very slow action.

49. In a separate experiment, with two-pairs battery, the equivalent of crystalline metal obtained was 40.728, and of amorphous metal 43.989; and in a very reliable experiment with a moving helix, the equivalent obtained of amorphous antimony was 43.284. With a one-pair battery, an equivalent of 44.95 parts of amorphous metal was obtained.

50. One source of variation in the resulting numbers of the dark variety appeared to arise from particles of the depositing liquid being mechanically enclosed within the depositing metal, chiefly by the formation of minute cracks and scales; these were, as far as possible, prevented by using cathodes formed of wire, maintaining a uniform temperature and an uninterrupted electric current, by suitable adaptation of the electric power, and by keeping the cathodes in constant motion. The adhering liquid was removed by washing in dilute hydrochloric acid, distilled water, and drying.

51. The amorphous metal invariably contained a small portion of chloride of antimony, more or less of which was always evolved during the molecular change, according to the temperature to which it was raised; thin pieces, changed under water, still retained the greater portion of the chloride. A thin specimen, formed upon a copper wire helix, lost by change in air 0.337 per cent. of its weight, and a thick one lost 3.556 per cent. The gray variety, when perfectly prepared, evolved scarcely a perceptible odour of burnt tartrates or hydrochloric acid by the application of heat.

52. To remove the chloride of antimony, portions of thin deposits were finely pulverized in cold water, then washed either with distilled water, dilute hydrochloric acid, or solution of caustic potash, finally with water and dried; small quantities of the chloride were removed, especially by the dilute acid; but in each case the powder still retained its thermic property, and evolved chloride of antimony during the change. Other portions of the pulverized metal were shaken with boiling hot dilute hydrochloric acid, also boiled in similar acid; the chloride was then found to be more completely removed, and the thermic property destroyed. By digesting the powder in one part of hydrochloric acid and three parts of water, for one week, less chloride was evolved during the change and during the combustion succeeding it.

53. A solid piece of amorphous antimony, weighing 774.0
grains, was placed under a bell-glass filled with water, and the change induced in it by a blow; a small quantity of chloride of antimony separated, together with a minute quantity (less than one-twentieth of a cubic inch) of gas: the water was then strongly acidulated with hydrochloric acid, and allowed to rest five days; the gas still remained unabsorbed.

54. By fusing well-washed specimens, either of the gray or of the changed dark variety, perfectly free from laminae or crevices, in a porcelain crucible, no material alteration occurred in them until at a temperature of incipient fusion; they then swelled to a large pasty mass, evolving volatile matter, and finally contracted to a clear shining globule, covered with downy crystals of oxide of antimony. Similar results were obtained by fusion under tersulphide of antimony or cyanide of potassium, except that in the latter case a minute quantity of black matter separated, which I found by combustion, alone and with nitrate and chlorate of potash, to be carbon. The metal, previously powdered and placed under cyanide of potassium, agglutinated into a pasty mass before fusion or evolution of vapour. Five specimens of gray antimony, fused under the cyanide, lost respectively 0·437, 0·618, 0·81, and 0·88 per cent. by weight. Two thin specimens of amorphous antimony, previously changed under water at 60° F., lost respectively, by fusion under cyanide of potassium, 5·1 and 5·2 per cent. A moderately thin specimen, previously changed in air at 60° F., lost 3·3 per cent.; a thick one, similarly changed, lost 2·1 per cent.; a second ditto lost 2·52 per cent.; and three other specimens of changed antimony lost respectively 3·484, 3·92, and 5·56 per cent. Two specimens of antimony one-tenth of an inch thick, changed in the air at 60° F., and then fused under chloride of potassium, lost each 7·0 per cent. by the fusion, while two other portions of the same lost by fusion under cyanide of potassium 1·86 and 2·1 per cent. respectively. Five pounds of the changed dark metal, fused in an ordinary crucible, evolved much volatile matter at complete fusion. The antimony, fused under cyanide of potassium, and cooled slowly without disturbance, had strong crystalline markings upon its upper surface, while that fused under chloride of potassium had not,—probably because in the latter case the salt solidified before the metal and confined its surface.

55. The combined loss of weight by the change, and by fusion under cyanide of potassium, of three specimens, was as follows:—first specimen, a thin one, changed in air at 65°0 F., lost 3·31; by fusion, 2·49; total loss, 5·8 per cent.: second specimen, a thin one, by change in air, 3·19; by fusion, 2·71; total loss, 5·9 per cent.: and third specimen, deposited at the upper part
of the liquid, by change in air, 1·94; by fusion, 3·098; total loss, 5·038 per cent.

56. Small pieces, either of gray or changed antimony, immersed in fused nitrate of potash, rose to the surface by the aid of the evolved gas when approaching fusion, and burned with great evolution of heat and formation of antimoniate of potash. No particular effect occurred by rubbing together unchanged antimony powder and pulverized chlorate of potash.

57. Contrast of the two varieties:

<table>
<thead>
<tr>
<th>Unchanged dark variety.</th>
<th>Gray variety.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Specific gravity:</td>
<td>6·369 to 6·673.</td>
</tr>
<tr>
<td>5·739 to 5·944.</td>
<td></td>
</tr>
<tr>
<td>Colour:</td>
<td>Color:</td>
</tr>
<tr>
<td>Polished steel.</td>
<td>Silver-gray.</td>
</tr>
<tr>
<td>Fracture:</td>
<td>Fracture:</td>
</tr>
<tr>
<td>Amorphous.</td>
<td>Crystalline.</td>
</tr>
<tr>
<td>Texture:</td>
<td>Texture:</td>
</tr>
<tr>
<td>Soft and weak.</td>
<td>Hard and strong.</td>
</tr>
<tr>
<td>Relative electric condition:</td>
<td>Relative electric condition:</td>
</tr>
<tr>
<td>Positive.</td>
<td>Negative.</td>
</tr>
<tr>
<td>Relative thermo-electric state:</td>
<td>Relative thermo-electric state:</td>
</tr>
<tr>
<td>Positive.</td>
<td>Negative.</td>
</tr>
</tbody>
</table>

58. It would be interesting to inquire, to what force or mode of physical action is the evolution of heat in antimony due? If it is caused by some new mode of physical action, then the force causing it will of course differ, in some respects, from all other forces, and will be found to manifest itself in a number of substances and not in antimony alone, like heat, electricity, magnetism, &c.; and will also be found, like each of them, to be correlative with, and convertible into, other natural forces.

Birmingham, November 19, 1857.

[Since the publication of this paper, the author, in a second communication to the Royal Society (dated June 1, 1858), has given the following additional information respecting this singular substance.]

The change observed in it is shown not to be an exercise of the force of cohesion, because the amount of heat evolved by the powdered metal is not sensibly different from that set free by the substance in its coherent massive state.

The thermic discharge is not limited to a particular temperature, but commences between 170° and 190° Fahr., and increases in rapidity to some point above 212° Fahr., when it becomes sudden.

The heat may be discharged either suddenly or gradually, accord-
Electro-deposited Antimony. 

The specific heat of the unchanged metal was found to be \( \varepsilon = 0.06312 \); and of the same specimens, after being gradually discharged, the specific heat was not sensibly different. But the specific heat of the substance, after sudden discharge, was found to be \( \varepsilon = 0.0543 \).

The total amount of heat evolved by the substance during its change was sufficient to raise the temperature of its own weight of ordinary antimony (sp. heat = 0.0508) about 650° Fahr.

The evolution of vapour which generally occurs during the change is a result of the molecular heat acting upon the terchloride of antimony contained in the substance. It occurs when a sufficient temperature is produced either by internal or external causes, and does not occur when the molecular discharge is gradual and the temperature is not sufficiently raised; in such cases the weight of the substance remains unaltered.

The substance, as usually produced from ordinary muriate of antimony, or from a mixture of that substance and tartar-emetic, contains small quantities of nearly all the ingredients and impurities of the depositing liquid.

The pure substance deposited upon sheets of platinum, in a solution of pure hydrochloric acid three-fourths saturated with pure oxide of antimony, with an anode of pure antimony, exhibited no material difference in properties from the less pure variety.

Two analyses of the pure unchanged substance gave the following per-centages:

<table>
<thead>
<tr>
<th></th>
<th>No. 1.</th>
<th></th>
<th>No. 2.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sb</td>
<td>93.36</td>
<td>( \varepsilon )</td>
<td>6.44</td>
</tr>
<tr>
<td>SbCl₃</td>
<td>5.98</td>
<td>SbCl₃</td>
<td>6.03</td>
</tr>
<tr>
<td>HCl</td>
<td>0.46</td>
<td>HCl</td>
<td>0.21</td>
</tr>
<tr>
<td></td>
<td>99.80</td>
<td></td>
<td>99.75</td>
</tr>
</tbody>
</table>

A trace of water contained in them was not estimated.

Solvents removed the chloride of antimony from the powdered substance much more readily after the thermal discharge than before it.

Differences of physical appearance were detected in the changed and unchanged substance in the state of powder under a microscope; the surfaces of the latter were smooth and brilliant, whilst those of the former were granular and less bright. No mechanical mixture could be detected in the changed powder.

From the various experiments detailed in the paper, it appears that the substance in question is a feeble chemical compound of antimony and acid hydrochlorate of terchloride of antimony, apparently in variable proportions, decomposable by heat, and that the change observed in it, in cases of gradual discharge, consists of a molecular alteration, attended by weakened chemical affinity, and by evolution of heat; but in cases of sudden discharge the evolved heat produces a partial chemical decomposition, which is of greater or less extent, according to the temperature acquired.
A portion of the powdered unchanged substance, digested sixty-three days with an aqueous solution of caustic potash, lost 2·95 per cent. in weight, but still retained about $\frac{3}{4}$ths of its heating power. A second portion, digested fifty-six days with strong hydrochloric acid, lost 6·66 per cent. and all its heating power.

Exposure to light did not destroy the heating power of the powdered substance.

By depositing the gray variety of antimony into mercury, a pasty compound of the two metals was formed. The amorphous variety did not combine with mercury under similar circumstances.

An acid solution of fluoride of antimony yielded by electro-deposition gray crystalline antimony not possessing the heating power.

LIII. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 395.]

April 15, 1858.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On Tangential Coordinates." By the Rev. James Booth, LL.D., F.R.S.

Extract of a Letter to Admiral FitzRoy, F.R.S., from Captain Pulken of H.M.S. 'Cyclops,' dated Aden, March 16, 1858.

My first sounding for temperature at any depth, was in 32° 13' N., long. 19° 5' W., where at 400 fathoms the minimum temperature was 51°·5, the surface at the time being 70°. The water brought up in the bottle was of greater density than we have since found it, namely, 1031 at temp. of 70°, whilst at the surface it was 1026. Supposing that you will see all the registered depths, &c. sent to Captain Washington, I do not enter into full detail here. The next time, I sent two thermometers down at 500 and 800 fathoms; at the greater depth, 44°·5; at the lesser, 50° was the minimum temperature. But now I began to observe some alterations in the indexes of the instruments, that of the maximum column not returning to the surface in the same position in which it stood on starting, viz. close to the mercury (brought to the surface temperature by being kept sufficiently long in the water alongside, and then compared with the deck-thermometer in constant use for that observation). Now I know from former experience that these indexes will shift by shaking the instrument, and with much less force than is frequently communicated to it by a shake of the line, on its passing up and down. From this we may infer that the minimum index also moves, how much it is impossible to say. And on looking over the results obtained during the voyage, I find that but few of the maximum indexes have come in standing at the point they started with. I therefore draw your attention to the fact, that such remedy may be applied as will obviate the defect. I have found another fault in thermometers before now (but then it was at a
low temperature, and not Six's self-registering instrument that was employed). During my second winter at Fort Simpson, I, every three hours throughout twelve of the twenty-four, registered twenty-one thermometers, eighteen of Adie's and three of Negretti's, on glass scales. I never found Adie's at any temperature to differ more than a degree and a half from each other. Negretti's, when they ranged low, say twenty below zero, I have found twenty, eighteen, and twelve degrees lower than Adie's. Here, in a high temperature (eighty degrees), I find three differing from the deck-thermometer, as well as from the other three (having only six remaining), being six, seven, and ten degrees higher. The correction (but a few tenths of a degree) can be allowed for certainly, but this difference may not be the same at a lower temperature, therefore it occasions a serious drawback to the efficiency of these instruments; and I always feel doubtful about the results. As yet I have only used those that agree nearest with each other.

My next sounding was in 10° 7' N., long. 27° 32' W., the position of the Hannah Shoal, no bottom with 2000 fathoms of line. There is 15 marked in some charts on this shoal. In 4° 10' N. and 28° 42' W., two thermometers were sent down to 1500 and 1000 fathoms, the greater depth showing a minimum 39°.4, the lesser 42°.5. No specimen of bottom had yet been brought up, as all the soundings hitherto, except those for the Devil's Rock in the Bay of Biscay, as well as the determinations of temperature, had been taken from a boat with small lines; so in the next cast I sounded from the ship with a large line,—the regular deep-sea line—and combined the experiments. Two thermometers were sent down on the line; and the sinker was down, by intervals, at about 1080 fathoms. The valve brought in a plentiful supply of bottom, and the thermometers showed a minimum temperature of 38°.5 at the lowest depth, and 46°.2 at 680 fathoms. This was in latitude 2° 20' N., longitude 28° 44' W., 90 miles from Saint Paul's Island. The specimen of bottom was a fine light greyish sand.

Drawing now to the Equator, I determined, if possible, to get a cast directly on it, and also the temperatures; accordingly the boat took the cast for bottom, while from the ship an endeavour was again made to combine the experiments. It failed, however, I am sorry to say, resulting in the loss of a large portion of the line, and two instruments sent down with it. From this I felt fully convinced that the uncertainty of concluding when the weight is down, from the intervals, is such, that the sounding becomes of little value, as far as the true depth is concerned.

[An extract of Capt. Pullen's Journal is here given,—showing the uncertainty of judging by the "intervals" as to the time of reaching the bottom, and the consequent liability to pay out too much line; also the increased strain then occasioned by the friction of the under current on the over-long line, and the great risk of the latter giving way on being pulled in; and stating the conclusion of the writer, that, in order to arrive at true results, soundings for bottom and temperature must not be combined.]
I have before this noticed how irregular the intervals always were when getting temperatures, particularly when more than one thermometer was on the line, but had never thoroughly considered the cause, nor the results likely to follow from the increased weight on the line; for although the addition of the thermometer (weighing about six pounds in the water-bottle) gives rapidity to the descent of the weight, its bulk offers great resistance on coming in, consequently the line has more tendency to break. And when it is intended to send down more than one thermometer, the line must be stopped to attach the addition, which at once checks the rapidity of descent, and the line has then to be let off the reel with more force, impossible to apply equally; and the intervals become so irregular, that all certainty of when the sinker is at the bottom is lost, and you feel at a loss when to stop.

From this experience I think I may say we have profited, for not a single fathom of line has been lost since, although going double the depth; once, too, with a fresh breeze with 2380 fathoms of line out; and it was with the greatest difficulty we could get the deck engine to reel it in again, and then only by putting all the Watch on to assist with their weight. I now began to observe more regularly for the temperatures, and with a stouter line than that usual for sounding, kept exclusively for the purpose. After crossing the Equator, I sent the thermometers down at nearly every tenth parallel, three at a time, at twelve, eight, and four hundred fathoms, reserving portions of the water brought up to send home for analysis.

In latitude 26° 40' S. and longitude 23° 52' W., nearly midway between where Sir James Ross has sounded without getting bottom, I got 2700 fathoms. A single thermometer was sent down to this 2700 fathoms depth, secured just above the sinker, and came in showing a minimum temperature of 35° F.; and the bottom brought up in the valve was a very fine brown-coloured sand.

In this case the common deep-sea line was used, and weighted with 120 lbs. sinker (Brook's detaching), just one-fifth of its breaking strain; and the rapidity of descent has hardly if ever been equalled in speed by smaller lines when weighted nearly up to their breaking strain, as shown in the American soundings. One hour from the time we let go, the intervals showed that the weight was down.

I ran the casting down between the parallels of 35° and 38° S., from the Cape of Good Hope eastward outside Mauritius in the Indian Ocean, in the route of many doubtful dangers, and on nearing them the lead was brought into play. The first was the 'Broomwick,' on which is marked 85 fathoms, deep enough certainly for any ship that swims; but to clear up all doubt, two casts were obtained not far from its position, of 1410 and 1102 fathoms, without reaching bottom. Then comes the 'Atalanta,' having three positions, one on our Admiralty Charts, and two from Horsburgh, giving it as an extensive shoal under water, with pointed rocks on its western part. I passed from the westward between the northern position, and the first in order to the southward, and got four deep casts, besides
Captain Pullen on Deep-sea Soundings. 461

several of 50 and 80 fathoms. The first was 1110 fathoms down by intervals, but no valve coming in, it appearing to have been broken off from the rod by striking some hard substance, either at the bottom or on its passage up or down, I immediately determined on getting another cast, although darkness was coming on. The weather fortunately was calm with very little sea on; so stationing a number of lanterns, the lead was at once dropped over the bows, and as satisfactory a cast was got as could possibly be wished, the sinker striking bottom by intervals at 1120 fathoms, thus proving the correctness of the first sounding, and finally, the valve coming in with a sufficient portion of bottom to prove it again. This specimen consisted of what appeared to the eye very fine sand covering a hard substance (coral I suspect), but under the microscope it was found to be some of the most beautiful specimens of Diatomaceae that can be imagined. I send home these specimens, small as the quantity is, being quite sufficient for examination. The next morning further to N.E. with 800 fathoms, no bottom: another cast still further N.E., bottom with 900 fathoms, bringing up another specimen of the same sort of sand as last night, with a small pebble amongst it.

[Capt. Pullen here explains how his attention was drawn to parts of the sea where the surface was covered, for a considerable space, with a white milky substance, apparently of animal nature, in large patches, with strips of deep blue water between; producing an effect which, viewed from a distance with a glass, had very much the appearance of breakers; and he suggests that some of the reports of shoals and breakers between the parallels of 35° and 40° may have no better foundation than the phenomenon in question.]

Steering now to pass to the cast of Mauritius, a little south of the parallel of 20°, about 90 miles from the land I got no bottom with 1375 fathoms of line, which gave me the first idea, that what I had before thought of the Indian Ocean not being so deep as the Atlantic was wrong. Proceeding northward, I passed west of Cargados Garazos, Saya de Malha, east of Seychelles, and crossed the Equator in 58° 20' E.; getting a cast 9 miles south of it with 2380 fathoms, no bottom. This is the sounding alluded to in a former part of this letter.

Forty or fifty miles west of the northern part of Cargados, 1400 fathoms of line reached the bottom. In 14° 41' S., 58° 43' E., no bottom with 1570 fathoms; 10° 30' S., and 58° 52' E., no bottom with 1320 fathoms. At this cast I sent down three thermometers at the 1320, 880, and 440; the minimum at the greatest depth 51° 5, at the centre depth 41° 5, and at the least depth 51° 5. The maximum tell-tales at the two least depths came in all right, but that of the greatest depth had fallen 6°; and its minimum showing an increasing temperature after passing through the colder stratum, is quite proof enough of its tell-tale falling down too, at least down the column instead of remaining up at what it once must have been in passing through that stratum, which the tell-tale of the thermometer at 880 fathoms shows the temperature of.

Winds now light and northerly. I got close to the doubtful
George Island; and three quarters of a mile west of its southern point, bottom was not reached with 2000 fathoms of line. I then passed over it nearly a mile within its southern point; and having no signs of being on shore, I conclude that no such island ever existed in the position given it on our charts; and I find no account of it in Iforsburgh.

Steaming now for Rose Galley Rocks, five miles south of the most western of them, I got bottom with 2254 fathoms of line, and brought up a plentiful supply of bottom, as well as the minimum temperature 35°. A thermometer was sent down with the weight yesterday at 2000 fathoms, and returned with a minimum temperature of 38°5. Now 35° was the minimum temperature at 2700 fathoms in the Atlantic, further south than this cast, which was near Rose Galley Rocks. I am therefore inclined to think that this is about the minimum temperature of the great depths of the Ocean, and that it commences soon after passing 2000 fathoms.

"On the Stereomonoscope, a new Instrument by which an apparently Single Picture produces the Stereoscopic Illusion." By A. Claudet, Esq., F.R.S.

In a former paper "On the Phenomenon of Relief of the Image formed on the ground glass of the Camera Obscura," after having investigated the cause of that extraordinary fact and tried to explain it, I found that the images produced separately by the various points of the whole aperture of an object-glass are visible only when the refracted rays are falling on the ground glass in a line nearly coinciding with the optic axes; so that when both eyes are equally distant from the centre of the ground glass, each eye perceives only the image refracted in an oblique direction on that surface from the opposite side of the object-glass. Consequently each side of an object-glass, in proportion to its aperture, giving a different perspective of a solid placed before it, the result is an illusion of relief as conspicuous as when looking naturally at the objects themselves.

From the consideration of these singular facts, unnoticed before, I was led to think that it would be possible to construct a new Stereoscope, in which looking with both eyes at once on a ground glass at the point of coalescence of the two images of a stereoscopic slide, each refracted by a separate lens, we could see it on that surface in the same relief which is produced by the common stereoscope.

This instrument, as may be perceived at once, is nothing more than an ordinary camera obscura supplied with two lenses, each mounted on a sliding frame in order to be able to give them, according to the focal distance, the horizontal separation necessary for producing on the ground glass the coalescence of the images of the two sides of a slide placed before the camera.

The slide itself being cut in two parts, the two images can also, moving in a groove, be separated in a horizontal direction, until they are sufficiently apart to be refracted on the ground glass by the two lenses in the most oblique direction consistent with the pro-
duction of a well-defined image; for it is to the increased degree of obliquity of the refracted rays in falling on the ground glass that is due the more effective extinction or evanescence of the image for the eye whose axis consequently deviates in a greater degree from the line of refraction.

By the same principles which produce the phenomenon of relief of the image formed on the ground glass of the camera obscura, the right picture of the slide, being obliquely refracted on the ground glass by the right lens in a line coinciding with the axis of the left eye, is visible only to that eye; and the left picture, being refracted obliquely by the left lens in an opposite direction coinciding with the right eye, is only visible to that eye. Consequently each eye seeing only one image, and that image having its own perspective, the optic axes have to converge more or less according to the angular separation of the similar points of the two coincident images; and by the different degrees of convergence producing single vision of these various similar points, we have the sensation of the comparative distances of the objects represented on the ground glass.

Before having constructed this new stereoscope and tried its effect, it would have been hasty on my part to pretend that its success was certain, and for this reason I took care in my former paper to propose it as a mere speculative idea suggested by the phenomenon I had discovered, without vouching for the result. Indeed it was not long before I had to congratulate myself on my caution, when I found that, the truth of my experiments being questioned and the deductions from these experiments denied, my proposed stereoscope was declared impossible, as being founded on principles completely at variance with the laws of optics.

However, these remarks did not shake my conviction, and after the usual difficulties, I have now the satisfaction of being able to prove that I was perfectly right, and that I had not been led astray by any erroneous notion in my analytic and synthetic deductions. I have constructed the instrument which I propose to call the Stereomonoscope, as it exhibits in perfect relief a picture which appears single on the ground glass of the new instrument, and as single as the image of the camera obscura has always been supposed to be.

The instrument, in its present rough state, is undoubtedly very imperfect and susceptible of many improvements which time and experience will suggest. I present it as the result of a first attempt, hoping that it will be found curious as illustrating a new and interesting scientific fact and producing an effect quite unexpected in optics.

April 22.—Major-General Sabine, Treasurer and V.P., in the Chair.

The following communication was read:—

"On the Stratification of Vesicular Ice by Pressure." By Prof. William Thomson, F.R.S. In a Letter to Professor Stokes, Sec. R.S.

In my last letter to you I pointed out that my brother's theory of
the effect of pressure in lowering the freezing-point of water, affords a perfect explanation of various remarkable phenomena involving the internal melting of ice, described by Professor Tyndall in the Number of the ‘Proceedings’ which has just been published. I wish now to show that the stratification of vesicular ice by pressure observed on a large scale in glaciers, and the lamination of clear ice described by Dr. Tyndall as produced in hand specimens by a Brahman’s press, are also demonstrable as conclusions from the same theory.

Conceive a continuous mass of ice, with vesicles containing either air or water distributed through it; and let this mass be pressed together by opposing forces on two opposite sides of it. The vesicles will gradually become arranged in strata perpendicular to the lines of pressure, because of the melting of ice in the localities of greatest pressure and the regulation of the water in the localities of least pressure, in the neighbourhood of groups of these cavities. For, any two vesicles nearly in the direction of the condensation will afford to the ice between them a relief from pressure, and will occasion an aggravated pressure in the ice round each of them in the places farthest out from the line joining their centres; while the pressure in the ice on the far sides of the two vesicles will be somewhat diminished from what it would be were their cavities filled up with the solid, although not nearly as much diminished as it is in the ice between the two. Hence, as demonstrated by my brother’s theory and my own experiment, the melting temperature of the ice round each vesicle will be highest on its side nearest to the other vesicle, and lowest in the localities on the whole farthest from the line joining the centres. Therefore, ice will melt from these last-mentioned localities, and, if each vesicle have water in it, the partition between the two will thicken by freezing on each side of it. Any two vesicles, on the other hand, which are nearly in a line perpendicular to the direction of pressure will agree in leaving an aggravated pressure to be borne by the solid between them, and will each direct away some of the pressure from the portions of the solid next itself on the two sides farthest from the plane through the centres, perpendicular to the line of pressure. This will give rise to an increase of pressure on the whole in the solid all round the two cavities, and nearly in the plane perpendicular to the pressure, although nowhere else so much as in the part between them. Hence these two vesicles will gradually extend towards one another by the melting of the intervening ice, and each will become flattened in towards the plane through the centres perpendicular to the direction of pressure, by the freezing of water on the parts of the bounding surface farthest from this plane. It may be similarly shown that two vesicles in a line oblique to that of condensation will give rise to such variations of pressure in the solid in their neighbourhood, as to make them, by melting and freezing, to extend, each obliquely towards the other and from the parts of its boundary most remote from a plane midway between them, perpendicular to the direction of pressure.

The general tendency clearly is for the vesicles to become flattened
and arranged in layers, in planes perpendicular to the direction of
the pressure from without.

It is clear that the same general tendency must be experienced
even when there are bubbles of air in the vesicles, although no doubt
the resultant effect would be to some extent influenced by the run-
ing down of water to the lowest part of each cavity.

I believe it will be found that these principles afford a satisfactory
physical explanation of the origin of that beautiful veined structure
which Professor Forbes has shown to be an essential organic prop-
erty of glaciers. Thus the first effect of pressure not equal in all
directions, on a mass of snow, ought to be, according to the theory,
to convert it into a stratified mass of layers of alternately clear and
vesicular ice, perpendicular to the direction of maximum pressure.
In his remarks "On the Conversion of the Nevé into Ice*," Pro-
fessor Forbes says, "that the conversion into ice is simultaneous"
(and in a particular case referred to "identical") "with the forma-
tion of the blue bands; . . . and that these bands are formed where
the pressure is most intense, and where the differential motion of the
parts is a maximum, that is, near the walls of a glacier." He farther
states, that, after long doubt, he feels satisfied that the conversion
of snow into ice is due to the effects of pressure on the loose and porous
structure of the former; and he formally abandons the notion that
the blue veins are due to the freezing of infiltrated water, or to any
other cause than the kneading action of pressure. All the observa-
tions he describes seem to be in most complete accordance with the
theory indicated above. Thus, in the thirteenth letter, he says,
"the blue veins are formed where the pressure is most intense and
the differential motion of the parts a maximum."

Now the theory not only requires pressure, but requires difference
of pressure in different directions to explain the stratification of
the vesicles. Difference of pressure in different directions produces the
"differential motion" referred to by Professor Forbes. Further,
the difference of pressure in different directions must be continued
until a very considerable amount of this differential motion, or dis-
tortion, has taken place, to produce any sensible degree of stratifica-
tion in the vesicles. The absolute amount of distortion experienced
by any portion of the viscous mass is therefore an index of the per-
sistence of the differential pressure, by the continued action of which
the blue veins are induced. Hence also we see why blue veins are not
formed in any mass, ever so deep, of snow resting in a hollow or corner.

As to the direction in which the blue veins appear to lie, they
must, according to the theory, be something intermediate between
the surfaces perpendicular to the greatest pressure, and the surfaces
of sliding; since they will commence being formed exactly perpen-
dicular to the direction of greatest pressure, and will, by the differen-
tial motion accompanying their formation, become gradually laid
out more and more nearly parallel to the sides of the channel through
which the glacier is forced. This circumstance, along with the com-
paratively weak mechanical condition of the white strata (vesicular


layers between the blue strata), must, I think, make these white strata become ultimately, in reality, the surfaces of "sliding" or of "tearing," or of chief differential motion, as according to Professor Forbes's observations they seem to be. His first statement on the subject, made as early as 1842, that "the blue veins seem to be perpendicular to the lines of maximum pressure," is, however, more in accordance with their mechanical origin, according to the theory I now suggest, than the supposition that they are caused by the tearing action which is found to take place along them when formed. It appears to me, therefore, that Dr. Tyndall's conclusion, that the vesicular stratification is produced by pressure in surfaces perpendicular to the directions of maximum pressure, is correct as regards the mechanical origin of the veined structure; while there seems every reason, both from observation and from mechanical theory, to accept the view given by Professor Forbes of their function in glacial motion.

The mechanical theory I have indicated as the explanation of the veined structure of glacial ice is especially applicable to account for the stratification of the vesicles observed in ice originally clear, and subjected to differential pressure, by Dr. Tyndall; the formation of the vesicles themselves being, as remarked in my last letter*, anticipated by my brother's theory, published in the 'Proceedings' for May 1857.

I believe the theory I have given above contains the true explanation of one remarkable fact observed by Dr. Tyndall in connexion with the beautiful set of phenomena which he discovered to be produced by radiant heat, concentrated on an internal portion of a mass of clear ice by a lens; the fact, namely, that the planes in which the vesicles extend are generally parallel to the sides when the mass of ice operated on is a flat slab; for the solid will yield to the "negative" internal pressure due to the contractility of the melting ice, most easily in the direction perpendicular to the sides. The so-called negative pressure is therefore least, or which is the same thing, the positive pressure is greatest in this direction. Hence the vesicles of melted ice, or of vapour caused by the contraction of melted ice, must, as I have shown, tend to place themselves parallel to the sides of the slab.

The divisions of the vesicular layers into leaves like six-petaled flowers is a phenomenon which does not seem to me as yet so easily explained; but I cannot see that any of the phenomena described by Dr. Tyndall can be considered as having been proved to be due to ice having mechanical properties of a uniaxial crystal.

June 17.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Problem of Three Bodies." By Charles James Hargrave, LL.D., F.R.S.

The author states that the principal object of this memoir is to set forth two new methods of treating the dynamical equations by processes of variation of elements, differing from the ordinary pro-

* See Proceedings for February 25, 1858.
cesses of this nature principally in this particular, that the variations are represented in explicit terms of the elements themselves and of the time, and not through the medium of partial differential coefficients. It has been his object to render the processes as elementary as possible; and to preserve them in a rigorous form, by postponing all attempts at approximation until the formulæ are actually applied to practical problems. The applications given in the paper comprise the circular and spherical pendulums, and the planetary and lunar theories, and a special theorem as to the movement of the plane of a planet’s motion under the influence of several other planets.

The original normal problem which is taken as the basis, is that of motion about a fixed centre of force, where the force is directly as the distance; or, in other words, the system of equations not exceeding three in number, of the form

\[ x'' + n^2 x = 0; \]

whose solutions are represented under the form

\[
\begin{align*}
x &= \lambda_a \cos(nt + \rho) + \mu_a \sin(nt + \rho), \\
y &= \lambda_b \cos(nt + \rho) + \mu_b \sin(nt + \rho), \\
z &= \lambda_c \cos(nt + \rho) + \mu_c \sin(nt + \rho);
\end{align*}
\]

where

\[
\begin{align*}
\lambda_a &= \cos \phi \cos \psi - \sin \phi \sin \psi \cos \iota, \\
\lambda_b &= \cos \phi \sin \psi + \sin \phi \cos \psi \cos \iota, \\
\lambda_c &= \sin \phi \sin \iota; \\
\mu_a &= -\sin \phi \cos \psi - \cos \phi \sin \psi \cos \iota, \\
\mu_b &= -\sin \phi \sin \psi + \cos \phi \cos \psi \cos \iota, \\
\mu_c &= \cos \phi \sin \iota;
\end{align*}
\]

to which are afterwards added,

\[
\begin{align*}
v_a &= \sin \psi \sin \iota, \\
v_b &= -\cos \psi \sin \iota, \\
v_c &= \cos \iota.
\end{align*}
\]

These are the equations of an ellipse whose centre is at the force, and situated in a plane inclined at the angle \(\iota\) to the plane of \(xy\), and the longitude of whose node is \(\psi\); and \(\phi\) is the angular distance of the major axis of the ellipse from the node; \(a\) and \(b\) are the semi-axes of the ellipse; and \(\rho\) is the angular distance, from the major axis, of the zero-point of the motion, measured on the circle described on the major axis. A uniform motion around the circle represents the place of the body by the corresponding point on the ellipse, where it is cut by a perpendicular dropped on the major axis.

If the force be not situated at the origin, but at the point \((X, Y, Z)\), we have merely to substitute \(x - X\) for \(x\), &c. in the above equations of motion and solutions.

It is then shown that a system of the form

\[ x'' + a^2 x = P_{ax} \]

is the solution of the differential equation

\[ x'' + a^2 x = \frac{P_{ax}}{2} \]
where \( n^2 \) and \( P_x, P_y, \) and \( P_z \) are any variables, may be solved by the same set of final integrals, and the same values of \( x', y', \) and \( z' \), by supposing the elements \( a, b, \phi, \psi, \iota, \) and \( \rho \) to become variable. These elements are those of an ellipse tangential to the actual curve of motion; and the following formulae are obtained for their variation:

Let

\[
\begin{align*}
\lambda_a P_x + \lambda_b P_y + \lambda_c P_z &= (P_x), \\
\mu_a P_x + \mu_b P_y + \mu_c P_z &= (P_y), \\
v_a P_x + v_b P_y + v_c P_z &= (P_z);
\end{align*}
\]

and let \((\text{putting } T \text{ for } nt + \rho)\),
\[
\begin{align*}
\alpha \cos \phi \cos T - b \sin \phi \sin T &= \xi, \\
\alpha \sin \phi \cos T + b \cos \phi \sin T &= \eta;
\end{align*}
\]
then

\[
\begin{align*}
n,a b \delta \phi &= \xi(P_z), \\
n, a b \sin \iota \delta \psi &= \eta(P_z), \\
\frac{1}{2} \delta(n^2(a^2 + b^2)) &= -n(a \sin T(P_x) - b \cos T(P_y) + r^2 n n'), \\
(a^2 - b^2)(\delta \phi + \cos \iota \delta \psi) &= -\frac{1}{n}(b \cos T(P_x) - a \sin T(P_y)) + 2 a b \sin T \cos T - \frac{n'}{n}, \\
(a^2 - b^2) \delta(nt + \rho) &= -\frac{1}{n}(a \cos T(P_x) - b \sin T(P_y)) - (a^2 + b^2) \sin T \cos T - \frac{n'}{n}.
\end{align*}
\]

It may be observed that \( \xi \) and \( \eta \) are coordinates of the body referred to the plane of the tangential ellipse, and to an axis of \( \xi \) coinciding with the node.

This method is denominated the method of Tangential Variation; and it is applied directly to the problem of the circular pendulum, that of the spherical pendulum, and that of the motion of a particle where the force is a function of the distance, and in particular that of elliptical motion, where the law of force is that of the inverse square.

In a subsequent part of the paper it is shown that a system of the form

\[
x'' + n^2(x - X) = 0, &c.,
\]

where \( n^2, X, Y, \) and \( Z \) are any variables, may be solved by the same set of final integrals, and the same values of \( x', y', \) and \( z' \), as those which have been already given as the solutions of the same system when \( n, X, Y, \) and \( Z \) are constant, by supposing the elements to become variable. In such a case, the elements are those of an ellipse osculating with the actual curve of motion, always of course having its centre at the moveable point \((X Y Z)\). The following formulæ are obtained for the variation of these elements:

Let

\[
\begin{align*}
\lambda_a Y' + \lambda_b Y' + \lambda_c Z' &= (X'), \\
\mu_a Y' + \mu_b Y' + \mu_c Z' &= (Y'), \\
v_a Y' + v_b Y' + v_c Z' &= (Z');
\end{align*}
\]
then
\[ n\alpha b \delta \zeta = (Z') \frac{d\zeta}{dt}, \]
\[ n\alpha b \sin \nu \delta \psi = (Z') \frac{d\eta}{dt}, \]
\[ \delta (n\alpha b) = -n((X') b \cos T + (Y') a \sin T), \]
\[ \frac{1}{2} \delta (n^2(a^2 + b^2)) = -n^2((X') a \cos T + (Y') b \sin T) + r^2 \sin \nu', \]
\[ (a^2 - b^2) \delta (\nu + \rho) = ((X') b \sin T + (Y') a \cos T) + 2a b \sin T \cos T \frac{n'}{n}, \]
\[ (a^2 - b^2) \delta (nt + \rho) = ((X') a \sin T + (Y') b \cos T - (a^2 + b^2) \sin T \cos T \frac{n'}{n}; \]
in which \( \frac{d\zeta}{dt} \) and \( \frac{d\eta}{dt} \) are the differential coefficients of the expressions
for \( \zeta \) and \( \eta \), taken explicitly with regard to \( t \).

This method is denominated the method of Osculating Variation.

Applying the method of tangential variation to the system
\[ x'' + \frac{\mu}{\rho^3} x = 0, \&c., \]
we perceive that this system admits of complete solution in finite terms, leading in fact to the usual theory of elliptical motion. Taking this system, therefore, as a normal system, the author proceeds to deduce the formulæ for the variation of the elements of this system, in order to arrive at the solution of the system
\[ x'' + \frac{\mu}{\rho^3} x = P_x, \&c. \]

The elements which have been selected, for reasons fully explained in the paper, are \( \epsilon \) and \( \psi \), whose meanings are already known; \( A \) and \( N_T \) denoting respectively the mean distance, and the longitude of the epoch measured in the plane of the tangential ellipse as it exists at the time \( t \), and measured from the node at that time; and \( e \) and \( \varpi \) denoting respectively the eccentricity of the tangential ellipse, and the longitude of its perihelion measured as above; and it is observed that these are strictly normal elements, according to Professor Donkin’s definition of normal elements.

The variations of these elements are then rigorously found, and are expressed as follows:—Denote
\[ \cos \psi P_x + \sin \psi P_y \] by the symbol \( P_\xi \),
and
\[ \cos \epsilon (\cos \psi P_x - \sin \psi P_y) + P_x \sin \epsilon \] by the symbol \( P_\eta \);
and let
\[ -P_\xi \sin \theta + P_\eta \cos \theta = P_{\xi, \theta}; \]
\[ -P_\xi \sin \varpi + P_\eta \cos \varpi = P_{\xi, \varpi}; \]
\[ P_\xi \cos \theta + P_\eta \sin \theta = P_{\eta, \theta}; \]
\[ P_\xi \cos \varpi + P_\eta \sin \varpi = P_{\eta, \varpi}; \]
then
\[ \delta \epsilon = \frac{r \cos \theta}{\sqrt{A^2(1 - e^2)^2}} (P_\xi), \]
\[ \sin \epsilon \delta \psi = \frac{r \sin \theta}{\sqrt{A^2(1 - e^2)^2}} (P_\xi), \]
\[ \delta e = \frac{r}{\text{NA}^2(1-e^2)^2} \left\{ (\cos \theta + \omega + e \cos (\theta - \omega)) P_{\xi, \theta} + (1 + e \cos (\theta - \omega)) P_{\xi, \omega} \right\}, \]

\[ e(\delta w + \cos \; \delta \psi) = \frac{r}{\text{NA}^2(1-e^2)^2} \left\{ \sin \theta \cos \theta - (1 + e \cos (\theta - \omega)) P_{\eta, \omega} \right\}, \]

\[ \frac{\delta A}{\text{NA}^2(1-e^2)^2} (P_{\xi, \theta} + e P_{\xi, \omega}) = \frac{2}{\text{NA}^2(1-e^2)^2} \{ -\sin \theta P_{\xi, \theta} + (1 + e \cos (\theta - \omega)) P_{\eta, \omega} - 2 e P_{\eta, \theta} \}, \]

which are capable of being expanded in terms of the elements, and \( t \) by means of the ordinary expressions for \( r \), \( \theta \), and \( \theta - \omega \) in terms of the same quantities. The values of the elements at the time \( t \) being supposed to be found, by the integration of these formulae, in terms of \( t \), and their initial values, are to be substituted in the ordinary expressions for the coordinates, so as to obtain their values at the time \( t \).

The author exhibits the application of the preceding formulae to certain simple examples, and then proceeds to apply them to the planetary theory. For two planets (distinguished by the suffixes 2 and 3) supposed to move in the same plane, the following are the rigorous expressions for the variations. Let \( \alpha_3 \) and \( \alpha_2 \) be the ratio of the mass of each planet to that of the central body. Let \( P \) denote the cube of \( r_3 + r_2 \), and let \((P-1) \sin(\theta_3 - \theta_2)\) be called \( Q \), and \((P-1) \cos(\theta_3 - \theta_2) - \frac{r_2}{r_3} P \) be called \( R \); then

\[ \delta e_2 = \alpha_3 \frac{N_2 A_3 r_2}{r_3 (1-e^2)^4} \left\{ (\cos \theta_2 - \omega_2)(2 + e_2 \cos(\theta_2 - \omega_2)) + e_2 \right\} Q + \sin(\theta_2 - \omega_2)(1 + e_2 \cos(\theta_2 - \omega_2))R \],

\[ e_2 \delta w_2 = \frac{N_2 A_3 r_2}{r_3 (1-e^2)^4} \left\{ \sin(\theta_2 - \omega_2)(2 + e_2 \cos(\theta_2 - \omega_2))Q - \cos(\theta_2 - \omega_2)(1 + e_2 \cos(\theta_2 - \omega_2)) R \right\}, \]

\[ \delta A_2 = 2 \alpha_3 \frac{N_2 A_3^2 r_2}{r_3^2 (1-e^2)^4} \left\{ (1 + e_2 \cos(\theta_2 - \omega_2)) Q + e_2 \sin(\theta_2 - \omega_2) \right\} R \],

\[ \delta (N(t+r)) = -(1-e^2)^4 \delta \omega - 2 \alpha_3 \frac{N_2 A_3 r_2}{r_3^2} R. \]

From these formulae, the secular variations of the elements are obtained without difficulty; and a new method of integrating the equations for the variations of the eccentricity and longitude of perihelion is given.

The author then enters upon a minute examination of the mathematical character of secular variations, and their bearing upon the methods of approximation to which the problem of three bodies has given rise. It is pointed out that the disturbance finally effected through the medium of a secular variation is not of the order of the
disturbing force, or rather of the ratio of the disturbing force to the
central force; but that it may remain precisely the same, though this
ratio should be diminished or increased without limit. The differ-
eence affects not the aggregate amount of deviation or disturbance
carried, but the time in which this aggregate amount is produced.
If we consider the undisturbed problem of two planets about a sun
as representing motion in two planes inclined to each other at the
angle $I$, and in ellipses having eccentricities $e_2$ and $e_3$, it is shown
that, no matter how small or how large may be the disturbing force
produced on each orbit by the other planet, the aggregate amount of
disturbance of the planet $m_2$ is of the order of the quantities $I$ and $e_2$,
and that of the planet $m_3$, of the order of $I$ and $e_3$. From considera-
tions of this nature, which are dwelt upon at length in the memoir,
the author concludes that the ordinary direct methods of solution
by approximation, being based upon the erroneous assumption that
the variations of the coordinates are of the order of the disturbing
force, are not, in a mathematical sense, legitimate processes; and
that, in the planetary theory, they produce results practically true
only on account of the minuteness of the disturbing forces, and the
consequent great length of the secular periods; and that, in the
lunar theory, their failure is made evident, in consequence of the
comparatively large magnitude of the disturbing force, and the con-
sequent rapidity with which the elements of the moon’s orbit pass
through their secular periods.

The formulæ for the variations of the elements are then applied
to the lunar theory; and some of the integrations are effected
by means of a lemma containing the solution of the differential
equation

$$\frac{d\phi}{dt} = F \cos (pt - q\phi)$$

(where $F$, $p$ and $q$ are numerical coefficients), in the form

$$\cos (pt - q\phi) = \frac{qF + p \cos Mt}{p + qF \cos Mt}, \quad M \text{ being } (1 - \frac{q^2F^2}{p^2})p.$$

By this method, the total motion of the moon’s perigee, as well as
the coefficients of the evocation, are fully obtained in the first in-
stance, without the necessity of any second approximation; and the
usual difficulty as to the movement of the perigee does not present
itself. The motion of the node, and the evocation in latitude, are
correctly obtained in a similar manner.

This part of the memoir is concluded by an extension of the
general formulæ for the tangential variation of elements to the case
in which we suppose the constant $\mu$ to become variable, the result
being to add to each variation a term involving $\epsilon\mu$.

The third part of the Paper contains the development of the
method of osculating variation, before briefly described; from which
are deduced the formulæ for the osculating variations of elliptic ele-
ments. This method is capable of being applied to the planetary
and lunar theories, as well as that of tangential variation; but the
advantages of this method did not appear to be such as to justify the actual expansion of the formule for these theories. The author, however, shows that with reference to any system of three bodies, the equations of motion for each body naturally assume the form

\[ a'' + a'(x - X) = 0, \&c. \]

(being the system solved by this method); and that the X, Y, and Z are absolutely the same for each of the three bodies. This is shown by demonstrating, that at any given moment the three lines which represent the direction of the force acting on each of the three bodies all pass through the same point, which is denominated the centre of force. The coordinates of this common centre of force are,

\[ X = \frac{(23)x + (31)x_3 + (12)x_3}{(23) + (31) + 12}; \]

with similar expressions for Y, and Z; (12) being \( r_{12} = \frac{m_2}{m_1} \chi_{r_{12}} \), \( \chi \) denoting the law of force, &c. Each body has its own value of \( n^2 \); their ratios being denoted by the proportion

\[ n_1^2 : n_2^2 : n_3^2 = \frac{r_{23}}{\chi_{r_{23}}} : \frac{r_{31}}{\chi_{r_{31}}} : \frac{r_{12}}{\chi_{r_{12}}}. \]

The invariable plane of this system of three bodies is then found; and it is shown that the nodes of the three orbits upon this plane are always in a certain relative position, constituting a kind of triangle of equilibrium about the centre of force; resulting, in the limiting case where one of the three bodies is infinitely larger than the other two (or in what is denominated the undisturbed Problem of Three Bodies), in an exact opposition of the two nodes of the orbits of the latter two bodies upon the invariable plane of the system.

The formule for the osculating variation of elements are then applied to a system of three bodies, of which one possesses a predominating magnitude, so far as is necessary to determine the movement of the planes of the orbits; and it is readily shown that, if we consider only the first order of the disturbing force, the inclination of the plane of each orbit to the invariable plane is absolutely constant; and that the two nodes are always in opposition to each other, and move with a uniform angular velocity round the invariable plane.

This theorem is then extended to a system of \( n \) bodies moving about a central predominant body; and it is shown that the aggregate effect of the disturbing forces of such a system upon the plane of any one of the bodies can always be represented by stating that its node upon a certain fixed plane revolves with a uniform angular velocity, the plane of the orbit always remaining at the same inclination to the fixed plane. The rate of this angular movement, and the coordinates of the fixed plane upon which the movement takes place, are found by means of formule of remarkable simplicity. These three quantities may be ascertained once for all for each planet (viz. the inclination of the fixed plane on which the
node moves to any coordinate plane, the longitude of the node of the fixed plane in relation to any coordinate line, and the angular rate of movement of the node of the orbit upon this fixed plane), and, when once ascertained, may be regarded as fixed elements of the planet, from which the position of the plane of its orbit can always be determined without the use of tables.

GEOLOGICAL SOCIETY.
[Continued from p. 398.]

November 3, 1858.—Prof. J. Phillips, President, in the Chair.

The following communications were read:

1. "On some Natural Pits on the Heaths of Dorsetshire." By the Rev. O. Fisher, M.A., F.G.S.

On Affpuddle Heath and Piddletown Heath, near Dorchester, the surface is pitted with numerous circular or oval hollows, like inverted cones. They usually vary from about 60 to 80 yards in circumference; but one measures 130 yards, and another, called "Culpepper's Dish," is 290 yards round: in the former the sloping sides are 23 yards high; in the latter 47 yards. After observing that these pits could not have been formed by the washing away of the underlying sand-beds, the author proceeded to show that their formation seemed to be due to the subsidence of the material into "sand-pipes" in the subjacent chalk, owing to the percolation of rain-water containing carbonic acid, which dissolved the chalk; and Mr. Fisher referred to the explanation of this process given by Mr. Prestwich in a paper formerly read before the Society.

As this process could have gone on only during a subaërial condition of the surface, and must have occupied a very long time, the author remarks, that the larger pits on the Heaths referred to must have been formed by the sinking of the Eocene beds into enormous "sand-pipes" during an extended geological period, and that the area they occupy was dry land during all that time, and has been so ever since, and must therefore have formed islands or headlands in the sea, which last filled the adjacent valleys and gave them their present configuration.

As the formation of these pits was subsequent to the outspread of the superficial gravel of these Heaths, and previous to the last depression and elevation of the land, their date would be perhaps near that of the great mammalian fauna. The author also explained his views of the method by which the subsidence of the materials gave rise to the peculiar shape of the pits; and he observed that somewhat similar conical depressions have been noticed in process of formation at the present day.

2. "Notice of the occurrence of an Earthquake along the Northern Edge of the Granite of the Dartmoor District, on September 28, 1858." By G. Wareing Ormerod, Esq., F.G.S.

The shock was slight, and appears to have been confined to a very narrow district, that may be estimated as not exceeding 8 miles
in width, and running along the northerly edge of Dartmoor, along the line of junction of the granite and the altered carbonaceous rocks. The length of the area affected by the earthquake is about 21 miles, from Crediton on the east to the Fox and Hounds Inn, on the Tavistock Road, about 7 miles from Okehampton. The shock seems to have taken a direction from east to west, to have occurred about 8 o'clock in the evening, and to have lasted, where most severe, about 15 seconds.


The following localities are quoted by the author as affording more or less distinct evidence of granitic veins traversing the carbonaceous rocks. Near Meldon, 2 miles S.W. of Okehampton; Cocktree Moor; in the Gorge of the Teign after leaving Hunts Tor; the most northerly point of Whyddon Park, and the hill-side nearly opposite; on the hill-side above the Logan Stone; near Westerly Tor; on Sharp Tor; on the road from Cranbrook Castle to Fingle Bridge; and the road down the hill to the west of Cranbrook Farm.


The author first described several specimens of the peculiar banded flints found in the chalk and in gravel, and of which he had made a large collection during several years. They usually exhibit a central longitudinal axis or narrow stem, crossed on its middle third by numerous short parallel stripes of alternately light and dark flint, and frequently terminated at each extremity by an irregular mass of flint, often clouded or grey. The axis occurs sometimes isolated, sometimes covered with a thin coating of grey flint only, and sometimes associated with only a few cross stripes of the banded structure. In some instances the banded flint has for its axis a sponge, or fragments of sponge.

The author had not found in the banded flint any spongy tissue peculiar to it; in some instances, however, a silicified sponge appears to have been traversed by alternate lines of the light and dark colour analogous to those of the banded flints. In some instances a concentric arrangement of light and dark layers of flint occurs around the two ends of an axis, or around isolated nuclei. Mr. Wetherell regarded this banded appearance in the flint as not being due to an organic structure, but to have originated in a peculiar arrangement of the siliceous matter around organic bodies, frequently long and stem-like, such as those of the Graphularia which supplied so many axial nuclei to the concretions in the London Clay.

November 17.—L. Horner, Esq., V.P., in the Chair.

The following communications were read:—

1. "On some Fossils from South Africa." By C. W. Stow, Esq.

At the close of 1850 Mr. Stow and his party fell back into the
The interior to avoid the Kaffirs; in making the journey he collected largely the fossils on his route, and succeeding in preserving them on his return.

In a plain at the foot of the Rhenosterberg, which is a branch of the Sneewbergen range, he met with patches of ground strewed with nodular concretions and fossil wood, probably derived from the neighbouring mountains. These mountains are composed of horizontal strata. Eight of the beds at the foot of the Rhenosterberg are sandstones, above them are four layers of calcareous grit or pebbly limestone with other sandstones. These calcareous beds sometimes contain bones; but at one spot in the sandstone-rock, Mr. Stow discovered and chiselled out a nearly perfect skeleton of a small reptile. Other reptilian bones, and especially two small well-preserved skulls, rewarded his search; one of these belonged to a small Dicynodon, the other to a little undescribed reptile.

Mr. Stow sent numerous specimens of the nodular concretions and septaria from the rocks of this place, and also specimens of the concretional and other trappean rocks of two dykes that crossed the plain.

2. "On some points in the Geology of South Africa." By Dr. R. N. Rubidge. Communicated by the President.

The author had observed in Namaqualand the occurrence of horizontal siliceous beds, covering other siliceous inclined beds, the silification of the latter being apparently due to the infiltration of silica from the upper quartzose beds into the inclined beds below. In this communication Dr. Rubidge details the evidences that he observed of such a process having taken place, and points out how the observations on some of the Namaqualand rocks by Mr. Bain, Mr. Bell, and Dr. Atherstone, respectively, tend to support his views on this point. The inclined beds of this district are gneissic, and, in the instances referred to, very quartzose.

The horizontal sandstones of this district he correlates with the Table-mountain sandstones, but in them he has found only a few obscure traces of fucoidal or other plants.

The author then passes on to the Cape district; and, first offering his testimony to the industry and general exactitude of Mr. Bain as a geologist, he proceeds to compare Mr. Bain's section of Mitchell's Pass with the section he lately made for himself on two hasty journeys.

Mr. Bain describes the indurated sandstone or quartzite of Mitchell's Pass as being at first horizontal, and then suddenly dipping at a strong angle northward, so as to underlie the Devonian schists of the Bokkeveld, at Ceres, and to divide them from the slates of the Cape district. Dr. Rubidge points out the apparent difficulty of explaining such an inclination of the quartzite, the slates underlying both the inclined and horizontal positions not presenting any evidence of a difference in dip beneath the two portions of quartzite; and he suggests that the inclined beds of quartzite have nothing to do with the horizontal quartzite, except that, being immediately
beneath the horizontal siliceous beds, they have by some means been silicified from above, and thus rendered in appearance only identical. Taking this view, he considers that the inclined quartzite is a conformable successional portion of the schists and slates, the horizontal sandstones being of younger age than any of the schistose beds, and extending over them from Table Mountain to Orange River on the west, and to George on the east. On the north the schists are known to be of Devonian age by the fossils of the Bokkeveld, and the recent discovery of a few trilobites and spiriferae at some spots in the slates of the southern districts of the Cape (near Cape St. Francis, at Klein Winterhoek, and near Jeffery's Bay), are considered by the author as corroborative of his view, that the slates of the Cape are not divisible from the schists of the Bokkeveld, but are to be linked to them by the intercalated quartzites described in this portion of his paper, the schistose rocks of Ceres, Cape Town, and Malmesbury (Silurian and Carboniferous? of Bain) having generally a similar strike and dip.

In the eastern province of the Cape Colony, Dr. Rubidge thinks that a similar condition of silification exists in the Zuurberg range, although no overlying horizontal sandstones were there seen. He describes in detail a section made by himself and Mr. R. Pincher, along the road from Port Elizabeth to Somerset, which shows the inclined schistose beds intercalated with a band of dark felspathic rock (the claystone-porphry of Bain) lying conformably on and passing into the quartzite of the Zuurberg on the south, and, after some great flexures of the quartzites, a similar series of conformable schistose rocks (and a felspathic band) dips from the other side of the Zuurberg in an opposite direction. Similar beds continue with a diminishing dip as far as Van der Merwe's River, whence they rise again to the north to beyond Bushman's River (at Gower's), a little beyond which the felspathic band appears intercalated with them. The section then becomes obscured until the Karoo beds are met with near Brak River, having a slight southerly dip, and probably abutting unconformably against the schists near Callaghan's Inn. This section differs in some important features from that published by Mr. Bain of the same district.

Dr. Rubidge considers that the slaty beds flanking Zuurberg on either side, and forming the synclinal trough at the Van der Merwe's River, are of the same age as the quartzites of the Zuurberg, which are in his opinion silicified by metamorphic influences: the interbedded felspathic rock may also in his opinion be possibly of metamorphic origin.

The author follows up his argument by reference to other parallel sections, and regards the plant-beds of Écca, as well as those of the Great Fish River and the Van der Merwe's River, as being of Devonian age, and not belonging to the lower Karoo beds, regarded by Mr. Bain as having a more southerly extension. Dr. Rubidge notices that some members of the two formations resemble each other so strongly, that sometimes it is difficult to distinguish between them.

The plant-beds above referred to contain innumerable obscure
On some Mineral Springs at Teheran, Persia.

vegetable fragments, like those of stems, reeds, &c.; and in the rocks at Gower's on Bushman's River Dr. Rubidge has seen, beside fragmentary vegetable remains, some fine jointed stems.

The plant-beds of the Karoo beds, at Bloemkop, contain two or more kinds of *Glossopteris*, very similar to those of the plant-beds of Central India and Bengal.

Amongst the fossils sent by Dr. Rubidge, are several fossils from the Zwartzkop and from the mouth of Sunday River; amongst the latter, some *Belemnites* and *Hamites*, probably of Cretaceous age.

A large series of specimens from the Zuurberg and Van der Merwe's River accompanied this communication; and Dr. Rubidge also sent a series of fossil plants from the Dicynodon or Karoo beds of Bloemkop, with which Mr. C. J. Powell, of Graaf Reinett, had supplied him.

3. "On some Mineral Springs at Teheran, Persia." By the Hon. C. A. Murray, in a letter to Sir Charles Lyell, F.G.S.

In August the author made an excursion into the wild and rocky valleys of Laridjan, on the northern side of the Elburz chain, to examine some mineral springs near the village of Aske. This village is placed on a steep declivity above the impetuous torrent of the Laur, and is about 40 or 45 miles E.N.E. from Teheran, and near where latitude 36° N. intersects longitude 52° E. It is only a few miles from the lofty and slumbering volcano Demavend. Round Aske the country is chiefly limestone, with dark-coloured pudding-stone, and in several places large tracts of sandy grit, in many precipitous heights of which numerous caves and hermitages have been excavated in olden times.

The most celebrated spring in the neighbourhood is the Ab-i-garm (hot-water); a warm sulphur-spring that rises on one of the spurs of the Demavend, about six miles to the eastward of Aske, on the left bank of the Laur, and probably about 2000 feet above the bed of that river.

The principal mineral ingredients of this spring are sulphur and naphtha, with some iron and lime. On the 15th of August, the temperature of the atmosphere in the shade, at two p.m., being 75° F., the temperature of the spring at its source was 150° F. From hence it flows down the side of the mountain to a large basin about 15 feet long, 10 broad, and 4 deep, over and around which a large stone bath has been raised. The temperature in the basin is about 118° F. Here in summer thousands congregate from every part of northern Persia.

The bath seems to be beneficial in rheumatism, neuralgia, and some diseases of the skin.

About forty years there issued from the rock, a few feet from the Ab-i-garm, a cold spring of pure water, which disappeared after an earthquake.

The tepid baths of Aske are about half a mile from the village on the right bank of the Laur, and about 250 or 300 feet above that river. These are used both for bathing and drinking. Lime and
carbonic acid gas are abundant in this water. There are several springs of different dimensions, the bathing basins of which are at distances varying from ten to fifteen yards from each other. At half-past five P.M., the temperature of the air in the shade being 71°F., the water in the centre of the largest basin (where the bubbles are thrown up by the gas to an elevation of 6 or 8 inches above the surrounding surface) was at 82°F.—the average of the temperature of the other springs near by.

On the left bank of the Laur a small bubbling spring of water, similar to those last mentioned, had a temperature of 85°F. These tepid mineral springs lying to the eastward of Aske, do not appear to be affected by the intervention of the deep and rocky bed of the Laur. To the westward of Aske, at the distance of about 1½ mile, on the left bank of the river, and not more than 150 feet above its bed, is a fine cold chalybeate spring: besides iron, this probably contains carbonic acid gas and a small portion of sulphur. Its temperature was found to be 50°F., that of the atmosphere being 73°F. It is not disagreeable to the taste, and in some complaints is found very strengthening and efficacious.

In conclusion, the author referred to the undeveloped riches of the Elburz—its coal, iron, copper, silver, sulphur, marble, and other mineral treasures.

LIV. Intelligence and Miscellaneous Articles.

OBSERVATIONS ON MR. COUPER'S NEW CHEMICAL THEORY.

BY M. A. KEKULÉ.

At the meeting of the 14th of June, Mr. Couper presented to the Academy of Sciences a "Note on a New Chemical Theory," in which I find several ideas so nearly identical with others published by me a short time since, that I think it my duty to submit some observations on the subject. In fact, in two memoirs which have appeared in Liebig's Annalen, one "On Copulated Compounds, and the Theory of the Polyatomic Radicals" (November 1857), and the other "On the Constitution and Metamorphoses of Chemical Compounds, and the Chemical Nature of Carbon" (dated March 16, and published May 19, 1858), I have put forward different views, which, in my opinion, should furnish a clearer insight into the constitution of chemical compounds.

Mr. Couper commences his memoir with these words:—"I ascend to the elements themselves, of which I study the mutual affinities. This study is sufficient, in my opinion, for the explanation of all chemical combinations."

Now in my second memoir, at page 136, there is the following phrase:—"I think it necessary for the explanation of the properties of chemical compounds, to ascend to the elements themselves of which these are composed."

Further on, in speaking of carbon, Mr. Couper says, "The highest known power of combination for carbon is that of the second

* See also p. 104 of the present volume of this Journal.
degree, that is to say, 4;" and he finds that carbon has a peculiar physiognomy, "inasmuch as it enters into combination with itself, a feature so characteristic that," according to him, "it accounts for the important and still unexplained fact of the accumulation of the molecules of carbon in organic compounds. In the compounds in which 2, 3, 4, 5, 6, &c. molecules of carbon are linked together, it is carbon that serves to bind the carbon."

We cannot agree with him that these properties are indicated by him for the first time. In my first memoir (p. 133, note) I have said expressly that carbon was quadriatomic in its nature, that is to say, that 1 atom of carbon (C=12) is equivalent to 4 atoms of hydrogen (H=1); and I have added that consequently the simplest compounds of carbon with elements of the first group (monatomic elements) were CH₄, C Cl₄, &c. In my second memoir I have also given a further development to this idea (p. 153), and from it I have drawn the corollary (p. 154), that, in substances which contain several atoms of carbon, this accumulation can only be explained by the hypothesis that the atoms of carbon itself are linked together, thus neutralizing a part of their general affinity. I thought that this hypothesis might be founded upon various examples too circumstantial to be referred to here; I shall content myself with pointing out that I also have given a general formula which expresses, for a certain class of compounds, the number of atoms of hydrogen combined with n atoms of carbon, in the following terms—

\[ C^n H^{n(1-2)} , \]

whilst Mr. Couper, on his side, expresses it in this way—

\[ C^n M^{n.4} - M^{n.2} = nCM^4 - mM^2 , \]

in which \( m < n \).

At a glance, certain features of resemblance will be traced between our two formulæ; and I prove that mine is only a particular application of that of Mr. Couper, which is more general, indeed, but also more vague. I shall also remark that H in my formula, like M in that of Mr. Couper, represents all the elements belonging to the first group.

I shall not dwell upon the discovery of this grouping of the elements, which is developed in more detail in my first memoir (p. 133), and of which the germ may be found in the works of Laurent, Gerhardt, and Williamson; but I may be allowed to indicate that it lays down the principle, that, besides the unknown force which is by common consent called chemical affinity, we must also, for the explanation of chemical combinations, attribute a great part to that which I have called the basicity of atoms. If Mr. Couper thinks he has discovered the cause of this difference of basicity in the existence of a special kind of affinity, I am the first to admit that I have no right to contest his priority in this.—Comptes Rendus, Aug. 30, 1858, p. 378.

METEORIC STONE OF BORKUT.

This stone, according to Dr. Franz Leydolt, fell on the 13th of October, 1852, at 3\textsuperscript{rd} P.M., at Borkut in Hungary. It was found two
feet under ground, and still warm. The largest piece weighed 7
Vienna pounds and 64 loths, and the whole 12 pounds. The exter-
ior is partly black or dull gray, and much blebby or subcorioraceous,
it being covered with a thin crust. The interior is ash-gray, and
irregularly fine-granular. Its specific gravity is 5:242. It afforded
18°26 per cent. of magnetic material, and 81°74 non-magnetic. The
former was found to contain—Iron, 85°14; nickel (with some co-
balt), 10°06; copper and tin, 0°40; sulphur, 4°19; phosphorus,
0°18 = 99°97. Separating 11°53 as FeS³, the composition of the
iron corresponds to—iron, 87°96; nickel, 11°38; tin and copper,
0°46; phosphorus, 0°20.

The non-magnetic part was partly soluble in muriatic acid. The
soluble and insoluble parts consisted of—

SiO₃, AFO₃, FeO. CaO. MgO. KO. NaO.
1. Soluble. 36°16 3°07 21°43 1°20 36°34 0°50 1°27
2. Insoluble. 57°29 4°20 12°08 3°90 17°67 1°14 3°72

The soluble part corresponds nearly to olivine; the insoluble to
34°93 per cent. oligoclase, and 65°59 pyroxene. From these two
analyses there were excluded—from the first, before deducing the
per-centaje, 0°82 sulphuret of iron, and 13°09 nickeliferous iron;
from the second, 1°60 chromic iron.—Silliman’s American Journal
for September 1858.

ON CRYSTALLIZED VALERIANATE OF ATROPINE.

BY H. CALLMANN.

This salt forms perfectly white and light crusts; the crystals
appear to belong to the rhomboidal system, and the faces are very
brilliant. At a temperature of 68° F. the crystals soften, and at
57°6 F. they are liquefied. Under the double influence of air and
light, they soon acquire a yellow colour. The carbonic acid of
the air displaces a certain quantity of valerianic acid, which is
recognizable by its peculiar odour.

The crystallized salt presents the various reactions of the salts of
atropine and of the valerianates. It is extremely soluble in water,
less so in alcohol, and still less in ether. The analysis of the salt,
dried in vacuo at the ordinary temperature, gave:

<table>
<thead>
<tr>
<th></th>
<th>I.</th>
<th>II.</th>
<th>Calculated.</th>
</tr>
</thead>
<tbody>
<tr>
<td>C</td>
<td>66°40</td>
<td>66°20</td>
<td>66°00</td>
</tr>
<tr>
<td>H</td>
<td>8°90</td>
<td>8°81</td>
<td>8°50</td>
</tr>
<tr>
<td>O }</td>
<td>24°70</td>
<td>24°99</td>
<td>25°50</td>
</tr>
<tr>
<td>N }</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

These numbers lead to the formula

C₁₀H₉O₃, C₃4H₅₃NO₆ + 2H₂O.

Comptes Rendus, September 6, 1858, p. 417

CRYSTALLIZED FURNACE-PRODUCTS.

We are requested to state that the furnace-products which are
said by Professor Miller, in his communication to the Magazine for
October, to have been received by Dr. Percy from the United States,
were sent to the latter by Professor Brush, of Yale College.—W. F.

In a paper on the Integral of Gravitation that appeared in the Philosophical Magazine of May last, I endeavoured to draw attention to the principle of physical causation supplied to us by the mechanical theory of heat,—how it leads us to study natural phenomena in their dynamical sequence, and suggests the arranging of problems of molecular statics so that they may present the aspect of motor transition to the mathematical inquirer. It may perhaps be of advantage, as testing the general argument, to discuss the question, Can the theory of the propagation of sound developed by Newton and Laplace be viewed, consistent with the modern idea of heat, as a correct exposition of the actual mode in which pulses are transmitted through air? It appears from recent memoirs, that this opinion is still generally entertained by physicists; and the theory of Laplace has in some instances been considered so perfect as to afford the means of deducing the specific heat of air from the velocity of sound, and this by zealous professors of the mechanical theory of heat.

The ideas upon which Laplace's theory was constructed are to be found in the second chapter of the second book of the Mécanique Céleste. The following extracts show how very remote they are from those now universally prevalent:—

Page 105, line 8. "Doubtless it is necessary to admit that between the molecules of air there is a repulsive force which is only sensible at imperceptible distances: the difficulty consists in deducing from it the laws of elastic fluids. This may be done by the following considerations."

P. 105, line 4 from bottom. "I suppose then that these molecules retain the heat by their attraction, and that their mutual repulsion is due to the repulsion of the molecules of the heat which I assume to extend to an insensible sphere of activity."

P. 111, line 9. "But what ought we to understand by the temperature $u$, and what is its measure? It appears natural to

* Communicated by the Author.
take for this measure only the density of caloric produced in a space by the radiation of neighbouring bodies."

P. 111, line 19. "A supposition that it appears very natural to admit, is, that the action of the caloric of a molecule of gas on the caloric of another molecule, &c."

P. 113, line 6. "We have supposed, in that which precedes, that the caloric of a molecule was retained on it by the attraction of the molecule, which experienced no sensible action except by the repulsive force exerted on this caloric by the caloric of the surrounding molecules."

P. 117, line 16. "It is then extremely probable that the attractive force of the caloric of one molecule by another molecule is insensible in the state of gas."

Professor Thomson, who has written largely on the dynamical theory of heat (Philosophical Magazine; Edinb. Phil. Trans.; London Phil. Trans.), has the following in a memoir "On the Thermal Effects of Fluids in Motion" (Phil. Trans. 1854, p. 361):—

"In the notes (Prof. Thomson's notes) to Mr. Joule's paper on the Air-engine (Phil. Trans. 1852, p. 82), it was shown that if Mayer's hypothesis be true, we must have approximately—

Specific heat of air with constant pressure 237.4
Specific heat of air with constant volume 168.4

because observations on the velocity of sound, with Laplace's theory, demonstrate that \( k = 1.410 \) within \( \frac{1}{70} \) th of its own value. Now the experiments at present communicated to the Royal Society prove a very remarkable approximation to the truth in that hypothesis*; and we may therefore use these values as very close approximations to the specific heats of air."

The ratio \( k \) was found by MM. Gay-Lussac and Welter to be 1.37, and by MM. Clement and Desormes 1.35. The same experiments repeated by the author of the article 'Hygrometry' in the Encyc. Brit., impressed him with the conviction that the initial ratio was exactly \( \frac{4}{3} \).

Here, therefore, we have an instance of a zealous adherent and expounder of the dynamical theory of heat showing such con-

* At p. 341, under the heading "Theoretical deductions from these Experiments," Prof. Thomson arrives at the conclusion that air and carbonic acid "evolve more heat than the amount mechanically equivalent to the work of compression," thus representing the experiments as proving an exception to Mayer's hypothesis. That such cooling effects must take place in consequence of the deviation from Mariotte's law discovered by Regnault, if Mayer's hypothesis and the dynamic theory of heat hold good, seems obvious enough (see Phil. Mag. vol. xiv. p. 279). So that they are actually a delicate test of the accuracy of that hypothesis; and taken as supplementary to Mr. Joule's previous experimental inquiries, seem completely to establish it as a theory proved by induction.
dence in Laplace's views, such conviction of their truth to nature, that he considers them entitled to supersede the result of direct observation; and no attempt is made to reconcile the statical theory of caloric with the *vis viva* theory of heat; nor is a necessity for doing so apparently even felt as a preliminary step to the adopting of one of the deductions of the former as an ally to the deductions of the latter.

As Mr. Joule's name is conjoined with that of Professor Thomson in this memoir, it is perhaps necessary to assume that he also approves of this manner of deducing the ratio of specific heats.

Another instance of the commingling of statical and dynamical theories is to be found in a memoir by M. Masson, "On the Correlation of the Physical Properties of Bodies," which appeared in a recent number of the *Annales de Chimie.* M. Masson also adopts the mechanical theory of heat (see § III. chap. 2). Yet in the beginning of the same chapter he writes, "Laplace has discovered the true mathematical expression of the velocity of sound in gas." Further, M. Masson states that he has found, by experimenting according to the method employed by MM. Gay-Lussac and Welter, that the value of *k* is actually what is required to make Laplace's formula agree exactly with the best observations on the velocity of sound. No details are given, although the amount of compression in such experiments is of some importance, as the ratio increases with this amount; and the formula has only to do with the initial ratio, or when the change of density is infinitely small. M. Masson has also computed the velocity of sound in a considerable number of composite gases and vapours from the pitch of the sound given by an organ pipe while immersed in them; and thence, employing Laplace's theorem, obtains the respective values of *k*, which are thus found to range from 1.26 to 1.42 in the composite gases, and from 1.06 to 1.27 in the vapours.

Thus all authorities, both of the statical and dynamical school, seem to agree that Newton and Laplace's theory of sound is a perfect representation of nature, and that its success is as complete as the theory of gravitation.

Newton, in several parts of the *Principia*, calls upon us to keep in mind that his principles are mathematical, not philosophical. At the beginning of Book 3 he thus expresses himself:—"In the preceding books I have laid down the principles of philosophy; principles not philosophical, but mathematical, such to wit as we may build our reasonings upon in philosophical inquiries." Again, after proving in Prop. 23, Book 2, that "particles flying each other with forces that are reciprocally proportional to the distances of their centres, compose an elastic
fluid whose density is as the compression," he adds, in the scholium, "But whether elastic fluids do really consist of particles so repelling each other, is a physical question. We have here demonstrated mathematically the property of fluids consisting of particles of this kind, that hence philosophers may take occasion to discuss that question."

In Prop. 47 of Book 2, Newton shows that "if pulses are propagated through a fluid, the several particles of the fluid going and returning with the shortest reciprocal motion are always accelerated or retarded according to the law of the oscillating pendulum." It is assumed that the elastic force is proportional to the density; and in the direction of the pulse the fluid is supposed to be divided into physical linolea, which are expansible and contractile, and exhibit a force that resists compression inversely as their breadth. The mathematical reasoning defines the law by which the breadth of these linolea must vary while they go and return,—and hence the law of the difference in the breadth of two adjacent linolea, and consequently the law of the accelerative force operating on each corpuscle, which is thus found to be the same as a body moving in a cycloid is subject to under the influence of gravity.

Newton's fundamental hypothesis is, that the particles of air in the direction of the pulse are successively agitated with like motions; that both the dynamic condition and the static force of repulsion, which is determined by the length of the line that separates two adjacent particles (called a linola), is transferred onwards in the direction of the pulse from one particle to the next adjacent in regular succession.

The demonstration takes account of three orders of magnitudes:—1, the breadth of a pulse (L); 2, the breadth of an oscillation of a particle (2l); 3, the length of a linola (λ), each considered as infinitesimal with respect to the preceding.

If the motion of a particle forward and backward in the line 2l corresponds to that of a cycloidal pendulum, i.e. if the relation between the accelerative force (acting in the line of motion), the acquired velocity, and the time is the same in the line 2l as in the complete cycloid, the force in this line must vary simply as the distance (y) from its middle point. The value thus assigned to the force implies that δ, the difference between the lengths of two adjacent linolea, should vary also in this proportion. If a semicircle is described on the diameter 2l, y is the cosine of an arc, of which x being the sine, we have \( \delta = \frac{y}{x} = dx \); so that the differential of the linola ought to be equal to the differential of the sine, and hence the absolute magnitude of the deviation of the length of a linola from its mean length ought to be proportional to the sine.
Thus if the motion of a particle is oscillatory, like a complete cycloidal pendulum, the required sequence of force demands the above specific sequence of change in the distance of the particles. Again, if the motion of each particle is oscillatory, the required sequence in its velocity (viz. that it should vary as $x$ the sine) demands also a specific sequence of change in the distance of the particles; and this sequence is precisely the same as what is required by the sequence of force.

To obtain a clear idea of the proof of this (which is a problem of pure mathematics), we may suppose with the same radius $l$ another semicircle to be described, placed also in the line of the pulse, and removed to the distance $\lambda$ from the preceding semicircle. Let a third also be drawn, removed the same distance $\lambda$ from the second. We have further to suppose these semicircles divided into as many parts $(aa_1, a_1a_2, a_2a_3, \&c.; bb_1, b_1b_2, b_2b_3, \&c.; cc_1, c_1c_2, c_2c_3, \&c.)$, beginning at where the line of pulse intersects them as $\lambda$ enters into $L$ (or $\frac{L}{\lambda}$ number of parts). The length of each of these parts or steps is thus $\frac{\lambda}{L} 2l \pi = s$ (being infinitesimal with regard to $\lambda$).

Having made this construction, we have next to consider that the motion of each particle to be oscillatory must be such that, at the instant when particle $A$ has traversed the versed sine of $aa_n$, the particle $B$ (next in advance of $A$) being one step behind in its motion, has traversed only the versed sine of $bb_{n-1}$, and particle $C$ the versed sine of $cc_{n-2}$. If $B$ had traversed as many steps as $A$, the distance $\lambda$ that separates them would not alter; but since it is a step behind, $AB$ is at this point less than $\lambda$ by the difference between vers $aa_n$ and vers $bb_{n-1}$, or vers $aa_n - vers aa_{n-1}$, which equals $s \cdot \sin aa_{n-\frac{1}{2}}$. In the same way, $C$ being a step behind $B$, their distance is less than $\lambda$ by $s \cdot \sin aa_{n-1\frac{1}{2}}$. Thus we have $BC - BA = s (\sin aa_n - \sin aa_{n-1}) = s \cos aa_n \frac{\lambda}{l}$. [Here $s$, being an absolute magnitude, has to be divided by the absolute radius $l$ to represent the differential of arc.] At the beginning of the vibration $n = 1$, and $\cos aa_n = \text{radius}$; hence with $B$ at the initial point $b$, $C$ at $c_{-1}$ (a step back on the returning half of the previous oscillation), and $A$ at $a_1$ (the points on the circle being supposed projected on the diameter), the difference

$$BC - BA = \frac{s^2}{l} = 4l \left(\frac{\pi \lambda}{l}\right)^2.$$

This initial amount determines the accelerative force acting at the beginning of the motion of each particle, which is obtained by comparing it with the reciprocal of $\lambda$, which represents the
whole static force of repulsion between two particles at the distance $\lambda$. This force having to support the weight of $\frac{H}{\lambda}$ particles (H being the height of a uniform atmosphere), $\frac{1}{\lambda}$ represents the force $\frac{H}{\lambda} g$, viz. a force that in one second is capable of communicating a velocity of $\frac{H}{\lambda} g$ feet per second.

To obtain the value of the initial force acting on particle B when it is at b, we have the following proportion:

$$\frac{1}{\lambda} \cdot \frac{1}{\lambda} = \frac{1}{s^2} \cdot \frac{H}{\lambda} g \cdot \frac{s^2 H}{\lambda} g = HgA\left(\frac{\pi}{L}\right)^2$$

The time ($\tau$) taken by a particle to traverse $2l$, with this force diminishing as the distance from the centre of the semicircle, is the same as the time required for one oscillation of a pendulum whose length is $l$, if subject to an influence of gravity equal to this force, and is the same as the time taken by the pulse to travel through $\frac{1}{2} L$. By the law of the pendulum, $\tau$ is equal to $\pi$ multiplied by the square root of quotient of length of pendulum by force of gravity, hence

$$\tau = \pi \sqrt{\frac{l}{\pi^2 Hg^{-4} l}} = \frac{1}{2} L \sqrt{\frac{1}{Hg}}$$

and the velocity of the pulse per second is $\sqrt{\frac{Hg}{2}}$.

This supposes Mariotte’s law maintained; but the experiments made at the suggestion of Laplace, proved that for rapid compressions the elastic force increases in a higher ratio than the density; and for small increments of density, the correspondent increments of elastic force are very nearly $\frac{4}{3}$ those computed on the hypothesis that Mariotte’s law is maintained; that is, as if the repulsive force of a particle supported the weight of $\frac{4}{3} \lambda$ particles: hence Newton’s theorem for the velocity of sound in elastic fluids is strictly represented by $\sqrt{\frac{4}{3} Hg}$.

The numerical results from this formula, taking Regnault’s data, compared with observations made at low temperatures (so that the influence of aqueous vapour should be avoided), show a difference at most of about $\frac{1}{3}$, or as if $\frac{4}{3}$ should be augmented $\frac{1}{3}$th part. Thus no objection to Newton’s theory can well be made on the ground of its not according with observation, as no theory can be expected to embrace all the circumstances that may affect the result; e.g. the repulsive force may not emanate from the centre of the particles, so that the size of the particles may
influence the result. Also the repulsive action is necessarily assumed to be limited to adjacent particles, not extending through the interstices of these to the particles beyond (for such is the extraordinary and improbable hypothesis required to deduce Mariotte's law from a static repulsive force). This may be supposed subject to modification during vibratory action.

But the hypothesis upon which the mathematical demonstration rests is open to three grounds of objection:—1. It does not take account of the condition of the front of a pulse when the particles from a condition of rest enter into the cycle of motion defined by the theory. 2. The force of repulsion between two adjacent particles required by the theory is extravagantly large. 3. The other physical properties of gases are not deducible from the hypothesis.

To these may be added, that the dynamical theory of heat has suggested another hypothesis which is free from these objections, and which therefore claims a preference according to Newton's first "rule of reasoning in philosophy," viz. "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes."

1. The theory does not take account of the condition of the front of the pulse, or rather of the front of the first of the series of pulses of which a sound consists. This is apparent if we consider that a particle is represented by the theory as at rest at each extremity of its oscillation, and at those points the accelerative force is at its maximum, and is derived from the difference between the lengths of the lineola that issue from the particle in front and in rear. The front lineola cannot differ from the mean length so long as the front particle is at rest unaffected by the advancing pulse. The rear lineola is less than the mean length by a certain small amount \( \alpha \). If the front particle were in action in a pulse cycle, the length of the front lineola would be greater than the mean length of a lineola by the same amount \( \alpha \), so that the accelerative force at each extremity of the oscillation of a particle is represented by \( 2\alpha \); and unless it were so, the condition required to sustain the beautiful relation of velocity and propelling force would be wanting. But at the front of the first pulse the lineola does not differ from the mean length, so that the accelerative force is represented by \( \alpha \), and this is only one-half the amount required by the theory to begin the oscillation. In truth, the demonstration only applies to a pulse having similar pulses operating on both sides.

2. The force of repulsion between two adjacent particles re-
quired by the theory is extravagantly large. The recent advances in the theory of heat have, in a measure, compelled us to realize the dynamic value of natural forces. To compute the absolute value of the repulsive force acting between two adjacent molecules of air, we have to consider that it has to support the gravity of the number of molecules in the height of a uniform atmosphere \( \frac{H}{\lambda} \); it must therefore exceed the force of gravity of one molecule in this ratio. Now the force of gravity in one second can communicate a velocity of 32 feet per second, so that the force of repulsion between two adjacent molecules of air must be capable in one second of communicating a velocity of \( 32 \frac{H}{\lambda} \) feet. The absolute value of \( \lambda \), the distance between two adjacent molecules of air, we can now with great probability deduce from the phenomena of capillarity (Phil. Mag. vol. xv. p. 1). At the boiling-point of water the number of molecules of steam in a cubic inch is the same as the number of molecules of air in the same volume. At 86° the number of layers of aqueous molecules in a cubic inch is 215 millions (Phil. Mag. vol. xv. p. 11). Hence at ordinary temperatures the distance between two adjacent molecules of air must be about \( \frac{1}{17} \) th of a millionth of an inch, and the value of \( \left( 32 \frac{H}{\lambda} \right) \), the velocity communicable in a second, is 160 thousand times the velocity of light. Can we for a moment believe that such a force has any real existence, that it is other than a mathematical fiction?

3. The other physical properties are not deducible from the hypothesis of a static force of repulsion. The deductive power of Newton's theory is confessedly limited to Mariotte's law and the velocity of sound. Laplace, by the invention of calorific atmospheres, is allowed to have added to these Dalton and Gay-Lussac's theory of expansion; but it is a question whether the reciprocal action between heat-atmospheres and molecules, which he expresses by mathematical symbols, can be realized by the mind. In judging of this, we must not forget the chapter of the *Mécanique Céléste*, in which the author 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*. What is to be expected from a superstructure resting upon such a foundation as this reveals? Nevertheless, granting that

* The author of the article 'Virtual Velocities' in the Penny Cyclopædia has the following remark upon this chapter of the *Mécanique Céléste*:—

"We have never met with any one who could give us an intelligible account of the meaning of this investigation."
Mariotte's law, Dalton and Gay-Lussac's law, and the velocity of sound are represented by the statical hypothesis, we have still Dalton and Graham's law of diffusion and diffusive velocity; Gay-Lussac's law of volumes; Dulong and Petit's law of specific heat, extended to the more simple gaseous bodies by Haycraft and the French physicists; the law of latent heat partially discovered by Gay-Lussac and Welter's experiments; also the diminution of temperature in ascending the atmosphere,—all as yet undeduced from any statical theory of elastic fluids. It may be that additions to the mathematical hypotheses of Laplace will be attempted with the view of extending their capacity, as indeed there seems to be no limit to this artificial and barren system of procedure, which is as far removed from the simplicity of nature as the hideous epicycles of Ptolemy.

There is another mode by which pulses may be conceived to be transmitted, which admits of being set forth in a popular way.

Suppose we range a number of ivory balls in a straight line upon a billiard table, and strike the first of the row upon the second, the initial velocity will be carried forward from one ball to the next adjacent, and so will make its appearance in the last—supposing perfect elasticity and no resistance in rolling—undiminished as if the motion of the first ball had continued, and the impulse had been carried by it alone, and not transferred by impact through others. These balls, confined to one line, are supposed to be in motion among themselves, so that those adjacent alternately strike against each other in opposite directions; the end ones being reflected from the cushions, and then back again after striking the next adjacent ball, the vis viva in one direction being at all times equal to the vis viva in the opposite.

If we now suppose one cushion to move forward with comparatively slow velocity, each time the adjacent ball strikes it it will be reflected with a velocity greater than that with which it impinged. This increment of velocity it transfers to the next ball, and so on; and the velocity with which the impulse is transferred along the line is equal to the common velocity with which the balls move. We may suppose the line extended indefinitely, and the motion of the cushion to be alternately forwards and backwards. While the adjacent ball impinges many times during each advance and retreat—during the former carrying forward a succession of small increments of vis viva, during the latter a succession of small decrements of vis viva—a pulse is formed, the intensity and duration of which depends on the motion of the cushion, but the velocity of propagation upon the motions of the balls, upon their common velocity.
Now suppose these balls reduced excessively in their dimensions, and to be perfectly elastic as well as the cushion, and we shall have obtained an idea of how pulses may be conveyed in a manner quite different from that depending upon a-statical repulsive force between adjacent particles. Instead of such stupendous force, we have to substitute molecules simply moving with the velocity of a cannon-ball, and assume the atmosphere to maintain its elasticity by its particles striking against each other with such velocity, which, viewed cosmically, by no means exceeds what is moderate, and even highly probable if heat is molecular motion.

Such a theory of elastic fluids was started by Mr. Herapath so far back as 1821 in the pages of the Annals of Philosophy, and has more recently suggested itself to M. Krönig, as we find noticed by M. Clausius in his memoir "On the Nature of the Motion we call Heat" (Phil. Mag. vol. xiv. p. 108).

The following is an extract from Mr. Herapath's memoir, p. 278, vol. i. Annals of Philosophy, April 1821:—"...... if gases, instead of having their particles endued with repulsive forces, subject to so curious a limitation as Newton proposed, were made up of particles or atoms mutually impinging on one another, and the sides of the vessel containing them, such a constitution of aëriform bodies would not only be more simple than repulsive powers, but, as far as I could perceive, would be consistent with phænomena in other respects, and would admit of an easy application of the theory of heat by intestine motion. Such bodies, I easily saw, possessed several of the properties of gases: for instance, they would expand, and if the particles be vastly small, contract almost indefinitely; their elastic force would increase by an increase of motion or temperature, and diminish by a diminution; they would conceive heat rapidly and conduct it slowly; would generate heat by sudden compression, and destroy it by sudden rarefaction; and any two having ever so small a communication, would quickly and equally intermix."

At p. 341, in paragraph beginning "These impulses," &c., and in those which succeed, ending "have from one another," we have a very clear announceement of the mode by which a static force of pressure is counterbalanced by a dynamic force of elastic impact.

At p. 345, Prop. 8. "The same things remaining, the elasticity of a gas under a variable temperature and compression is proportional to its numeratom (number of atoms in constant volume) and the square of its temperature conjointly; or the elasticity varies as the square of the temperature directly, and the simple of the space inversely.
Mr. Herapath unfortunately assumed heat or temperature to be represented by the simple ratio of the velocity instead of the square of the velocity—being in this apparently led astray by the definition of motion generally received—and thus was baffled in his attempts to reconcile his theory with observation. If we make this change in Mr. Herapath's definition of heat or temperature, viz. that it is proportional to the \textit{vis viva} or square velocity of the moving particle, not to the momentum or simple ratio of the velocity, we can without much difficulty deduce, not only the primary laws of elastic fluids, but also the other physical properties of gases enumerated above in the third objection to Newton's hypothesis. In the Archives of the Royal Society for 1845-46, there is a paper "On the Physics of Media that consists of perfectly Elastic Molecules in a state of Motion," which contains the synthetical reasoning upon which the demonstration of these matters rests. The velocity of sound is therein deduced to be equal to the velocity acquired in falling through three-fourths of a uniform atmosphere. This theory does not take account of the size of the molecules. It assumes that no time is lost at the impact; and that if the impacts produce rotatory motion, the \textit{vis viva} thus invested bears a constant ratio to the rectilineal \textit{vis viva}, so as not to require separate consideration. It also does not take account of the probable internal motion of composite molecules; yet the results so closely accord with observation in every part of the subject, as to leave no doubt that Mr. Herapath's idea of the physical constitution of gases approximates closely to the truth. M. Krönig appears to have entered upon the subject in an independent manner, and arrived at the same result; M. Clausius, too, as we learn from his paper "On the Nature of the Motion we call Heat" (Phil. Mag. vol. xiv. p. 108).

The physics of such media is a study that must be ungenial, perhaps repulsive, to mathematicians brought up in the statical school. The fundamental hypothesis does not permit us at once to transfer the subject to the domain of pure mathematics, as Newton's hypothesis converted physical astronomy into a purely mathematical study. The \textit{mode} of action by which certain phenomena make their appearance must be realized at each step in conformity with the conservation of force: the causal relation must be ever present to the mind, or no true progress can be made. On the other hand, the mathematics required is simple, and almost every one of the applications of the theory admits of popular illustration. An attempt of this kind I have given above in reference to the conveyance of sonorous impulses, introducing a dynamic theory of sound. The strict demonstration of the velocity is given in the memoir above referred to. A few further illustrations may here be added to show in brief the capabilities
of the theory; but it is difficult to limit these to one branch, all the physical properties of elastic fluids being so interwoven with each other; and it is an admirable instance of the simplicity of nature, that the cluster of elegant quantitative relations which the physics of gases present, should flow from the constitution assigned, which indeed is the simplest that it is possible to imagine.

The velocity of sound is not affected by the height of the barometer, but it is sensibly influenced by a change of temperature. This latter is to be looked for; since the velocity of the particles of air increases with the temperature, the velocity with which they convey pulses must increase in the same proportion: but it is not so obvious that the height of the barometer or weight of the atmosphere should have no effect either to accelerate or retard.

Let \( m \) be an elastic ball traversing the vertical \( PM \) backwards and forwards from the sphere, \( M \), to the plane, \( P \), the surfaces of \( m \), \( M \), and \( P \) being perfectly elastic. The condition of permanence in the mean distance of \( M \) from \( P \) requires that the impacts of \( m \) upon \( M \) should have the effect of changing the velocity of \( M \) downwards into the same velocity upwards. Gravity affects \( M \) in the interval of time that elapses while \( m \) descends from \( M \) to \( P \) and ascends from \( P \) to \( M \); during half this time gravity is employed in destroying the upward motion of \( M \), and during the second half in producing the velocity downwards with which it encounters \( m \) on its return,—\( m \) and \( M \) thus meeting each other, and separating after impact with the same velocity, but with directions reversed.

The relation between the distance \( MP (= \lambda) \), the velocity of \( m (= v) \), the weight of \( M (= \frac{H}{\lambda} m) \), and of \( m \), is very simple, and enables us to compute the absolute value of \( v \).

The time taken by \( m \) to traverse \( MP \) is \( \frac{\lambda}{v} \) part of a second; and in this time gravity communicates to \( M \) the velocity \( \frac{\lambda}{v} g \).

From the law of elastic collision, two bodies impinging and reflected back in the direction they came with unaltered velocities, must have their velocities inversely proportioned to their masses, so that

\[
M : m : v : \frac{vm}{M} = \frac{\lambda}{H} v = \frac{\lambda g}{v} \quad \text{as above.}
\]
Thus we have \( v^2 = Hg \), or the velocity is that produced by gravity in a body falling through \( \frac{1}{2} H \). [Strictly, the square velocity of air-molecules must be six times this, because the above calculus only takes account of the action in one of the six rectangular directions of space.]

To trace the influence of the barometer or weight of the uniform atmosphere, we may suppose the weight of \( M \) doubled and \( \lambda \) reduced one-half: this leaves \( H \) and \( v \) unaltered; so that if the density of air increases as the weight of the uniform atmosphere, the velocity of sound is unaffected by the barometer. If with \( M \) constant the density represented by \( \frac{1}{\lambda} \) diminishes, \( H \) must increase in the same ratio, and thence \( v^2 = \frac{\lambda}{\lambda} \), or the volume under constant pressure as the square molecular velocity,—which conforms to Dalton and Gay-Lussac’s law, if \( v^2 \approx \) temperature from zero of gaseous tension.

If we view these relations in another elastic fluid, where the weight of the molecule is twice that of air, \( M \) being unaltered, and the number of molecules in a unit volume also the same as with air, we have \( H \) inversely as \( m \), or one-half the height of a uniform atmosphere of air, and \( v^2 \) reduced in the same proportion; also the velocity of sound reduced inversely as the square root of the molecular weight or specific gravity of the gas.

To explain the increase of temperature that arises from suddenly condensing air, we may imagine an elastic ball traversing a vertical between two horizontal plates and striking alternately against them. Those plates being also considered as perfectly elastic, the velocity of the ball will continue uniform without its motion being impaired. If we now suppose the distance between the plates to be gradually diminished by one of them assuming a velocity incomparably less than that of the ball, the ball will, each time it strikes this advancing plane, receive an increment to its velocity, and thus to its \textit{vis viva}.

Let \( v \) represent the velocity of the ball, \( \delta \) the distance between the planes, \( \frac{v}{n} \) the velocity of the plane. The number of impacts upon the advancing plate in a unit of time is \( \frac{v}{2\delta} \). The velocity after one impact has increased from \( v \) to \( v + \frac{v}{n} \), and the square velocity from \( v^2 \) to \( v^2 + \frac{2v^2}{n} \), the increment being \( \frac{2v^2}{n} \) of the square velocity; at the same time the decrement of space is \( \frac{v}{n} \) (the space moved over by the plate in a unit of time) divided by \( \frac{v}{2\delta} \); this
gives \( \frac{2\delta}{n} \); and the ratio of this to \( \delta \) is also \( \frac{2}{n} \); thus the increment of \textit{vis viva} is equal to the decrement of distance, irrespective of the velocities and distance, and is equal to the mechanical force exerted by the plate. In an elastic medium, the increment of absolute temperature is equal to one-third the decrement of volume, and the increment of temperature is the equivalent of the force expended in the act of compression. We thus gain a knowledge of the mechanical equivalent of heat, and further deduce the amount of \textit{vis viva} in a gas to be equal to the work performed by its pressure acting through three times its volume.

The diminution of temperature found when ascending the atmosphere may be illustrated by supposing a series of elastic balls, \( a, b, c, d, \ldots \), to be arranged in a vertical, and moving in the vertical so that those adjacent shall alternately strike against each other at the extremity of their up-and-down motion without any transference of \textit{vis viva}, which requires that they should encounter each other with equal velocities, e.g. \( b \) in its up-motion striking \( c \), and in its down-motion striking \( a \). Now we have to mark, that, between the upper and lower impact, \( b \) receives an accession of \textit{vis viva} from the force of gravity which is proportional to the vertical distance traversed; so that comparing the \textit{vis viva} of \( b \) with any other of the series, such as \( z \), we shall find that the higher ball \( z \) has less \textit{vis viva} than \( b \), and the difference is equal to gravity acting through \( bz \). Thus in the atmosphere we might expect the decrease of temperature to be uniform if its constitution agrees with this hypothesis.

The strict demonstration represents this to be the case, and that the gradient of temperature is 1° in 319 feet; also that the absolute height of the atmosphere is six times the height of a uniform atmosphere, the density in a stratum as the fifth power of the depth of that stratum below the summit, and the elastic force or height of barometer as the sixth power of that depth.

Here we find that the diminishing temperature, in ascending the atmosphere, is represented as the natural condition of vertical equilibrium; and the question occurs, may not the increasing temperature found in descending through the earth's crust be also its natural condition of vertical equilibrium? This may be cited as one of many instances of the suggestive power of the \textit{vis viva} theory, marking it as specially the natural introduction to the dynamic theory of heat, and as likely to promote a beneficial change in the application of mathematics to molecular physics generally. Upon this account it seems to merit the attention of the educational authorities in the higher depart-
ments of scientific instruction. The subject, however, is so remote from practical application, that there is little hope of any impression being made in such quarters for many years. In the mean time it seems the duty of those who have profited by it, to do what lies in their power to proclaim its merits and acknowledge the value of the idea first struck out by Mr. Herapath, and perhaps saved from oblivion by the Philosophical Magazine of that period.

Edinburgh, Nov. 6, 1858.

LVI. On Solar Spots, and the Method of determining their Depth. By Father Angelo Secchi, Professor of Astronomy, and Director of the Observatory of the Roman College*.

It seems that the attention of astronomers and natural philosophers has been now turned again in a special manner to the study of the chief luminary of our system,—while some time ago it appeared to be abandoned, as though they feared to be overcome by the force of such a mass of light.

From the unwearied attention of men of science much has been already discovered in the sun, and there is a hope of our yet knowing much more concerning it; it is therefore with pleasure that we see this subject followed up with general ardour, particularly after the result of the recent experiments on the solar temperature.

The most interesting question is that of the nature of the solar spots, with respect to which, although a great number of natural philosophers are agreed in admitting them to be gaps in the solar atmosphere, yet from time to time the old hypotheses respecting them are revived, which suppose them to be clouds, or something else indefinable; and it is for that reason that I deem it to be fitting not to recede from what was established by some of my recent observations, which tend to subvert for ever all those ancient hypotheses that took their origin only from the inferior quality of the instruments with which the observations were made, or from want of experience on the part of the observers themselves.

The luminous coating of the sun, to which the name photosphere has been given, is without doubt in a fluid state, as is shown by its prodigious instability; and the spots are nothing else than gaps in this coating, that allow of the sensibly dark body of the sun being seen. In another memoir of mine contained in the Acts of the Academia di Nuovi Licei, I have dwelt strongly upon this point, the fundamental idea of which is not

* From the Astronomische Nachrichten, No. 1148; communicated by W. G. Lettsom, Esq.
Prof. Secchi on Solar Spots,

mine, but Wilson's. To establish this theory, he relied upon the appearances presented by a spot on its becoming visible at the edge of the sun, or on its quitting it, because it then presented itself with the penumbra diminished, and altogether wanting on the side towards the centre of the disc. According to Wilson, the penumbra would be formed by the declivity or slope of the borders of the cavity itself. The case observed by Wilson was thought to be an exceptional one; but, on the contrary, it is in truth tolerably frequently observed, and I have very often noticed its occurrence, especially in the case of spots of a circular form, and which are not about to disappear. If this is so, it will then be in our power to ascertain the depth of these cavities, and thus to measure the thickness of the layer of the sun's photosphere. A truly marvellous thing, that the human eye, which in its natural state cannot direct itself on that luminary, should be able at length to provide itself with instruments adapted to measure the depth of that ocean of flame that invests it! The matter in itself is extremely simple. If we suppose there is on the spherical body $BP\,A$, any cavity having the form of a truncated cone, $n o o' n'$, in conformity with this theory its bottom will form the nucleus, and the walls the penumbra. When the spot is projected on the middle of the disc it will appear circular; but having moved by rotation to the margin, its shape will be altered to an oval, and it will finally arrive in a position in which the inclination or slope of the declivity of the cone will become parallel to the visual ray, so that the optical projection of the bottom will touch the upper margin of the cavity, as shown in the figure.

In this state of the matter the depth of the spot may be thus arrived at. Let us suppose that the moment chosen to observe it is that of the disappearance of the internal penumbra, and let its distance to the edge of the disc be measured, as well also as the breadth of the penumbra in the longer direction of the oval in which this spot is projected; this will readily give us the depth sought.

For if we draw the radius $Cn$ from the centre of the sun, we have the angle $BC\,n = CN\,K$, whence as a consequence $onp,$
which is the depression of the side of the cavity reckoning from
the surface of the sun, will be equal to \( ACn \), the heliocentric
distance of the spot at the point nearest the limb. Now this
angle is easily obtained from the measurement of the spot at the
margin itself; and this distance is its versed sine: \( KA = \) versed
sine \( KCn \). Hence knowing, either from the Tables or from
direct measurement, the semidiameter of the sun, \( R \), and the di-
stance of the spot from the limb, \( D \), we have

\[
R - D = CK = R \cos ACn,
\]

whence the angle sought,

\[
\cos ACn = \frac{R - D}{R}.
\]

The measurement of the penumbra of the spot in a cross
direction will give \( pn \), from which \( op \) may be calculated readily
by means of the formula

\[
op = np \tan oup = np \tan ACn.
\]

This theory supposes that the spot is circular and symmetrical
in its penumbra; and from the rare occurrence of such circum-
stances, it happens that spots are somewhat scarce which are suit-
able for its being brought into play. When, however, we meet
with isolated spots that present themselves with a regularly
formed penumbra in the central portions of the disc, and the regu-
larity of which is preserved equally both above and below when
they have approached the edge, it seems one may feel assured
the error arising from this cause will not prove very considerable.
It is consequently necessary to keep an attentive watch for those
circumstances that are most advantageous; and by making suf-
ficiently numerous observations, any accidental irregularity may
be got rid of. These favourable circumstances seemed to me to
present themselves in one of the two spots that made their ap-
pearance at the beginning of March last, one of which, when it
had approached the limb on the 8th of that month, presented
itself without a penumbra on the inner side, while on the pre-
vious days it had been observed to grow continually less. The
following elements for the calculation were obtained from obser-
vations:—Rome, mean time, 1858, 8th of March, 1\(^{h}\) 50\(^{m}\). Di-
stance of the border of the spot devoid of penumbra, from the
margin of the disc, = 33''0; breadth of the penumbra in a
direction crossing that of the preceding distance = 12''025; 
breadth of the penumbra on the outer side towards the edge
= 1''35. With these data, employing the solar parallax and the
diameter of the sun, as given in the ‘Nautical Almanac,’ for the
period of the observation, we find the inclination of the sides to
the surface to be = 10\(^{o}\), and the depth to be 0:37 of the radius

of the earth, or in other words, somewhat more than a third of that radius.

Such a depth (equal to about 1330 English statute miles) may appear too inconsiderable, since it would not form a coating of 0.16 of an inch on a globe of 39.37 inches radius English measure; nevertheless it cannot be very far from the truth. Not that I, however, maintain one can found a certain opinion upon this point on this sole measurement; for it is without doubt necessary to repeat such observations frequently, inasmuch as indisputably the depth of the photosphere is not the same in all parts; yet I am convinced that the stratum cannot be of any great altitude, because the spots never show themselves devoid of a penumbra in the inner portion; except when they have arrived extremely close to the edge.

We have found that the inclination of the declivity is but very inconsiderable; but in the spots about to disappear this element must vary to a great extent, and diminish, as is shown by the fact that, at the time the penumbra is barely to be distinguished from the other parts, its limits are very ill defined, so that under a high magnifying power it can hardly be recognized. It is a curious but certain fact, that the inequality of light between the general ground of the sun and the penumbras diminishes considerably with the power of the eye-piece, the contrast being far more marked with lower powers,—a remark which likewise holds good with respect to the belts of Jupiter and Saturn. At any rate the diminution in this contrast, when the spot is about to disappear, is an additional proof that Wilson's theory is the most probable,—that is to say, that the penumbra is due to the diminution of light arising from the different inclination of the surface from which it emanates with respect to the eye of the observer.

Many have objected to Wilson's theory, that so great a diminution could not take place from this sole cause;—but if the theory assigned by him is not satisfactory in every respect, I think that, from the recent observations of ourselves and others, no doubt can remain with regard to the fundamental point when attention is given to another consideration which I will now enter into. We have repeated several times, that the penumbras are formed of streamlets and divided into extremely fine filaments, each of which is in itself of almost equal brightness to the general photosphere, but that, when seen confusedly mixed up together with intervals and obscure lines under moderate powers, they produce the illusion of a half-tint, as in engravings on copper. This is the case; and subsequent observations have borne out what we stated some years since. But there is, in addition, yet another cause not sufficiently insisted upon or known; and it is as follows:—
Observations of the spots with powerful instruments have established beyond a doubt, that on the black of the nucleus there are occasionally spread out semilucid veils of the form of frizzly curls, and usually the heralds of some irruption that the matter itself of the photosphere is about to make into the nucleus. This phenomenon, which for brevity, and that we may agree upon our terms, I have distinguished by the name of curls or clouds, I have observed most distinctly. Very frequently, indeed almost always, but especially in a splendid whirl'd spot observed on the 7th of May, 1857, and likewise in the magnificent spot visible to the naked eye the day of the eclipse in March 1858, in which these clouds presented the aspect of a kind of semilucid promontory, and were of a decided reddish colour. And it was moreover curious to see how at a certain point the filaments were united into an aggregated mass, forming, as it were, a whirlpool, or boiling in a state of the greatest agitation*.

These details demand, indispensably for their being visible, two circumstances—a quiet state of the air, and a magnifying power of at least 300. The aperture employed by me was commonly that of the entire 9 French inches, or at least 6\(\frac{3}{4}\) inches, of the great equatorial, according to the state of the air. The drawings made under these circumstances are of a nature that proves highly instructive, and they have been published on another occasion†.

Now these clouds or semitransparent veils frequently confuse the filamentous aspect of the spots; and occupying, it would seem, the lower portion of the solar atmosphere, they conceal to a certain extent the structure of those spots, as a cloud in our atmosphere would render indistinct terrestrial objects to a person looking down on them from above. Moreover the solar atmosphere in its lower regions cannot but be excessively dense and absorbent; and consequently the depth even of only a third of the semidiameter of the earth must absorb a great portion of the rays; and therefore the cavities of the penumbras will, from that reason alone, appear more obscure.

When, then, the difference of the inclination of the radiating surface pointed out by Wilson is combined with the two causes referred to (above that is to say, the presence of numerous indistinct and reddish clouds and the occurrence of a deep bed of dense atmosphere), it will be more readily understood why the interior of the cavities forming the spots should be considerably

* These clouds were observed by the late Mr. Lawson of Bath, some fifteen years ago, with a 7-inch refractor of Dollond. The full aperture of the instrument was used.—TRANSLATOR.

† For one of these sketches, see the Astronomische Nachrichten, No. 1089.—TRANSLATOR.
less lucid. I have already shown elsewhere how considerable
the effect of the absorbing power of the solar atmosphere is upon
the heat. By having recourse to a partition of the light into
two equal portions by means of a double-refracting prism, with
which a projection of the solar disc is thrown steadily on to white
paper, I have arrived at the following results:—

1st. That the faculae when near the edge are noways more bril-
liant than the centre of the disc, but that they appear to be so
only when contrasted with the lesser degree of brilliancy that the
disc has towards its margin, in consequence of the projection of
their summits above the lower portion of the absorbent atmo-
sphere. I saw, a few days prior to the eclipse in March last,
one of these faculae of an enormous size, its length near the edge
being not less than 30°, with a breadth of 30°.

2nd. That the penumbra of a spot near the centre of the disc,
when observed with a low power, is not at all darker than the
parts close to the edge itself of the solar disc, and that its light
is about equal to half that of the lucid central portion.

From this it appears evident that the influence of the lower
strata of the transparent atmosphere of the sun must exercise an
enormous power of absorption, and produce a great diminution
of light in the interior of the cavities of the photosphere.

---

LVII. On Luminous Meteors and Temporary Stars.
By Daniel Vaughan, Esq.*

In my communications published in the Philosophical Maga-
azine for May and August 1858, I endeavoured to show that
several peculiarities of the variable stars and of the solar spots
give evident indications of the great office which a widely diffused
aether performs in maintaining the effulgence of all self-luminous
orbs. A study of the phenomena of meteorous light furnishes
somewhat more satisfactory evidence on this subject. In obe-
dience to the power of gravity, the aetherial fluid of space must
be accumulated and condensed about the sun, while it also forms
less dense and extensive atmospheres for the planets. If (as
my theory supposes, or as we might infer from the principles of
chemistry) this subtle fluid requires a certain density or a definite
amount of compression to induce the chemical action which deve-
lorges heat and light, it can exhibit incessant heating and illumi-
nating energy only about the surface of immense spheres like
the sun. On the planets it must remain inactive, except in
places where it receives an extraordinary pressure from falling
meteorites; and accordingly, when these wandering masses

* Communicated by the Author.
and Temporary Stars.

plunge into the dense luciferous æther which invests the earth, they cause it to burn and become luminous around them. The tremendous pressure which gives birth to such brilliant displays, is felt several hundred feet from the meteorite itself, especially when it sweeps along the verge of our ærial atmosphere; for in this case the violent encounter drives the particles of rarefied air through the more subtle æther, and enlarges the sphere of the illumination. It is only to such causes that meteoric bodies can be indebted for the immense globes of light by which they are enveloped; and these seem to exhibit the greatest splendour when their course is nearly parallel to the horizon.

To remove some misapprehensions respecting this theory, I deem it necessary to state that the fluid concerned in producing meteoric light cannot belong to the meteor itself, but must be regarded as part of the earth’s photosphere. From his last report on luminous meteors, it would seem that Professor Powell has understood me as maintaining that it is the æther which meteoric masses themselves collect from space that burns on entering our atmosphere, and becomes a source of their heat and light. I must say, however, that this is not the idea which I intended to convey in my writings, and that I do not believe that any appreciable quantity of æther or of inflammable gas could be confined by the feeble attractive force of such small bodies, especially during their rapid flight through our ærial ocean. I have in some instances alluded to the compression of the ætherial fluid by falling meteorites, without stating the precise manner in which it is affected; and as the time at which my work must have reached England could have permitted only a partial examination of its contents, there was too much room for a misconception of my views; and I can attach no blame to the eminent Professor who has honoured me with a kind allusion to my humble labours in the cause of science.

The possibility of meteoric phenomena occurring on a colossal scale on some celestal spheres, might be a proper subject for scientific inquiry. According to Professor William Thomson, the solar heat and light are produced by meteors continually falling to the sun; and he supposes that the precipitation of these bodies is so abundant that they raise all parts of the solar surface sixty feet every year. If we judge of the size of these cosmical masses from such as visit the earth, the immense swarms of them required to support the solar brilliancy for a thousand years must have an amount of surface about sixty thousand times as great as that of the sun; and their effects in intercepting and reflecting his light would be great in proportion. Such an immense host of meteoric stones would be almost sufficient to cause a perpetual eclipse of our central luminary, if they were all confined within
the orbit of Mercury. Were the space enclosed by Venus's orbit assigned to them for their revolutions, they could not avoid sending us during the night, several hundred times as much reflected light as we receive from our full moon. It is moreover difficult to imagine that the meteoric showers can observe an almost exact uniformity in falling at all times and in all parts of the sun's surface. For the continual precipitation of these meteorites, Professor Thomson finds a resisting medium necessary; but it would seem that the chemical changes which such a fluid may be expected to undergo when condensed at the sun's surface, would account in a more simple manner for the illumination of our system.

It appears, however, that the resistance of a space-pervading medium must occasionally lead to the occurrence of stupendous meteoric scenes among the numerous systems composing our universe. Each primary and secondary planet, by imperceptible contractions of their orbits during innumerable years, must ultimately arrive in the immediate vicinity of their respective central spheres, and must perform their last revolutions through the dense luciferous atmospheres collected around the latter. A satellite in such a condition could not end its career as one vast meteor, for it must be reduced to an unstable equilibrium by the attraction of the primary; and, by a sudden dismemberment, it would be converted into innumerable fragments. This change would evidently cause the meteoric illumination to be exhibited on a more extensive scale; but a constant decline would be inevitable as the fragmentary part, forming a flat circular ring, became less capable of rendering the ether luminous. In dark systems such occurrences would be most likely to attract the attention of an observer in very distant abodes of creation; and on comparing the necessary deductions from this theory with the recorded phenomena of temporary stars, it seems difficult to avoid the conviction that the stellar curiosities which called forth such astonishment in 1572, 1604, and other times, were the result of great meteoric exhibitions occurring in the ethereal atmospheres of dark central bodies as they were traversed by the ruins of dilapidated worlds.

From the appearance of meteors and temporary stars, we must therefore conclude that photospheres envelope, not only suns, but also planets and dark central spheres, whose attraction is not sufficient to maintain them in a constantly luminous condition. Perhaps a closer relation between the atmospheric appendages of all these great bodies may be revealed, if we consider that the lower envelope of the sun is non-luminous, and that meteoric stones cease to give light when they enter the lower regions of our atmosphere. But I must defer a further consi-
deration of this subject. I feel some degree of satisfaction in finding that the non-luminous condition of the planets has been recently urged as an objection to my views, as a disposition to give them a fair test is apparent; and I have reason to hope that it will lead to a careful examination of those facts on which I mainly rely for the confirmation of my theory on solar light.

Cincinnati, Ohio,
November 29, 1858.

LVIII. Contributions to the knowledge of the Electric Spark.
By B. W. Feddersen*.

[With a Plate.]

It is not on the whole surprising, that the investigation of electrical phænomena and their laws presents the greatest difficulty in those cases where electrical motion arises from electrical rest and the reverse. On the contrary, from analogy with the other provinces of physics, we should be inclined to pronounce the complete explanation of this state of transition to be one of the most difficult problems of electricity. Although it has not been proved that the electric spark gives in its path a direct and undistorted representation of the electrical discharge which produces it, yet the two stand in such intimate connexion, that the study of the optical phænomena in the electrical spark must be of use in gaining a knowledge of the mode of discharge. The study of the spark, it is true, also offers great difficulties. The only possible method of obtaining a direct analysis of the spark, appears to me to be the conversion, by a mechanical motion, of the intervals of time into intervals of space. The rotating mirror which I judged most suited to my purpose is based upon this principle. Although in my experiments I employed an electro-magnetic rotation apparatus to obtain an approximately uniform motion during short spaces of time, yet in one important point my arrangement differs from that of Wheatstone. I effected the discharge, namely, when the apparatus was in one particular position, by means of a slipping spring, thus furnishing every such discharge during its whole continuance with a circuit of as constant a nature as possible. After converting, by means of such an arrangement, intervals of time into intervals of space, the next problem is to devise a method for the measurement of the latter. If, now, for this purpose the mirrored image of the spark was removed beyond the distance of distinct vision, and a scale was held at the same distance from the eye, in such a manner that the image appeared to fall upon

* Translated by Dr. F. Guthrie, from Poggendorff's Annalen, vol. ciii. p. 69.
it, the measurements so obtained could only lay claim to approximative accuracy, because, on the one hand, the impression of light on the eye is only of momentary continuance, on the other hand, the manifold disturbances and irregularities of the phenomenon totally preclude all possibility of repeating the experiment*.

In order to give the electric spark as long a continuance as possible, I made use of the great resistance which moist conductors offer to the electric current. Finding that moist hemp cords were useless for my purpose on account of their property of dispersing the electricity, I introduced into the circuit several thin columns of distilled water enclosed in glass tubes, and placed them either in immediate contact with the internal coating of the Leyden jar (represented in the Tables by +), or separated from it by the spark-micrometer (in these cases marked — in the Tables). The columns of water had the following absolute and reduced lengths, if for the latter we take as unit a column of 1 millim. in thickness and 1 millim. in length†.

<table>
<thead>
<tr>
<th>Columns of water.</th>
<th>Absolute lengths.</th>
<th>Reduced lengths.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>mm.</td>
<td>mm.</td>
</tr>
<tr>
<td>a</td>
<td>67</td>
<td>120</td>
</tr>
<tr>
<td>b</td>
<td>67</td>
<td>90</td>
</tr>
<tr>
<td>b₁</td>
<td>400</td>
<td>90</td>
</tr>
<tr>
<td>c</td>
<td>100</td>
<td>120</td>
</tr>
<tr>
<td>d</td>
<td>72</td>
<td>90</td>
</tr>
</tbody>
</table>

The same letter in the Tables always denotes one and the same tube, in which longer or shorter copper wires are inserted. The distance between the two copper wires in the tube was the absolute length of the water column.

I. Observations of the Spark, &c.

On introducing resistances between 100 and 1000 of my unit of resistance, I observed two very different kinds of discharge, of which sometimes the one, sometimes the other predominated.

Continuous discharge.—The first form of discharge appeared in the rotating mirror in the following manner. The discharge commenced by a single spark; two luminous parallel bands spread out from the two ends of the spark, enclosing a perfectly dark space when the resistance was very great (from 400 to 1000 units) (Plate I. fig. 15), showing perhaps a similar motion of the

* A more detailed account of the method and apparatus employed, may be found in my Inaugural Dissertation, Kiel, 1857 (published by Schwer, Kiel).
† In order to distinguish this unit from that ordinarily employed, I call it the "Reduced length of water."
electricity to that which occurs by the so-called dark discharge*. On diminishing the resistance, the bands became gradually fainter, spreading further and further into the dark space, until, by sufficiently small columns of liquid, this entire space appeared filled with a luminous glow (figs. 16 and 17). The appearance of these bands was always introduced by a spark, and has nothing whatever in common with the glow which may often be observed upon the limb of the spark-micrometer when the flask is charged for a spark of great length. The latter could not, on account of its feebleness, be seen at all in the rotating mirror. The bands mentioned generally ended suddenly in one or more partial sparks. In those rare cases where this did not occur, they gradually diminished in intensity until they entirely disappeared.

Intermittent discharge.—The second kind of discharge consists in a series of at first equidistant partial sparks, whose distances from one another gradually become greater towards the end of the discharge (fig 19). It is true that the intervals by different discharges are not always, ceteris paribus, the same. When, however, the intervals are of a minimum magnitude, the greatest regularity occurs in the discharge of the sparks. We may therefore with probability assume that such discharge is the normal one for the resistance employed, and that this is merely interrupted, giving place periodically to the continuous discharge when imperfect isolation or other disturbing influences come into play during the discharge. The intervals of time, moreover, which separate the regular partial discharges, do not correspond to times of electrical rest; for continuous discharge takes place during these periods, as is plainly noticed when the resistance is small, for then the intermediate spaces do not appear absolutely dark, but the sparks are only maxima of light. By greater resistance, however, the continuous luminous appearance is too feeble to be recognized between two consecutive partial sparks. If, moreover, we remember that the first kind of discharge is more easily obtained by small resistances, and the second by greater ones, we can understand why the intermittent discharge gradually degenerates into a continuous one as the resistance diminishes, and why, when the metallic circuit connectors are short, the latter exists alone under all circumstances.

Neither of these species of discharge, moreover, occurs with great constancy; for, generally speaking, an irregular mixture of both is observed; that is, separate sparks introduce the continuous discharge. The latter varies very much with one and

* Faraday's Experimental Researches, par. 1541.
the same resistance, the observed bands becoming longer, or
dwindling to extinction, or sometimes bounded by a new spark,
or short series of sparks (fig. 18). The cases where a long
quite regular series of sparks was observed, occurred less often,
but still they were sufficiently numerous for observation.

These two kinds of discharge offer also a difference worthy
of notice for the direct consideration of the spark discharge;
and probably we may venture to hope for the explanation
of many remarkable changes of light and colour in the electric
spark by a closer study of the manner of discharge. The con-
tinuous discharge, which plays a very subordinate part by great
resistance, and must therefore be observed by means of a small
column of water, appears in such cases as a homogeneous spark of
moderate diameter, and is recognized by the ear as a single dull
blow*. The intermittent discharge, on the contrary, gives rise
invariably to a hissing sound, and generally, instead of a single
spark, a broad pencil † of sparks often with distinct intervals.

The separate bands of light in this pencil are clearly the
partial sparks passing one after the other, but which pass so
rapidly that they appear simultaneous to the eye ‡. The proof
of this is sufficiently obvious in viewing the image of the series
of sparks in the rotating mirror.

Invariability of the course of the sparks.—If the spark-micro-
meter is not protected from the motion of the air caused by the
rotating apparatus, the first sparks almost always appear quite
straight, but they soon begin to bend towards the side to which
the draught blows, and such bending increases as the discharge
continues. The change of form is a continuous one, derived by
the one spark from its predecessor. Even when the line of the
spark is abruptly broken at one place, the angular bend is
assumed by the whole series of sparks (fig. 20). Such a
bending may, during the passage of the sparks, suffer a small
vertical displacement, so that it appears as an oblique line through
the series of sparks (fig. 21): this is undoubtedly attributable to
a small vertical motion of the air. All this appears to me to be

* The concordant observation of Riess in his latest investigations has
reminded me that I had neglected, in my first description of the experi-
ment, to draw attention to the difference of colour in the two species of
discharge. The spark of the continuous discharge (employing the smallest
resistances) assumed a yellowish colour,—while that of the intermittent
was red, with perhaps a shade of violet.
† The term pencil (Büschel) is strictly inappropriate, because it appears
in reality as though the successive sparks all lay nearly in the same plane.
‡ The discontinuity of the lightning observed by Dove on a rotating
coloured top, depends probably on nothing else than the occurrence on a
large scale of the above-described intermittent discharge.
sufficient proof that every partial discharge, under all circumstances, endeavours to proceed on the same track which the immediately preceding one had taken.

Relation between intervals of time to those of space of two partial discharges.—It appeared to me of interest to ascertain the interval of time which elapses between two partial sparks for different distances of discharge and different resistances. The observations, which were made at different times, did not, however, present the concordance which was required, so that the time ultimately obtained cannot lay claim to be an absolute measure. Such an absolute determination would, moreover, be of small value as long as bodies are employed for poles having an irregular form, such as those of the micrometer formed by the knob with clamp-screw and matrix. If, however, the smallest of the numerical values found be always taken, we appear to approximate most to the truth, for then the greatest agreement prevails. Inasmuch as the intervals increase towards the end of the series of sparks, I naturally only estimated them at the commencement. The following Table shows in what sense the time-interval observed depends upon the distance of discharge. For the first half of the Table I measured the intervals of the sparks by estimating how many sparks appeared to cover a scale of 2½ millims, or 5 millims, in length. The observations were here so made that the discharge-spark in the circuit was brought about by approaching the knob of the spark-micrometer to the charged flask after removing it from the electrifying machine. For the second half, and in all other experiments, the distance was directly estimated, and the flask left in connexion with the conductor during the discharge. I had, however, previously satisfied myself that neither this combination nor even the addition of a second flask exerted an appreciable influence upon the distance of the partial discharges, so that (of course within certain limits) the extent of the electrical surface was without influence. Generally speaking, the latter mode of observation appeared to give more discordant results than the first one; and although the following numbers show with sufficient clearness that the interval of time diminishes as the distance of discharge increases*, still the observations with the resistance can only serve to give an idea of the irregularities which often occur, especially with freshly filled resistance-tubes.

* This is undoubtedly a remarkable fact, although previously predicted by Riess. I am aware that the eye is generally disposed to estimate the intervals of short lines as greater than those of longer ones, although they be in reality equal; I believe, however, that I have guarded against error of this kind.
Interval of time between two partial discharges as influenced by the resistance.—Concerning the observations made with different resistance-tubes, I may state that there did not exist the necessary concordance between those performed on different days. The moist tubes, shortly after being filled, gave greater and more variable values for the interval between two partial sparks, than they did after some time. The reason for this I have in no way been able to discover; for before I was able to investigate it, I was compelled to relinquish my experiments. From amongst my numerous experiments, therefore, I select only those having the constant distance of discharge of 7 millims., and (excepting those marked with an asterisk) which were performed on the same day with tubes which I had already frequently employed. The observations, however, are only intended to show in general that the intervals increase with the increasing resistances. A law for this increase cannot be derived from my numbers, although it would seem that the intervals of time increase more rapidly than the resistances. In the following observations, the experiments were so arranged that the conductor of the machine remained always in contact with the inner coating of the Leyden jar. In the experiments marked with an asterisk, it was connected with two jars.
In general these results support the hypothesis advanced by Riccò: it is only on comparing those intervals of time where a resistance is introduced either on one side or symmetrically with respect to the spark-micrometer, that we meet with something remarkable. Of the observations, for instance, marked with an asterisk, a friend to whom I showed the experiment, and who did not know the resistance employed, was of opinion that the greatest interval occurred in the last observation. If, now, we consider these two observations as conclusive, we may come to the conclusion that the phenomenon of discharge is not altered by bringing on to one of the coatings any resistance smaller than that already existing on the other one. This may undoubtedly be made to agree very well with a theoretical explanation of the intermittent discharge. I must not, however, conceal the fact, that in other cases, after the introduction of the second resistance, an increase in the interval of time was produced; nevertheless such increase was never so great as that which would have been obtained by adding the intervals of time due to the two resistances acting independently and alone.

**Duration of the total discharge.**—Before I was acquainted with the regular series of the partial discharges, I had endeavoured,

<table>
<thead>
<tr>
<th>Resistance</th>
<th>Rotations in one second</th>
<th>Distance of the partial sparks in millims.</th>
<th>Smallest interval of the partial discharges in seconds.</th>
</tr>
</thead>
<tbody>
<tr>
<td>+9\frac{1}{2}c</td>
<td>7.4</td>
<td>2 to 2\frac{1}{2}</td>
<td>0.000174</td>
</tr>
<tr>
<td>+8\frac{1}{4}</td>
<td>4.2</td>
<td>1\frac{3}{4}, 2\frac{1}{2}</td>
<td>0.000112</td>
</tr>
<tr>
<td>+6\frac{1}{2}c</td>
<td>7.0</td>
<td>1\frac{1}{4}, 1</td>
<td>0.000152</td>
</tr>
<tr>
<td>+5c</td>
<td>8.4</td>
<td>1\frac{1}{4}, 1\frac{1}{2}</td>
<td>0.000067</td>
</tr>
<tr>
<td>+4\frac{3}{4}c</td>
<td>14.0</td>
<td>1\frac{1}{2}, 1\frac{1}{2}</td>
<td>0.000056</td>
</tr>
<tr>
<td>+4c</td>
<td>13.0</td>
<td>1\frac{1}{2}, 2</td>
<td>0.000048</td>
</tr>
<tr>
<td>+3c</td>
<td>18.4</td>
<td>2, 3</td>
<td>0.000022</td>
</tr>
<tr>
<td>+3c</td>
<td>18.4</td>
<td>1\frac{1}{4}, 1\frac{3}{4}</td>
<td>0.000031</td>
</tr>
<tr>
<td>+3c</td>
<td>18.4</td>
<td>2, 3</td>
<td>0.000068</td>
</tr>
<tr>
<td>+3c</td>
<td>10.7</td>
<td>1\frac{1}{4}, 1\frac{3}{4}</td>
<td>0.000058</td>
</tr>
<tr>
<td>+3c</td>
<td>10.6</td>
<td>1\frac{1}{4}, 1\frac{1}{2}</td>
<td>0.000074</td>
</tr>
<tr>
<td>+3c</td>
<td>10.7</td>
<td>1\frac{1}{4}, 1\frac{1}{2}</td>
<td>0.000037</td>
</tr>
<tr>
<td>+3c</td>
<td>10.5</td>
<td>1</td>
<td>0.000060</td>
</tr>
<tr>
<td>+3c</td>
<td>10.6</td>
<td>1\frac{1}{4}, 1\frac{1}{4}</td>
<td>0.000074</td>
</tr>
<tr>
<td>+3c</td>
<td>10.7</td>
<td>1\frac{1}{4}, 1\frac{1}{2}</td>
<td>0.000073</td>
</tr>
<tr>
<td>+4c</td>
<td>10.5</td>
<td>1</td>
<td>0.000060</td>
</tr>
</tbody>
</table>

† Some days previously, shortly after the first filling, I had found for +6c the interval 2\frac{1}{2} millims., to 6 millims., and thence the smallest interval of time, 0.000306 second. Any phenomenon of polarization would in time have exerted an opposite influence. There was, moreover, scarcely a trace of any separation of the two constituents of the water noticeable.
by employing smaller resistances, to measure the time of a total electrical discharge. A new difficulty, however, appeared here: the residue in a Leyden jar, after its discharge through a column of water, is, *aerteris paribus*, by no means constant; for the relation of the discharged electricity to the original charge may be three times as great in one extreme of two cases to what it is in the other: I shall return to this again. As, however, a mean value between these two extreme ones is by far of most frequent occurrence, I noted only those lengths of the luminous band which I observed to lie in the majority of cases upon the scale. At the same time care was taken that the entire discharge occurred at a time when the circuit-completing spring of the rotary apparatus was slipping upon the metal. The jars employed were always connected with the conductor and the one pole of the spark-micrometer, while the water column was brought into connexion with the other pole.

In this manner, although I have made only a few experiments on different days and, with different velocities of rotation, yet these may serve to show generally, what is *a priori* evident and otherwise sufficiently supported by indirect experiments, that the duration of a total electrical discharge increases both with the resistance and with the electrical surface. Further, it seems probable that while the duration increases with the distance of discharge, it does so much more slowly; for with a fivefold distance the duration is not even doubled*. In judging of the observations, it must further be remembered that the phenomena of discharge almost always ended as suddenly as they began, and that a gradual decrease without a sharply defined limit, probably only occurs with the smallest resistances. A small diminution of the intensity of the light towards the end of the luminous band, however, was always observed.

* The fact which is shown subsequently, that the residue in the Leyden jar increases more slowly than the entire quantity of charge, is here of importance. For if the residue be considered as always being a constant fraction of the charge, and if we adopt the hypothesis of partial discharges only gradually approaching one another as the distance of discharge increases, we should be rather inclined to expect a diminution of the total duration as the distance of discharge increased.
II. Measurement of the eliminated quantities of Electricity in a total discharge.

In considering the relation of the eliminated electricity to the entire original electricity on discharging a Leyden jar through a resistance of given magnitude, I have not had an opportunity of varying the resistances sufficiently. I intended therefore to discuss this question on another occasion, after the further examination of the phenomena under examination. The close connection, however, subsisting between this question and what has been above considered, determines me to communicate the two series of observations which I have as yet had an opportunity of making. The quantity of electricity eliminated on discharging a Leyden jar appears, it is true, for small resistances of the completing circuit, and with the most various charges, to be always a definite aliquot part of the latter. This constant character is, however, completely lost on the introduction of very great resistances.

The quantity of electricity eliminated by a discharge* was determined by measuring how much the 16-millim. thick sphere of the spark-micrometer had to be approximated in order that, after a discharge, a second, third, fourth, &c. discharge might be obtained †.

* It was here necessary that the jar should not have contained, a short time previously, a charge much greater than that employed in the experiment; for otherwise the always inconstant residue, the theory of which has been given by Kohlrausch (Poggendorff, vol. xci. pp. 56 and 179), would have produced a considerable effect.

† Although the distance of discharge does not offer a satisfactory measure for the quantity of charge and discharge, yet I have hesitated to employ a reduction from the charge of a measuring jar, given in the first de-
With the smaller discharge (240 millims. reduced length of water column) I read off directly on the nonius the alteration in the distance of discharge of the two discharges. With the greatest resistance this alteration was too small. I was compelled therefore to allow several (3 to 6) discharges to occur, whereby the diminution found in the distance of discharge of the two discharges—the small numbers under B in the first Table—assumed more the nature of a mean value. From three to six mean values, observed in this way with about the same charge, were then combined to a single mean value from which the relation in question was reckoned.

For the greatest resistance, with which the charge in the jar was very gradually increased, I have given each observation separately, while for the less resistance they are collected into four groups; for in one group the charges employed differed considerably. Accordingly, in the accompanying Table, by every group from which the greatest and least values for \( \frac{B}{A} \) and the mean value are given alone, there is given also the number of the observations which the group comprises.

\[
\text{Resistance} = 4830 \text{ millims. reduced length of water.}
\]

<table>
<thead>
<tr>
<th>A. Charge measured by distance of discharge in millimetres.</th>
<th>B. Alteration of the distance of discharge, in millimetres from one discharge to the other.</th>
<th>( \frac{B}{A} )</th>
<th>A. Charge measured by distance of discharge in millimetres.</th>
<th>B. Alteration of the distance of discharge, in millimetres from one discharge to the other.</th>
<th>( \frac{B}{A} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>3.05 Mean.</td>
<td>0.011 Mean.</td>
<td>3.97 Mean.</td>
<td>0.0065 Mean.</td>
<td>0.0148 Mean.</td>
<td></td>
</tr>
<tr>
<td>0.06 3.10</td>
<td>0.054 0.041</td>
<td>3.97 0.97</td>
<td>0.056 0.056</td>
<td>0.0148 0.0148</td>
<td></td>
</tr>
<tr>
<td>0.14 0.036</td>
<td>0.036 0.042</td>
<td>4.05 0.05</td>
<td>0.056 0.056</td>
<td>0.0148 0.0148</td>
<td></td>
</tr>
<tr>
<td>0.17 0.036</td>
<td>0.036 0.042</td>
<td>4.03 0.07</td>
<td>0.054 0.054</td>
<td>0.0148 0.0148</td>
<td></td>
</tr>
<tr>
<td>3.24 0.037</td>
<td>0.037 0.043</td>
<td>0.0133 Mean.</td>
<td>0.064 0.064</td>
<td>0.0153 0.0153</td>
<td></td>
</tr>
<tr>
<td>0.29 3.30</td>
<td>0.012 0.043</td>
<td>4.34 0.36</td>
<td>0.060 0.060</td>
<td>0.0153 0.0153</td>
<td></td>
</tr>
<tr>
<td>0.35 0.050</td>
<td>0.050 0.043</td>
<td>4.38 0.36</td>
<td>0.070 0.070</td>
<td>0.0153 0.0153</td>
<td></td>
</tr>
<tr>
<td>3.42 0.044</td>
<td>0.044 0.051</td>
<td>0.0148 Mean.</td>
<td>0.072 0.072</td>
<td>0.0153 0.0153</td>
<td></td>
</tr>
<tr>
<td>0.48 3.44</td>
<td>0.048 0.051</td>
<td>0.0148 Mean.</td>
<td>0.066 0.066</td>
<td>0.0153 0.0153</td>
<td></td>
</tr>
<tr>
<td>0.48 0.050</td>
<td>0.050 0.051</td>
<td>4.52 0.52</td>
<td>0.070 0.070</td>
<td>0.0153 0.0153</td>
<td></td>
</tr>
<tr>
<td>3.65 0.050</td>
<td>0.050 0.051</td>
<td>4.68 0.67</td>
<td>0.070 0.070</td>
<td>0.0153 0.0153</td>
<td></td>
</tr>
<tr>
<td>0.67 3.69</td>
<td>0.054 0.051</td>
<td>0.0146 Mean.</td>
<td>0.077 0.077</td>
<td>0.0164 0.0164</td>
<td></td>
</tr>
<tr>
<td>0.70 0.056</td>
<td>0.056 0.051</td>
<td>0.0146 Mean.</td>
<td>0.084 0.084</td>
<td>0.0164 0.0164</td>
<td></td>
</tr>
<tr>
<td>0.76 0.060</td>
<td>0.060 0.051</td>
<td>0.0146 Mean.</td>
<td>0.084 0.084</td>
<td>0.0164 0.0164</td>
<td></td>
</tr>
</tbody>
</table>

Ascription of my observations, and founded on a few experiments, because a reduction of errors of observation can only be admitted when by such reduction no new unknown errors can be introduced into the calculation.
Table (continued).

<table>
<thead>
<tr>
<th>A. Charge measured by distance of discharge in millimetres.</th>
<th>B. Alteration of the distance of discharge, in millimetres from one discharge to the other.</th>
<th>( \text{A}^* )</th>
<th>B. Alteration of the distance of discharge, in millimetres from one discharge to the other.</th>
<th>( \text{A}^* )</th>
</tr>
</thead>
<tbody>
<tr>
<td>4.92 Mean. 0.090</td>
<td>6.70 Mean. 0.150</td>
<td>5.00 Mean. 0.040</td>
<td>5.30 Mean. 0.060</td>
<td>7.22 Mean. 0.187</td>
</tr>
<tr>
<td>5.44</td>
<td>0.80</td>
<td>74</td>
<td>5.92</td>
<td>0.84</td>
</tr>
<tr>
<td>8.96</td>
<td>0.40</td>
<td>0.0167</td>
<td>0.006</td>
<td>1.00</td>
</tr>
<tr>
<td>2.98</td>
<td>0.40</td>
<td>0.0166</td>
<td>0.006</td>
<td>1.00</td>
</tr>
<tr>
<td>5.00</td>
<td>0.40</td>
<td>0.0166</td>
<td>0.006</td>
<td>1.00</td>
</tr>
<tr>
<td>5.50</td>
<td>0.40</td>
<td>0.0166</td>
<td>0.006</td>
<td>1.00</td>
</tr>
<tr>
<td>5.76</td>
<td>0.40</td>
<td>0.0166</td>
<td>0.006</td>
<td>1.00</td>
</tr>
<tr>
<td>6.66</td>
<td>0.40</td>
<td>0.0166</td>
<td>0.006</td>
<td>1.00</td>
</tr>
<tr>
<td>7.2</td>
<td>0.40</td>
<td>0.0166</td>
<td>0.006</td>
<td>1.00</td>
</tr>
<tr>
<td>5.80 Mean. 0.100</td>
<td>8.01 Mean. 0.220</td>
<td>6.51 Mean. 0.105</td>
<td>8.13 Mean. 0.220</td>
<td>6.02 Mean. 0.126</td>
</tr>
<tr>
<td>5.89 0.100</td>
<td>8.06 Mean. 0.225</td>
<td>6.13 0.126</td>
<td>8.13 Mean. 0.225</td>
<td>5.17 0.130</td>
</tr>
<tr>
<td>9.00 0.100</td>
<td>8.17 Mean. 0.225</td>
<td>6.13 0.126</td>
<td>8.13 Mean. 0.225</td>
<td>5.17 0.130</td>
</tr>
<tr>
<td>9.99 0.100</td>
<td>8.17 Mean. 0.225</td>
<td>6.13 0.126</td>
<td>8.13 Mean. 0.225</td>
<td>5.17 0.130</td>
</tr>
<tr>
<td>6.02 0.126</td>
<td>8.17 Mean. 0.225</td>
<td>6.13 0.126</td>
<td>8.13 Mean. 0.225</td>
<td>5.17 0.130</td>
</tr>
<tr>
<td>0.126</td>
<td>8.17 Mean. 0.225</td>
<td>6.13 0.126</td>
<td>8.13 Mean. 0.225</td>
<td>5.17 0.130</td>
</tr>
<tr>
<td>0.126</td>
<td>8.17 Mean. 0.225</td>
<td>6.13 0.126</td>
<td>8.13 Mean. 0.225</td>
<td>5.17 0.130</td>
</tr>
<tr>
<td>0.126</td>
<td>8.17 Mean. 0.225</td>
<td>6.13 0.126</td>
<td>8.13 Mean. 0.225</td>
<td>5.17 0.130</td>
</tr>
<tr>
<td>0.126</td>
<td>8.17 Mean. 0.225</td>
<td>6.13 0.126</td>
<td>8.13 Mean. 0.225</td>
<td>5.17 0.130</td>
</tr>
</tbody>
</table>

Two of the observations performed, the one immediately after the other, are omitted from the above Table as being probably erroneous.

Resistance = 240 millims. reduced length of water column.

<table>
<thead>
<tr>
<th>Number of observations</th>
<th>A. Charge measured by distance of discharge in millimetres</th>
<th>B. Alteration in distance of discharge from one discharge to the other in millimetres</th>
<th>B/A</th>
</tr>
</thead>
<tbody>
<tr>
<td>32</td>
<td>Maximum ... 3.00</td>
<td>1.75</td>
<td>0.449</td>
</tr>
<tr>
<td></td>
<td>Mean ......... 3.81</td>
<td>1.03</td>
<td>0.270</td>
</tr>
<tr>
<td></td>
<td>Minimum .... ... 3.80</td>
<td>0.60</td>
<td>0.158</td>
</tr>
<tr>
<td>35</td>
<td>Maximum ... 5.60</td>
<td>2.50</td>
<td>0.446</td>
</tr>
<tr>
<td></td>
<td>Mean ......... 5.25</td>
<td>1.61</td>
<td>0.307</td>
</tr>
<tr>
<td></td>
<td>Minimum .... ... 5.55</td>
<td>1.00</td>
<td>0.180</td>
</tr>
<tr>
<td>25</td>
<td>Maximum ... 6.55</td>
<td>3.35</td>
<td>0.511</td>
</tr>
<tr>
<td></td>
<td>Mean ......... 6.78</td>
<td>2.72</td>
<td>0.401</td>
</tr>
<tr>
<td></td>
<td>Minimum .... ... 6.70</td>
<td>1.40</td>
<td>0.209</td>
</tr>
<tr>
<td>16</td>
<td>Maximum ... 8.70</td>
<td>5.10</td>
<td>0.586</td>
</tr>
<tr>
<td></td>
<td>Mean ......... 8.30</td>
<td>4.17</td>
<td>0.502</td>
</tr>
<tr>
<td></td>
<td>Minimum .... ... 8.20</td>
<td>2.20</td>
<td>0.268</td>
</tr>
</tbody>
</table>

On looking at these two Tables, we are at once struck with the fact that for both resistances the relation $\frac{B}{A}$ of the eliminated electricity to the original charge increases with the distance of discharge; and this is the case both with the mean value obtained from a number of observations, and for the minimum and maximum value of them. When I made the observations described above, the relations of the intermittent discharge were entirely unknown to me. The result observed was consequently a source of great surprise to me, because it appeared scarcely reconcilable with any theoretical view. Proceeding, namely, from the hypothesis of a continuous discharge, there is no reason why, even when the resistances are great, as well as when the metallic circuit connexion is short, such quotient should not be constant with the most various charges*. Admitting, however, an intermittent discharge, it is possible for a difference in this respect to exist between great and small charges, for by the latter the continuous discharge alone may take place. In what way, however, the intermittent discharge by itself can cause an increase in the quotient $\frac{B}{A}$, is not immediately apparent; for such influence must be caused either by the number of the partial discharges or the value of a single partial discharge, or

* The circumstance that the quantities of electricity were measured by the distance of discharge, would here only account for a very small divergence from the proportionality.
by both. Concerning the value of a single partial discharge, that is, the relation between the electricity eliminated by it to the original charge, it is clear that such relation depends wholly upon the quantity of electricity concentrated upon the polar spheres (or upon one of them when the other is connected with the earth), if the statical electricity upon the thin connector of the great-resistance column may be neglected. Let us therefore consider the polar substance in connexion with the internal jar-coating. The irregular form of the body forbids the employment of calculation here; but by a simple course of reasoning, we may explain the nature of the changes; i.e. whether at the moment before the discharge, the quantity of electricity concentrated upon the polar substance increases proportionally to the charge, or more quickly, or more slowly; and the solution of this question is for the present sufficient.

It is, namely, evident that, without the induced action of the polar body in connexion with the earth, the second pole, which, with the internal coating, forms a continuous, invariable whole, always has upon its surface a definite aliquot part of the entire charge. This is also evidently approximately true if the first-named connected polar body be considered as present, but at a sufficient distance (at a great distance of discharge). If, now, the distant pole be brought nearer, on the one hand the free electricity upon the pole in connexion with the internal coating would remain the same with respect to the entire charge, on the other hand a certain quantity of fixed electricity* would be withdrawn from the reservoir, in consequence of the inducing action of the earth-connected pole. Such abstraction would always increase as the distance diminished. The entire electricity upon the pole (whose unit of quantity is always given by the corresponding charge) is therefore the sum of a constant and a variable, the latter of which increases as the distance of discharge increases. Hence, on increasing the total charge, the value of a partial discharge is not increased to the same extent‡.

Accordingly, if \( \frac{B}{A} \) were conditioned wholly or principally by the value of the partial discharge, its change could only take place in the opposite direction to that which is shown by ex-

* The capacity of the reservoir is always assumed to be so great that the loss of this electricity alters the density of the free electricity in the jar inappreciably.

‡ Perhaps the explanation gains in perspicuity by remembering that not only the jar, but also the system of the two polar bodies may be regarded as an electrical condenser, with the exception that in this case the thickness of the isolating coating is variable.
experiment to be the case. We have therefore to seek for some other cause capable not only of counteracting the influence of the variable just found, but of overpowering it. The knowledge of the phenomena which I have observed by the intermittent discharge may give us the clue to this.

Remembering that the interval of time between two partial discharges becomes less as the distance of discharge increases and bearing in mind the atmospheric disturbances caused by such partial discharges, it seems to be a natural consequence, that the total number of the partial discharges increases with the charge and the distance of discharge. This increase, concerning which nothing further is at present known, may possibly, when the distance of discharge increases, overcome the weakening influence of the electro-static arrangement. After I had learned to recognize the intermittent discharge directly, I was quite convinced that with the above-mentioned resistances (4830 millims. and 240 millims. reduced length of water column) I had had to deal exclusively with discharges of this kind.

If the same reasoning be applied to the experiments which were made by the determination of the duration of an electrical total discharge, the two facts observed seem partly to corroborate one another.

LIX. Chemical Notices from Foreign Journals.  
By E. Atkinson, Ph.D.  
[Continued from p. 368.]

Deville* has made a communication on what he terms "the disassociation of compound bodies." When heat acts upon a body it produces expansion, which is considered to be due to the action of a repulsive force of heat; and if a suitable compound be heated sufficiently high, the distance between the molecules may become so great that the latter pass into the free state. Such phenomena Deville classes under the above head. Thus anhydrous nitric acid spontaneously decomposes into its elements even at ordinary temperatures, anhydrous carbonate of ammonia at 60° C., and ammonia at a red heat. Grove has shown that platinum, at its melting-point, decomposes water into its elements. On repeating the experiment on a large scale, Deville found that only a very small quantity of detonating gas was produced in proportion to the heat employed. This arose from the fact that the liberated elements again combine, and that only those portions escape recombination which are immediately cooled down to the ordinary temperature by the sur-

rounding water. This was confirmed by an experiment in which high-pressure steam was passed through a platinum tube filled with pieces of lime, and heated nearly to the melting-point of platinum. In this case only a small quantity of detonating gas was formed.

Deville endeavoured to ascertain the temperature at which this occurred. Regnault had observed that silver at its melting-point decomposes water, which it again liberates in exhibiting the phænomenon of "spitting." Deville endeavoured to explain this by assuming that the water, in passing through the heated porcelain tube used in the experiment, decomposed into its elements, and on this depended the absorption of free oxygen, but that silver had not a sufficient affinity for oxygen to decompose water. He sought to confirm this explanation by using for the experiment an oxide readily reduced by hydrogen at a low temperature, and which is consequently itself incapable of decomposing water. A long and broad platinum tray, filled with pure fused litharge, was placed in a porcelain tube which it filled to two-thirds. The porcelain tube was heated to bright redness, and a rapid current of steam passed through. Much oxide of lead volatilized, and at the conclusion of the experiment there were evidences that the oxide of lead in solidifying had disengaged oxygen. On examining the tube, it was found that those parts exposed to the strongest heat showed symptoms of glazing, from the volatilized oxide of lead; further on there was pulverulent oxide of lead, and finally a mirror of reduced lead. In passing through the tube the water had been decomposed into its elements, the oxygen had been absorbed by the litharge, and the hydrogen had reduced some oxide of lead, but only where the decomposed water could be again formed. Deville estimated this temperature at very near the melting-point of silver.

Although water cannot exist at the melting-point of silver, still by the combination of hydrogen and oxygen so much heat is liberated that iridium can be melted. How is it that the oxyhydrogen flame melts platinum, and that molten platinum again decomposes water? If we could compare the quantity of platinum melted by a given weight of detonating gas with the quantity of water decomposed into hydrogen and oxygen by the platinum thus melted, a conclusion might be drawn in reference to the condition of these gases at the moment of that disengagement of heat at which water is no longer formed.

Deville describes an experiment which seems to prove that hydrate of soda, and especially hydrate of potash, easily decompose into their elements at a temperature near that of the melting-point of wrought iron. The experiment is essentially that of Gay-Lussac and Thenard, of the production of sodium. He
took a mercury-bottle provided at the mouth with an iron tube, and at the lower part, near the bottom of the bottle, with a second tube at right angles to the axis of the bottle. This bottle was heated in a blast-furnace in which a white heat could be produced, the roof of the furnace being perforated to allow the vertical tube to pass through. The bottle was filled either with pure iron filings reduced by hydrogen, or with pure iron sponge. When the temperature in the furnace had reached white heat, hydrate of potash was passed into the bottle by means of the vertical tube, and the horizontal tube was provided with a receiver. As soon as potassium had been obtained, the operation was rapidly stopped, when he observed—

1. That as soon as the supply of potash ceased to be very rapid, much potash was volatilized. Potassium is only obtained when large quantities of potash are added through the upper tube.

2. On sawing across the bottle, the iron in the part of the bottle which had been hottest was found to be as bright as before the experiment, and without a trace of oxide. In the lower part, where there had been less heat, there was a magma of oxide of iron and potash.

3. If the whole bottle is kept at the temperature at which the magma in the previous experiment is formed, no trace of potas-
sium, but only hydrogen and potash are formed.

It may therefore be concluded that, in passing over the white-hot iron, the hydrate of potash decomposes into its elements, and potassium, hydrogen, and oxygen in their free state reach the lower part of the apparatus. Only a portion of free oxygen is absorbed by the iron, and hence potassium is formed, which is carried away in the rapid current of hydrogen; and the potash mechanically prevents the oxide of iron from giving up its oxygen to the potassium, or to the excess of hydrogen.

The best proof that the iron on the hottest parts of the apparatus only serves for the transmission of heat is the fact that, if the whole bottle be kept at the somewhat lower temperature which the lower part of the bottle has when potassium is pro-
duced, only hydrogen and potash vapour are formed.

Deville finally mentions the great influence which velocity has upon those chemical actions which depend on the decomposition of compounds into their elements in the preparation of change-
able substances. Oxide of zinc volatilizes in a slow current of hydrogen, but is reduced in a rapid stream. In the same man-
ner, the use of chalk in Deville's preparation of sodium consists in the effect of the large quantity of carbonic oxide formed by the action of the charcoal on the chalk. Experiments in this direc-
tion will furnish the elements of a chapter of chemical dynamics, in
which those phenomena will be explained where velocity enters as influencing the result of chemical affinity.

In the above communication the view has been again raised and furnished with new grounds by Deville, that a body may decompose at a higher temperature, and at a lower temperature be again formed out of the elements into which it had decomposed,—so that, knowing the starting-point and ultimate results of the action of heat upon a substance, no change would appear to have taken place. Kopp* points out that, in the fact that this is possible, and probably is more frequent than is generally supposed, an explanation may be found of the unusual condensation shown by a number of substances in the vaporous condition.

Bodies usually correspond to a condensation to one, two, or four volumes. But there are compounds which, from the determinations of their vapour-densities, show condensations differing from these; and some of them, which may be considered as determined with certainty, are as if they had been decomposed, at the temperature at which the vapour-density was taken, into substances out of which they could be again formed at lower temperatures.

For the haloid compounds of ammonium, NH₄Cl, NH₄Cy, analogy with HCl would lead us to expect a vapour-density corresponding to a condensation to 4 volumes. Bineau found that their vapour-density corresponded to a condensation to 8 volumes; and it is probable that at their volatilizing-point they decompose into two substances whose space together occupies 8 volumes—into NH₃ = 4 vols., and HCl or HCy, also = 4 vols. The same is probably the case with the compounds PH₄Br and PH₄Cl.

Analogy with HS would lead us to expect for NH₄S a condensation to 2 volumes. Bineau’s determination gave 6 vols., just as if the compound had decomposed into NH₃ = 4 vols., and HS = 2 vols. The vapour-density of NH₄S, HS ought to correspond to 4 vols. Bineau found it to be 8 vols. Probably it decomposes into NH₃ = 4 vols., and into 2 HS = 4 vols.

Cahours found that PCl₅ showed, at temperatures which lie so far above the boiling-point of the body that its vapour density was constant, a condensation to 8 vols. This might be a decomposition into PCl₃ = 4 vols. and 2Cl = 4 vols.

Carbamate of ammonia, NH₄O, NH₂C²O³ (the body generally termed anhydrous carbonate of ammonia, 2NH₃, 2CO₂), was found by Rose and Bineau to show a condensation to 12 vols., which would be as if it had split up into 2NH₃ = 8 vols., and 2CO₂ = 4 vols.

Bineau had suggested the above explanation for carbamate of

* Liebig’s Annalen, March 1858.
ammonia, for hydrosulphide of sulphide of ammonium, and for cyanide of ammonium; and Gerhardt had explained the abnormal condensation to 8 vols. exhibited by the body \( C^4 Cl^6 O^2 \), obtained by the action of chlorine on methylcæther, by assuming that that density was not the density of its vapour, but the density of a mixture of phosgene gas, \( C^2 Cl^4 O^2 = 4 \) vols., and the chloride of carbon, \( C^2 Cl^4 = 4 \) vols., which might have been formed.

Wöhler has described a method* by which the preparation of inflammable siliciuretted hydrogen†, hitherto only formed by the aid of the galvanic current, is effected by a purely chemical process, and as readily as is that of phosphuretted hydrogen.

40 grms. of fused chloride of magnesium, 35 grms. of well-dried silicofluoride of sodium, and 10 grms. of fused chloride of sodium are finely pulverized in a hot mortar, well mixed, and then placed in a stoppered bottle. 20 grms. of sodium in small pieces are then added, and mixed with the powder by being well shaken. At the same time a Hessian crucible is raised to a full red heat, and the above mixture projected into it, the crucible rapidly covered, and the heat somewhat raised. When the action is over, and no more sodium flames issue from the lid, the crucible is taken out, allowed to cool, and broken. The result is a fused, blackish-gray slag, containing metallic, lustrous lamellæ and beads. This slag serves directly for the preparation of the gas. A slag which yields a spontaneously inflammable gas may also be prepared by taking other than the above proportions; and also when, instead of silicofluoride of sodium, a mixture of cryolite and water-glass is used; or instead of chloride of magnesium, fused chloride of magnesium and sodium.

To generate the gas, some of this slag coarsely pounded is placed in a small two-necked bottle, into one neck of which fits a funnel-tube reaching nearly to the bottom, and into the other a short and somewhat wide gas-delivery tube. The bottle is quite filled with water, and placed under the surface of the water in the pneumatic trough, so that the delivery-tube is also filled with water, and no air remains. A gas-jar filled with water is placed over the delivery-tube, and concentrated hydrochloric acid gradually added through the funnel-tube, care being taken that no air-bubbles are passed in at the same time. The gas is generated with great violence, and a thick foam is carried along with it into the jar, which, however, gradually settles down, so that the gas may be transferred from one jar to another. The whole opera-

* Liebig's Annalen, July 1858.
† Phil. Mag. August 1857, June 1858.
tion must be performed with boiled-out water; for with water containing air, the gas soon becomes cloudy and loses its spontaneous inflammability. The gas may be transferred to a jar over mercury, and at the same time dried, by collecting it in a jar with a stopcock, to which is attached a chloride of calcium tube, and a delivery-tube as narrow as possible. At first the gas takes fire in the tube which still contains air, and then burns at the mouth. It is then inserted under the mercury-jar.

Each bubble of the gas on issuing into the air takes fire with a violent explosion, and the resulting silica forms beautiful annular clouds. Issuing from a narrow tube into the air, it burns with a large, brilliant, white flame. Passed through a gently heated glass tube, it is readily decomposed, and the sides of the tube are covered with a mirror of reduced silicon. It also forms a mirror when its flame burns against a porcelain plate. Notwithstanding its great inflammability, the gas prepared by the above process contains a large quantity of free hydrogen, although less than that prepared by the electrolytic process. The gas precipitates solutions of sulphate of copper, nitrate of silver, and chloride of palladium. The copper and silver are precipitated as siliciurets, and the palladium is reduced to the metallic state.

The substance which yields the gas exists in the slag in the form of small particles, or very small globules. By effecting the reduction with the addition of fluor-spar at a very high temperature, a small proportion of dark iron-gray fused masses were obtained. With hydrochloric acid they gave off inflammable gas, leaving a residue of crystallized silicon and dense oxide of silicon*, the formation of which on the surface of the mass soon prevented the liberation of gas. But when these masses reduced to powder were digested with sal-ammoniac solution, spontaneously inflammable gas was liberated with great energy, and the solution contained magnesium. A gray metallic powder was left, which, treated with hydrochloric acid, gave off hydrogen, and left a residue of crystallized silicon and oxide of silicon. The metallic masses appear, therefore, to be a mixture of three substances,—of free silicon, of a siliciuret of magnesium, which yields with hydrochloric acid siliciuretted hydrogen, and of a siliciuret of magnesium, which forms with hydrochloric acid free hydrogen and oxide of silicon. The latter substance is contained in large quantity in the foam formed in the preparation of the gas.

In one preparation made on a large scale, small black, metallic, crystalline globules were obtained, an analysis of which gave numbers agreeing with the formula \( \text{SiMg}^2 \). If this decom-

* Phil. Mag. June 1858.
posed with hydrochloric acid exactly into chloride of magnesium and siliciuretted hydrogen, the latter would have the formula Si H^2, thus—

\[
\text{Si Mg}^2 + 2 \text{HCl} = \text{Si H}^2 + 2 \text{MgCl},
\]

and might contain in one volume half a volume of silicon gas and two volumes of hydrogen. But in the decomposition, oxide of silicon was formed, the production of which might, however, depend on a secondary process, and stand in connexion with the simultaneous occurrence of free hydrogen.

The manganese which Brunner* obtained by reduction of the fluorides and chlorides of sodium by means of sodium, was found by that chemist to possess other properties than that reduced from the oxide by charcoal; and on examining a specimen prepared by Brunner, Wöhler found that it dissolved in hydrochloric acid with difficulty, and left a residue of oxide of silicon; and it was hence probable that this difference in the properties depended on the presence of silicon which had been formed by the action of the fluorides on the substance of the crucible. Wöhler has made† a variety of experiments with the view of obtaining a definite compound of silicon and manganese.

A mixture of equal parts of fluoride of manganese, water-glass, cryolite, and sodium, firmly pressed in a Hessian crucible, and covered with a layer of a mixture of chloride of sodium and chloride of potassium, and then heated in a furnace, yielded a well-fused, hard and brittle regulus; the fracture of which had indications of a lamellar crystalline aspect, but appeared to contain no free silicon. It was much less readily attacked by acid than was Brunner's manganese. Each piece became covered with a crust of dense oxide of silicon; and the gas which escaped consisted of hydrogen containing siliciuretted hydrogen; for on being passed through a gently heated tube, a beautiful mirror of silicon was formed. This specimen contained 11·7 per cent. of silicon.

Another experiment made with a mixture of fused chloride of magnesium and sodium, fluor-spar, water-glass and sodium, gave a fine brittle regulus containing a few small cavities lined with steel-coloured prismatic crystals. This siliciuret of manganese contained 13 per cent. of silicon, thus nearly corresponding to the formula Mn^8Si, which would require 13·37 per cent.

In two experiments made with sodium, chloride of manganese and sodium, and silico-fluoride of potassium without the addition of fluor-spar, no manganese was obtained. In another experiment a mixture of chloride of manganese, sodium, cryolite, and quartz

* Phil. Mag. vol. xv. p. 112.
† Liebig's Annalen, April 1858.
sand was taken. The regulus had indications of a lamellar crystalline structure, and a colour slightly inclining to yellow. It contained 11 per cent. of silicon.

Brunner had found that manganese prepared by his process, without the intentional addition of silicon, contained from 0·6 to 6·4 per cent.; and in one preparation in which silicofluoride of potassium was added, a regulus was obtained containing 9·86 per cent. of silicon*. He found that the proportion of silicon may be diminished by mixing the regulus in coarse powder with twice its weight of chloride of sodium, to which 1 per cent. of chlorate of potash had been added, and keeping the mixture for a few minutes at a white heat. In this manner the silicon is oxidized and passes into the slag, and the proportion of silicon is diminished to 0·1 per cent. But the metal thus prepared is not sensibly altered either in colour, hardness, fusibility, or lustre; and Wöhler is inclined to think that even such a small proportion of silicon is sufficient to alter the properties of the metal, especially as the different kinds of iron—bar iron, steel, and cast iron—show something similar.

Schweizer found† that ammoniacal solution of oxide of copper, prepared by dissolving hyposulphate of copper or basic sulphate of copper in strong ammonia, has the property of dissolving vegetable fibre even at ordinary temperatures. Cotton immersed in the liquid soon becomes gelatinous, and finally dissolves-up to an almost clear blue solution, which when much diluted with water may be filtered. Hydrochloric acid added to this solution precipitates a voluminous white mass, which appears to be disintegrated but not essentially altered cellulose. Paper and linen also dissolve, but more slowly. Silk dissolves even more rapidly than cotton; and acids precipitate from the filtered solution a gelatinous mass. Wool is only dissolved when heat is applied. Hair swells up without dissolving. Bladder at first swells up, and then dissolves. Starch merely swells up, but inuline dissolves. Cramer‡ investigated the deportment of Schweizer’s reagent towards cellular membrane, starch, inuline, and cellular nucleus, and found Schweizer’s observations confirmed. He also observed that the solvent properties of the solution are diminished by being kept long, and also by the frequent access of air.

Schlossberger§ has recently observed that ammoniacal solution of oxide of nickel has the property of dissolving silk, and,

* Poggendorff’s Annalen, vol. ciii. p. 139.
§ Liebig’s Annalen, July 1858.
as it does not dissolve cotton, affords a means of distinguishing the two substances. When silk is stirred in a test-glass with the solution by means of a glass rod, it becomes of a yellowish-brown colour, like that of hydrated oxide of iron, gelatinous, and finally dissolves into a homogeneous solution of a yellowish-brown colour. Solutions of the alkalies do not precipitate this solution; sal-ammoniac restores to it its blue colour without precipitating it. By acids the silk is precipitated. Cellulose is not at all attacked by the ammoniacal oxide of nickel solution. Starch does not swell up; but inuline is gradually dissolved.

The same chemist has communicated* some facts in reference to Schweizer's reagent. The solvent property of CuO NH₃ is greatly diminished by the presence of salts. The reagent is conveniently prepared by dissolving freshly precipitated and well washed hydrated oxide of copper in strong ammonia. The richer the solution in oxide of copper, the more rapid is the action; even the strongest ammonia is without action if it is poor in oxide of copper. The clear filtered solution of cellulose is immediately precipitated by strong solutions of the alkalies: the same is the case, though even more rapidly and completely, with honey, or with a concentrated aqueous solution of gum-arabic or dextrine. Cellulose is undoubtedly contained in the solution as such. Silk is dissolved into a blue solution, its solution in nickel being yellowish brown. Salts, solution of sugar, and mucilage precipitate the solution either not at all or very sparingly, and even acids but imperfectly.

**LX. Proceedings of Learned Societies.**

ROYAL SOCIETY.

[Continued from p. 473.]

April 29, 1858.—J. P. Gassiot, Esq., Vice-President, in the Chair.

The following communications were read:—


"On the Structure and Functions of the Hairs of the Crustacea." By Campbell De Morgan, Esq.

"Note on the Measurement of Gases in Analysis." By A. W. Williamson, Ph.D., F.R.S., Professor of Chemistry in University College, and W. J. Russell, Ph.D.

In Bunsen's admirable method of gas analysis, considerable time

* Liebig's Annalen, July 1858.
and trouble are expended in observing the exact temperature and pressure to which the gas is subjected at the time of measurement; and also in calculating from these data the volume which the gas would occupy at the normal temperature and pressure. Frankland's excellent apparatus, on the other hand, protects the gas from the influence of variations of atmospheric pressure, and, under favourable conditions, even from the influence of change of temperature; but the complication of this apparatus, and its liability to derangement, seem likely somewhat to limit its use.

If, when a fall of temperature takes place, we could diminish the pressure on the gas exactly in proportion to the diminution of elasticity which it undergoes, such fall of temperature would evidently not alter the volume of gas in the eudiometer. In like manner a rise of temperature might, if known, be counteracted by lowering the eudiometer-tube. The same remarks apply to variations of barometric pressure; as an increase of this influence might be counterbalanced by raising the eudiometer, and a diminution by depressing it.

It is therefore a question of some interest, to find, for any atmospheric temperature and pressure, at what height of the eudiometer the enclosed gas will occupy the same volume as at the normal temperature and pressure. This is easily found by introducing a standard quantity of air into a tube over mercury, marking off the height of the mercury in the tube at the normal temperature and pressure, then, at any other temperature or pressure, raising or lowering the tube in the mercurial trough so as exactly to bring the enclosed air to its normal volume. The mercurial pressure needed for this purpose is evidently the same as that needed under the same circumstances for the reduction of any quantity of gas to the volume which it would occupy at the normal temperature and pressure.

The apparatus we use in applying this principle to gas-analysis (fig. 1) consists essentially of the ordinary Bunsen's eudiometer, and a "pressure-tube," which is simply a tube of some 6 or 7 inches in length, and about the diameter of an ordinary eudiometer. It is closed at one end; and to the other is fixed a smaller tube of about the same length. Such a quantity of air is introduced into this pressure-tube, that when it is inverted in the trough the mercury stands at a convenient height in the narrow tube. At this point a mark is made, which indicates the height of mercury needed at any temperature or pressure to reduce the enclosed air to its original volume. The mercurial trough which we have used differs only from the ordinary one in being provided with a well at one end, thus enabling the operator to raise or depress the eudiometer at pleasure, so as always to bring the gas which it contains to the same pressure as the air in the pressure-tube. Both the eudiometer and the pressure-tube are held in a perpendicular position by means of clamps which slide on upright rods. Each clamp is provided with a simple kind of slow movement, by which the tube can be raised or lowered by the operator whilst he is looking through a horizontal telescope at a suitable distance. We place the pressure-tube in front
of the eudiometer, and by means of the fine adjustment bring the column of mercury in the small tube exactly to the normal mark. The endiometer is then adjusted, also by means of the slow movement, so that the top of its meniscus (as seen through the horizontal telescope) exactly coincides with the top of the meniscus in the pressure-tube. This is easily done; for the diameter of the pressure-tube is considerably smaller than that of the eudiometer, and the meniscus in the latter can be clearly seen on both sides of the meniscus in the pressure-tube.

By this method we are able to obtain very accurate results with considerably less trouble than by Bunsen’s method, and also without having any calculations to perform. The following analyses made during very stormy weather, of air deprived of its carbonic acid by potash, gave results amongst which the greatest difference was only four-hundredths of a per cent. (0.04).

I.

<table>
<thead>
<tr>
<th>Volume of air taken</th>
<th>144.81</th>
</tr>
</thead>
<tbody>
<tr>
<td>After addition of hydrogen</td>
<td>234.50</td>
</tr>
<tr>
<td>After explosion</td>
<td>144.00</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>79.168</td>
</tr>
<tr>
<td>Oxygen</td>
<td>20.832</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100.000</strong></td>
</tr>
</tbody>
</table>

II.

<table>
<thead>
<tr>
<th>Volume of air taken</th>
<th>139.55</th>
</tr>
</thead>
<tbody>
<tr>
<td>After addition of hydrogen</td>
<td>229.07</td>
</tr>
<tr>
<td>After explosion</td>
<td>141.89</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>79.176</td>
</tr>
<tr>
<td>Oxygen</td>
<td>20.824</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100.000</strong></td>
</tr>
</tbody>
</table>

III.

<table>
<thead>
<tr>
<th>Volume of air taken</th>
<th>148.1</th>
</tr>
</thead>
<tbody>
<tr>
<td>After addition of hydrogen</td>
<td>236.04</td>
</tr>
<tr>
<td>After explosion</td>
<td>143.30</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>79.139</td>
</tr>
<tr>
<td>Oxygen</td>
<td>20.861</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100.000</strong></td>
</tr>
</tbody>
</table>

IV.

<table>
<thead>
<tr>
<th>Volume of air taken</th>
<th>149.14</th>
</tr>
</thead>
<tbody>
<tr>
<td>After addition of hydrogen</td>
<td>218.57</td>
</tr>
<tr>
<td>After explosion</td>
<td>155.28</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>79.150</td>
</tr>
<tr>
<td>Oxygen</td>
<td>20.850</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100.000</strong></td>
</tr>
</tbody>
</table>

We are still engaged in experiments on this and some other points.
of gas analysis, and hope to have the honour of communicating our results before long.

DESCRIPTION OF THE FIGURES.

Fig. 1 represents the whole apparatus.
Fig. 2 represents the clamp with the fine adjustment attached to it.
A is the part which slides up and down the vertical rod; it is furnished on the inside with a small steel peg which moves in a groove, thus causing this arm always to remain in the same plane.
C D is a tube through which the rod F carrying the clamp passes.
E is a screw which retains the rod F in its place, and by means of which the friction of the rod passing through the tube can be increased.
G is the fine adjustment. As this small cylinder is turned round to the right or to the left, so the string either above or below it is wound on to it, and consequently the rod F raised or lowered.
H is merely an arrangement by which the string can always be tightened.
K is a peg so placed with regard to the stop L, that when, by turning the clamp round, it is pressed against the stop, the tube is then in the right position for applying the final adjustment and reading off.

"On the Theory of Internal Resistance and Internal Friction in Fluids; and on the Theories of Sound and of Auscultation." By Robert Moon, Esq., M.A., late Fellow of Queen’s College, Cambridge.

The author shows, in the first instance, that when sound is propagated along a cylindrical tube filled with air, the compression which takes place in any element calls forth a resistance which diminishes the velocity of the particles in the element, at the same time that the dilatation which takes place in any element calls into play a force which will tend to increase the velocity of the particles in the element. He considers that the amount of the force thus called into play (whether it be accelerative of, or retarding the motion), in an element of given magnitude in a given indefinitely short interval, will depend solely on the amount of compression or dilatation developed in the element in the interval, and the state of density in the element at the time; and he is thus led to the conclusion, that to the ordinary equation for the transmission of sound through a column of air must be added a term of the form

\[ \pm b^2 \left( \frac{dy}{dx} \right)^{-1} \frac{d^2y}{dx
dt^2} \]

where \( x \) denotes the distance from the origin of the element when the air is at rest, \( y \) the same distance at the time \( t \) when the air is in motion, \( b^2 \) a constant depending on the compressibility of air under given circumstances,—so that the accurate equation of sound (variation of temperature being neglected) will stand

\[ \frac{d^2y}{dt^2} = a^2 \left( \frac{dy}{dx} \right)^{-2} \frac{d^2y}{dx^2} \pm b^2 \left( \frac{dy}{dx} \right)^{-1} \frac{d^2y}{dx
dt^2} \]

in which equation the upper or lower sign of \( b^2 \) is to be taken according as the motion of the particles is in the direction in which \( x \) is measured positively, or the contrary.

On the same principles the author shows that, in the case of elastic fluids, the general equations of motion, when internal resistance is taken into account, must be written as follows:—
\[
\begin{align*}
\frac{1}{\rho} \frac{dp}{dx} &= X - \frac{d(u)}{dt} \pm k^2 \frac{du}{dx} \\
\frac{1}{\rho} \frac{dp}{dy} &= Y - \frac{d(v)}{dt} \pm k^2 \frac{dv}{dy} \\
\frac{1}{\rho} \frac{dp}{dz} &= Z - \frac{d(w)}{dt} \pm k^2 \frac{dw}{dz}
\end{align*}
\]

where \( \rho \) denotes the density; \( X, Y, Z \) the impressed forces acting on the element; \( u, v, w \) the resolved parts of the velocity parallel to the coordinate axes; \( \frac{d(u)}{dt} \) the total differential coefficient of \( u \) with respect to \( t \), &c.; and \( k^2 \) replaces the \( b^2 \) of the preceding case. The author considers that, for moderate ranges of density, the above equations accurately represent the whole internal resistance.

It is next shown, that when the fluid is inelastic, the same equations will represent the motion, provided that we obliterate \( \rho \) in the terms involving \( k^2 \).

The force of internal friction in an elastic fluid in which the whole motion takes place parallel to the axis of \( x \), and in which the whole lateral variation of motion transverse to the axis of \( x \) occurs in a direction parallel to the axis of \( z \), is then shown to be properly represented by \( \pm n^2 \rho \frac{du}{dz} \), where \( n^2 \) is a constant depending on the nature of the fluid; the sign of the term to be introduced into the equation of motion being determined by the consideration that friction must always be a retarding force. The author thence derives the conclusion, that in order to represent the effect of internal friction in the motion of an elastic fluid, we must add to the first of equations (2) a term of the form

\[ \pm n^2 \frac{u}{V^2} \cdot \rho \psi, \]

where

\[ V^2 = u^2 + v^2 + w^2, \]

and

\[ \psi = \left( u \frac{dV}{dy} - v \frac{dV}{dx} \right)^2 + \left( w \frac{dV}{dx} - u \frac{dV}{dz} \right)^2 + \left( v \frac{dV}{dz} - w \frac{dV}{dy} \right)^2, \]

and similarly with regard to the other two equations. When the fluid is inelastic, the terms in the equations of motion depending upon friction will be identical with those in the preceding case if we obliterate from the latter \( \rho \).

Reverting to the equation of sound, which (neglecting terms of the second order) may be put under the form

\[ \frac{d^2 y}{dt^2} = a^2 \frac{d^2 y}{dx^2} + 2ae \frac{d^2 y}{dx \, dt} \]

the author next shows that if the initial disturbance consist of a condensation alone, it will be transmitted with the velocity \( a(1 - e) \) in the direction in which its particles are moving; and that if it consists

*Phil. Mag. S. 4, No. 110, Suppl, Vol. 16.*
of a rarefaction alone, it will be transmitted with the velocity $a(1 + e)$ in the direction contrary to that in which its particles are moving. It is here shown also incidentally, that whether the resistance be taken into account or not, the particles of a wave of condensation must all move in the same direction, which will be the direction of transmission; and the particles of a wave of rarefaction will all move in the same direction, which will be contrary to that of transmission.

In confirmation of the conclusion that waves of rarefaction are transmitted more rapidly than waves of condensation, the author adduces the fact, that when explosions of gunpowder have taken place, the glass in windows has been observed to break outwards rather than inwards.

It is then suggested that, as when sound is produced a condensation and rarefaction of air usually occur in immediate succession, if both kinds of disturbance were capable of affecting the human ear, we should hear sounds double; and as we know practically that this is not the case, it is contended that only one kind of disturbance, i.e. either rarefaction alone, or else condensation alone, can stimulate the ear.

It is shown to be à priori probable, that if one of the two classes of aerial disturbance is suppressed by the ear, that one would be disturbance by condensation, inasmuch as waves of rarefaction being swifter, would better perform the duty entrusted to them: and it is pointed out that if the sensation of sound is produced by aerial rarefactions alone, a difficulty attending the received theory will be obviated, by reason of the velocity deduced upon that theory being too small.

The author considers, however, that the question, whether either and which of the kinds of aerial disturbance is suppressed, can only be satisfactorily determined by examination of the ear itself. He accordingly endeavours to establish, by arguments derived from the structure of the ear, that aerial rarefactions are alone capable of stimulating that organ in man. These arguments are briefly as follows:—

1. The tympanal membrane being convex inwards, a condensation could only affect the air in the tympanal cavity by stretching the membrane, which would cause an expenditure of force, whereas a rarefaction would produce the effect by a simple flexure of the membrane.

2. The sense of hearing being certainly produced by the motion of the fluid in the labyrinth, which is a closed vessel filled with an incompressible fluid, the requisite motion could not be produced by a compression of the atmosphere.

3. The disposition of the muscles of the ear is such as is calculated to assist and regulate the impressions produced by rarefactions rather than those produced by condensations.

4. The existence of the Eustachian tube is indispensable to the action of the organ (when all its other parts are in a normal state), on the supposition that sound is occasioned by rarefaction, whereas its uses are not satisfactorily predicable on the contrary hypothesis.
The author observes that if rarefactions alone produce sound, it follows that a simple contraction of the muscles of the ear will render sounds inaudible. It follows also, on the same hypothesis, that a more delicate exercise of the same muscles will render the organ minutely susceptible to the influence of certain sounds, to the exclusion of others. It is urged also, that, admitting the action of these muscles to be to a large extent involuntary, there can be no doubt that by practice a great degree of command may be acquired over them. The author conceives that we may in this way account for the facility acquired by many persons of reading and writing, and of carrying on intricate trains of thought, without being disturbed by, or being conscious of, the noises around them. He conceives also that the same mode of explanation may be applied to account for the power of appreciating and analysing the most complex harmonies possessed by persons having a fine musical ear; which the author considers to be as certainly the result of specific mental and muscular training, as the faculty of vocalization, or the art of playing on a keyed instrument.

The author concludes by observing that the equations for the transmission of an undulation along a musical string require a similar correction to that introduced in the case of aerial vibrations. The discussion of this branch of the subject he reserves for a future opportunity.

May 6.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On the Influence of Heated Terrestrial Surfaces in disturbing the Atmosphere." By Thomas Hopkins, Esq.

In this paper the author stated that the Hadleian theory of winds, which is now the one generally recognized, is not supported by the evidence of facts, but rests on assumptions founded on imaginary effects of the partial expansion of the atmospheric gases by heat. It is assumed in that theory, that when the tropical heat expands these gases, they rise and flow away laterally in the higher regions towards the poles, from which they return to the tropics in the lower regions. But it was contended by the writer of the paper, that such heating of the gases merely expands them, without making them rise and overflow to other parts. The theory of Halley, once generally adopted, represented that the air was greatly heated in the particular part where the sun was nearly vertical, which made the air rise in that part alone, admitting cooler air to flow into the place of that which had ascended, and produced an influx of cool air below, from all parts around, to the heated part, and an overflow above from it. But in time experience showed that this hypothesis was not in accordance with facts, and it was abandoned. The theory of Hadley, which has been since adopted, substitutes the whole tropical belt, for the heated locality of Halley, which travelled with the sun in his daily course; but the supposed rise of air in the tropical belt, with an overflow above and an influx below, was asserted to be equally unsupported by experience, and, being unproved, may be fallacious.
The rise of heated air in a chimney, sometimes pointed at as an illustration, was shown to be not analogous to that which takes place when the sun heats the air unequally in different latitudes; if it were, the theory of Halley would be true, and cool air would flow from all parts around to the greatly heated locality, just as cool air passes to a fire, and, when heated, up a chimney. It was then shown that it is gravitation which establishes an equilibrium of pressure in the atmosphere, and that direct solar heating of the surface of the earth and the air near it, does not destroy that equilibrium. The sun by heating the gases merely expands them, in proportion to the increase of temperature in the part near the surface; and the gases over every portion of the hemisphere that is exposed to the action of the sun are proportionally heated, expanded and raised without any overflow of air taking place, leaving the equilibrium of pressure undisturbed by such heating. The solar heat merely raises the air that is near the surface, over the most heated latitudes, a little higher than the adjoining less heated, the difference in the rise in the various latitudes, from the polar to the tropical regions, being successively small; and as there is no alteration produced in the weight of any vertical column of the atmosphere, in any latitude, there is neither overflow of air above, nor disturbance of the equilibrium of pressure. The great disturbances that take place in the atmosphere were then maintained to be caused by the heat which is conveyed, from the surface of the globe, in vapour to different parts of the atmosphere at various heights, and liberated in those parts when the vapour is condensed into liquid. This liberation of heat creates ascending currents in the parts locally affected, when horizontal winds, produced by gravitation, blow over the surface towards the ascending currents to re-establish the disturbed equilibrium. This process, by heating the air in the middle regions, was asserted to have been proved to be the cause, not only of the great trade-winds and the monsoons, but of the storms and local winds over the different regions of the globe.

"Notes of Researches on the Poly-Ammonias." By A. W. Hofmann, LL.D., F.R.S.—No. II. Action of Chloroform upon Aniline.

In a former Note I have alluded to some new alkaloids which are produced by the action of the bromides of triatomic alcohols upon the primary amidogen bases.

I have since examined more minutely one of these bodies. At the common temperature, chloroform and aniline may be left in contact for a considerable time without any change becoming perceptible. Even at the temperature of boiling water scarcely any reaction takes place. But on exposing for ten or twelve hours a mixture of about equal volumes of chloroform and aniline in sealed tubes to a temperature of 180° or 190° C., a hard brown crystalline mass is obtained, which consists chiefly of the hydrochlorates of aniline and of a new crystalline base.

To obtain this compound in a state of purity, the brown crystalline mixture formed in the digester-tubes is triturated with a small quantity of water, thrown upon a filter and washed with water. The first
washings chiefly consist of hydrochlorate of aniline, which base separates in oily globules on addition of potassa to the filtrate. By testing the filtrate in this manner from time to time, it is found that the basic body separated by addition of potassa gradually exhibits a tendency to solidify, and ultimately falls as a yellowish-white crystalline precipitate. The residue upon the filter is now dissolved in warm (not boiling) water, separated by a filter from a brown resinous insoluble substance, and precipitated by ammonia or potassa. The crystalline precipitate obtained in this manner is washed till free from alkali, and repeatedly crystallized from weak spirit. It is difficult to obtain it perfectly white, a yellowish substance, which appears to be partly formed during the process of solution, adhering with great pertinacity.

Thus obtained, the new base is a white crystalline powder; frequently it is obtained in minute scales, generally of a yellowish tint. It is insoluble in water, but readily dissolves in alcohol and ether. From the hot solution in these solvents it is precipitated by water as a yellow oil, solidifying on cooling with crystalline structure. It is easily dissolved by acids, with many of which it forms crystalline compounds. From the saline solutions thus produced the base is reprecipitated by potassa and also by ammonia. The salts of the new base are not very stable; their solutions, especially when heated for some time, inevitably contain more or less aniline, the crystalline base itself undergoing changes which I have not yet sufficiently examined.

The analysis of the new compound presents some difficulty. Even after protracted exposure over sulphuric acid in the exsiccator, it retains a small quantity of water, while a temperature of 100° is apt to decompose it.

The nature of the body was, however, readily established by the examination of a perfectly stable hydrochlorate, and also of a very definite platinum-salt.

The results obtained in the analysis of these salts establish for the new base the formula

$$C_{26}H_{12}N_2 = \left( \frac{C_{12}H_5}{\text{I}} \right)^n N_2.$$  

It is obviously formed by the substitution of the triatomic molecule \((C_2\text{I})^n\) for 3 equivalents of hydrogen in 2 molecules of aniline, which thus coalesce into a diamino molecule. Accordingly the base might be called diphenyl-formyl-diamine, that is, diammonia, in which 2 equivalents of hydrogen are replaced by 2 molecules of phenyle, and 3 equivalents of hydrogen by 1 molecule of formyle, 1 equivalent of hydrogen remaining unreplaced. Its formation is expressed by the equation

$$4 \left( \frac{C_{12}H_7N}{\text{I}} \right) + C_{26}H_{12}N_2 \text{HCl} = C_{26}H_{12}N_2 \text{HCl} + 2 \left( \frac{C_{12}H_7N}{\text{I}} \text{HCl} \right).$$  

As seen from this equation, the new base, although unmistakeably
corresponding to 2 molecules of ammonia, like many other poly-
ammonias, is monoacid.

The analysis of the hydrochlorate leads in fact to the formula
\[ C_{26}H_{12}N_2, HCl. \]

The platinum-salt contains
\[ C_{26}H_{12}N_2, HCl, PtCl_2. \]

The new derivative of aniline undergoes several remarkable changes
which require further elucidation.

"Note sur un Organe, placé dans le Cordon Spermatique, et dont
l’existence n’a pas été signalée par les Anatomistes." Par F. Giraldès,
Professeur Agrégé de la Faculté de Médecine, &c.

"On Chondrosteus, an Extinct Genus of Fish allied to the Stu-
rioride." By Sir Philip de Malpas Grey Egerton, Bart., F.R.S.

May 20.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On the Resistance of Tubes to Collapse." By William Fair-
bairn, Esq., C.E., F.R.S. &c.

The object kept in view by the author of these researches was to
determine the law which governed the resistance of cylindrical tubes
to external uniform pressure. The anomalous condition in which
these constructions have been placed in reference to the internal flues
of boilers, and the frequent fatal accidents from explosions produced
by collapse, have imperatively called for inquiry into the causes which
have led to these unfortunate results. Ever since the first introduc-
tion of the steam-engine as improved by Watt, and especially since
the increased demand for its construction, and its application to
almost every branch of industry and every system of transit, the
consideration of all circumstances which may affect its economy and
security, has become of vast public importance.

During the more early period which followed its first introduction,
the form of boiler and its powers of resistance to strain, were con-
siderations of much less importance than at present. Then the force
of steam, or the pressure under which it was generated, was only
about one-eighth, and in some cases less than a sixteenth of what it
now is. Besides, the fertile genius of Watt had provided against
accident, by a self-acting apparatus, which regulated not only the
pressure, but the supply of water to the boiler. Since that time a
total change has taken place in the construction and working of the
steam-engine; and boilers which were perfectly safe at 7 lbs. upon the
square inch, are absolutely inadequate for generating steam at 40 lbs.
to 50 lbs. on the square inch. This being the case, it follows that every
precaution becomes urgently necessary which may serve to increase
the strength, and equalize the resisting power of vessels containing
an element of such potent influence, and yet so essential to the com-
forts and enjoyments of civilized life.

Entertaining these views, the author goes on to say, that hitherto
it has been considered an axiom in boiler engineering, that a cylin-
drical tube, placed in the position of an internal flue, is equally strong in every part when subjected to uniform external pressure; the length not affecting the strength of a flue so placed. This rule is, however, only true when applied to tubes of infinitely great length, and it is very far from true when the length of the tube does not exceed certain limits, and when the ends are retained in form by being riveted to the boiler, and thus prevented from yielding to external pressure. These facts were fully demonstrated by the experiments related in the Paper, which, for obvious reasons were conducted under circumstances as nearly as possible analogous to those now in actual operation upon a larger scale. With this view, a large and powerful cylinder, 8 feet long, and 2 feet in diameter, was prepared for the reception of the tubes; and being acted upon by hydraulic pressure, collapse was produced, and the results recorded, as fully explained in the Paper. It will suffice here to state the more important conclusions derived from the investigation, which fell under the following heads: viz.—1st, the strength of tubes as affected by length; 2nd, the strength of tubes as affected by diameter; lastly, the strength of tubes as affected by thickness of metal.

1. On the first head, the strength as affected by length, the results are conclusive and interesting. Within the limits of from 1 foot 6 inches to about 10 feet in length, it is found that the strength of tubes similar in every other respect, and supported at the ends by rigid rings, varies inversely as the length, as may be seen from the following results obtained with 4-inch tubes.

**Resistance of 4-inch tubes to collapse.**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Inches.</td>
<td>Inches.</td>
<td>Inches.</td>
<td>lbs. per square inch.</td>
</tr>
<tr>
<td>4</td>
<td>.043</td>
<td>19</td>
<td>137</td>
</tr>
<tr>
<td>4</td>
<td>.043</td>
<td>60</td>
<td>43</td>
</tr>
<tr>
<td>4</td>
<td>.043</td>
<td>40</td>
<td>65</td>
</tr>
</tbody>
</table>

The remarkable differences in the resisting pressure of the above similar tubes will be at once apparent, and it will be found by calculation that they follow the law of inverse proportion, the same as those of larger dimensions, the strengths diminishing as the lengths are increased.

The same law of resistance is maintained in 6-inch tubes, giving, for a tube 30 inches long, 55 lbs., and for one 59 inches long only 32 lbs. on the square inch, as the pressure of collapse. Again, in 8-inch tubes we have, in a long series of experiments, 32 lbs. per square inch in a tube 39 inches long, and 39 lbs. in one 30 inches long. In the same manner all the experiments on tubes of 10 and 12, and up to 18 inches in diameter may be compared, and the law of resistance is in like manner shown to hold true in every case. Discrepancies to a certain extent do certainly occur; but they are comparatively small, and, as they appear to follow no law, are evidently to be accounted for from defects in the construction of the tubes inseparable from such a mode of research.
2. The strength as affected by the diameter.

A precisely similar law is found to hold in relation to the diameter. Tubes similar in other respects vary in their resistance to collapse inversely as their diameters; and with a view of testing this law, we may place the calculated pressure beside that derived from experiment, as under:

### Resistance of tubes to collapse 5 feet long.

<table>
<thead>
<tr>
<th>Diameter (Inches)</th>
<th>By Experiment, lbs. per square inch.</th>
<th>By Calculation, lbs. per square inch.</th>
<th>Variation, lbs.</th>
</tr>
</thead>
<tbody>
<tr>
<td>4</td>
<td>43·0</td>
<td>28·6</td>
<td>-3·4</td>
</tr>
<tr>
<td>6</td>
<td>32·0</td>
<td>20·8</td>
<td>+0·7</td>
</tr>
<tr>
<td>8</td>
<td>20·8</td>
<td>17·2</td>
<td>+1·2</td>
</tr>
<tr>
<td>10</td>
<td>16·0</td>
<td>14·3</td>
<td>+1·8</td>
</tr>
<tr>
<td>12</td>
<td>12·5</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The above variations are slight when compared with the resisting powers of the tubes; they are doubtless caused by the varying rigidity of the iron, or by defects in the cylindrical form. Similar results follow in the experiments on tubes 2 feet 6 inches long; and although some slight variations occur, they are nevertheless not more than might have been anticipated within the ordinary limits of error.

3. The strength of tubes as affected by thickness.

In these experiments it is found that the tubes vary in strength according to a certain power in the thickness; the index of which, taken from the mean of the experiments, is 2·19, or rather higher than the square.

Combining the above laws into a general expression, we have, as the formula for the strength of tubes subjected to a uniform external force,

$$ P = C \times \frac{k^{2.19}}{L \times D} $$

where $P$ is the collapsing pressure, $k$ the thickness of the plates, $L$ the length of the tube, which should not be less than 1·5, or greater than 10 feet; $D$ the diameter, and $C$ a constant to be determined by the experiments. For tubes of greater length than those above specified, a variable quantity, dependent upon the length, must be introduced; and the value of this has yet to be determined. For ordinary practical calculations the following formula will probably, however, afford the needful accuracy:

$$ P = 806,300 \times \frac{k^{2}}{L \times D}. $$

Thus, for example, take a tube or boiler-flue 10 feet long, 2 feet diameter, and composed of plates 4 inch thick; and the collapsing pressure will be

$$ P = 806,300 \times \frac{25^2}{10 \times 24} = 210 \text{ lb.} $$

per square inch or nearly so.

Some experiments have also been made upon elliptical tubes; and the results have been most conclusive as to the weakness of such
forms in resisting external pressure. No tubes in use for boilers should ever be made of that form.

With regard to cylindrical internal flues, the experiments indicate the necessity of an important modification of the ordinary mode of construction, in order to render them secure at the high pressures to which they are now almost constantly subjected. If we take a boiler of the ordinary construction, 30 feet long, 7 feet in diameter, and with one or more flues 3 feet diameter, it will be found that the outer shell or envelope is from three to three and a half times as strong in resisting an internal force as the cylindrical flues which have to resist the same external force. This being the case, it is evident that the excess of strength in those parts of the vessel subjected to tension, is actually of no use so long as the elements of weakness are present in the other parts subjected to compression.

To remedy these defects, it is proposed to rivet strong rings of angle iron at intervals along the flue—thus practically reducing its length, or in other words increasing its strength to a uniformity with that of the exterior shell. This alteration in the existing mode of construction is so simple, and yet so effective, that its adoption may be confidently recommended to the attention of all those interested in the construction of vessels so important to the success of our manufacturing system, and yet fraught with such potent elements of disaster when unscientifically constructed or improperly managed.

"On some Remarkable Relations which obtain among the Roots of the Four Squares into which a Number may be divided, as compared with the corresponding Roots of certain other Numbers." By the Rt. Hon. Sir Frederick Pollock, F.R.S., Lord Chief Baron.

The first property of numbers mentioned in this paper is best illustrated by an example—

$$13^2 = 169 \quad 15^2 = 225.$$  

These odd numbers may be divided into 4 squares, and the roots may be so arranged that they will have this relation to each other: the middle roots will be the same, and the exterior roots will be, the one 2 more, the other 2 less than the corresponding roots of the other. Putting the roots below the number and comparing them, the result is obvious.

$$\begin{array}{cc}
169 & 225 \\
0,3,4,12 & -2,3,4,14 \\
-2,4,7,10 & -3,4,7,12 \\
-4,5,8,8 & -5,5,8,10 \\
-6,4,9,6 & -8,4,9,8
\end{array}$$

Each of the numbers may be divided into 4 squares in 4 different ways with this result, that the two middle roots of each are the same; and as to the exterior roots they differ by 2, the one being 2 more the other 2 less than the corresponding roots of the other. So comparing $15^2$ with $17^2$:

$$\begin{array}{cc}
225 & 289 \\
4,3,10,10 & 6,3,10,12 \\
6,5,10,8 & 8,5,10,10
\end{array}$$
the result is the same; and it is true of all adjoining odd squares. The paper contains a Table of odd squares (up to $27^2$), compared in this manner with the odd square immediately before it and after it.

It is then shown that the same property continues when the 2 odd squares are increased by any the same even number—

$$
\begin{array}{c|c}
49 & 81 \\
0,2,3,6 & -2,2,3,8 \\
51 & 83 \\
-1,3,4,5 & -3,3,4,7
\end{array}
$$

and also when they are (within certain limits) diminished by the same even number. It is then shown that a similar property belongs to the even squares $+1$, as seen below,

$$
16 + 1 = 17 \\
+1,0,0,4 \\
0,2,2,3 \\
37 \\
-1,2,4,4
$$

$$
36 + 1 = 37 \\
+1,0,0,6 \\
0,2,2,5 \\
65 \\
-3,2,4,6
$$

and also to these numbers increased or decreased by the same even number.

If, instead of comparing the adjoining squares, the alternate squares be compared, a similar result is obtained; the middle roots are the same, the exterior roots differ by 4 instead of 2.

The proof of this property depends upon a general property of all odd numbers and upon a general theorem.

The property of odd numbers is this, that every odd number can be divided into 4 squares in such manner that 2 of the roots will be equal, 2 will differ by 1, 2 will differ by 2, &c. as far as the number is capable (from its magnitude) of having roots large enough to form the difference required: thus in the No. 39 there cannot be roots having a difference of 9; for the least number that can have that difference is $41 = 4^2 + 5^2$ and $-4$ and 5 differ by 9; but $39 = 1^2 + 2^2 + 3^2 + 5^2$, and the difference between $-3$ and 5 is 8; and the numbers 1, 2, 3, 5, either as positive or negative, give all the differences up to 8, but they do not give 2 equal roots: 39 is however divisible into $1^2 + 1^2 + 1^2 + 6^2$, and then the equal roots are discovered. It is proved from the known properties of numbers that this property of having 2 roots whose difference will be 0, 1, 2, 3, &c., as far as is possible, belongs to all odd numbers. A new symbol is then suggested to represent the division of a number into 4 squares, such that 2 of the roots will have a given difference, and these are made the exterior roots; the number or figure denoting the difference is placed above on the left hand: thus $4^{25}$ denotes 0, 0, 3, 4 or $-2, 1, 4, 2$; $^{125}$ denotes 1, 2, 4, 2.

The general theorem is this:—If any odd number of odd numbers be in arithmetical progression (4 being the common difference), as 9, 13, 17, 21, 25, then if the common difference be assumed as the index of the difference of roots to the middle term, and the higher terms in the series have as indices $(4+1), (4+2), &c.$ in succession,
Baron Pollock on some Properties of Numbers.

and the lower terms have as indices \((4-1), (4-2), \&c.\), the series with its indices will be

\[
\begin{array}{cccccc}
2 & 3 & 4 & 5 & 6 \\
9 & 13 & 17 & 21 & 25 \\
\end{array}
\]

and if the terms less than the middle term be divided into 4 squares with exterior roots having the differences indicated by their respective indices thus,

\[
\begin{array}{cccccc}
2 & 3 & 4 \\
9 & 13 & 17 \\
0,1,2,2 & -1,2,2 & -2,0,3,2 \\
\end{array}
\]

then the terms greater than the middle term will have this relation to the terms less than the middle term; the terms equidistant from the middle term will have their middle roots the same, and the differences of the exterior roots will increase; those nearest the middle term will have a difference of 1, the next 2, and so on, thus:

\[
\begin{array}{cccccc}
2 & 3 & 4 & 5 & 6 \\
9 & 13 & 17 & 21 & 25 \\
0,1,2,2 & -1,2,2 & -2,0,3,2 & -2,2,2,3 & -2,1,2,4 \\
\end{array}
\]

An algebraic proof is then given as to a series whose middle term is \(n\) and common difference \(p\); and as \(n\) may be odd or even, and \(p\) also, and the index of differences may be \(\text{minus}\) as well as \(\text{plus}\), the theorem applies frequently to even numbers, but not universally. The following example is given of the theorem applied to 17 terms of a series whose first term is 25, and common difference 1:

\[
\begin{array}{cccccccccccccccccc}
-7 & -6 & -5 & -4 & -3 & -2 & -1 & 0 & \\
25 & 26 & 27 & 28 & 29 & 30 & 31 & 32 \\
4,0,0,-3 & 5,0,0,-1 & 5,1,1,0 & 2,2,4,-2 & 5,0,0,2 & 3,2,4,1 & 3,3,3,2 & 0,4,4,0 & 1 \\
& & & & & & & & \\
9 & 8 & 7 & 6 & 5 & 4 & 3 & 2 & 0,4,4,1 \\
41 & 40 & 39 & 38 & 37 & 36 & 35 & 34 \\
-4,0,0,5 & -2,0,0,6 & -1,1,1,6 & -3,2,4,3 & 1,0,0,6 & 0,2,4,1 & 1,3,3,4 & -1,4,4,1 \\
\end{array}
\]

Comparing the terms above with the terms below, it is manifest the terms of the series are divisible into 4 squares whose roots conform to the law of the theorem. It is then shown that the odd squares, and also all the numbers mentioned in the beginning of the paper, can be made terms in an arithmetic series, and will therefore have the property stated. It is then suggested that the properties of numbers stated in the paper may have been in some form a portion of the mysterious properties of numbers by which Fermat announced he could prove his celebrated theorem of the polygonal numbers.

A Postscript was added, dated 20th May, which is here given entire.

Since this paper was sent to the Society, some other theorems of a similar kind have occurred to me, in which the terms of a series
(not arithmetical of the 1st order) have a similar relation with regard to the roots of the 4 squares of which they may be composed, that is, those which are equidistant from the middle, or the middle term (according as the number of terms is even or odd), have the middle roots the same, and the exterior roots have an arithmetical relation to each other (varying with the distance from the centre), viz. the one being less and the other greater by the same quantity.

Thus, if any number of terms (exceeding 3) of either of the 2 series above-mentioned (viz. 1, 3, 9, 19, &c., or 1, 5, 13, 25, &c.), and, beginning with the first term, the differences be added "inverso ordine," a new series will be obtained possessing the property in question; thus the first 7 terms of the 1st series are, 1, 3, 9, 19, 33, 51, 73; the differences are 2, 6, 10, 14, 18, 22; if the differences be added "inverso ordine," the series becomes 1, 23, 41, 55, 65, 71, 73, each term of which may be divided into 4 squares, whose roots will be as follows:

<table>
<thead>
<tr>
<th></th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>0</td>
<td>2</td>
<td>23</td>
<td>41</td>
<td>55</td>
<td>65</td>
<td>71</td>
<td>73</td>
</tr>
<tr>
<td>0,0,1,0</td>
<td>+1,2,3,3</td>
<td>0,3,4,4</td>
<td>+3,1,6,3</td>
<td>-2,3,4,6</td>
<td>-3,2,3,7</td>
<td>-6,0,1,6</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>+2,0,1,6</td>
<td>+1,2,5,5</td>
<td>0,0,1,8</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Here there is a middle term, all the terms equidistant from it have the same middle roots, the terms next to the middle term have the exterior roots, the one 2 less, the other 2 more, those next but one 4 less and 4 more, and the extreme terms 1 and 73 have their exterior roots one 6 less and the other 6 more than the corresponding roots of the other.

If 8 terms of the series be taken as 1, 3, 9, 19, 33, 51, 73, 99, and the differences be added "inverso ordine," the series becomes 0, 2, 4, 6, 1, 27, 49, 67, &c., the terms of which divided into 4 squares, so that the differences of the exterior roots may correspond with the index, will be

<table>
<thead>
<tr>
<th></th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>10</th>
<th>12</th>
<th>14</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>0</td>
<td>2</td>
<td>27</td>
<td>49</td>
<td>67</td>
<td>81</td>
<td>91</td>
<td>97</td>
<td>99</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>0,0,1,0</td>
<td>-1,3,4,1</td>
<td>-2,4,5,2</td>
<td>-3,0,7,3</td>
<td>-4,0,7,4</td>
<td>-5,4,5,5</td>
<td>-6,3,4,6</td>
<td>-7,0,1,7</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>+2,0,3,6</td>
<td>-1,4,5,5</td>
<td>-2,4,5,6</td>
<td>-1,0,3,9</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>+1,1,4,7</td>
<td>0,3,4,8</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Here there is no middle term; the terms equidistant from the centre have the same middle roots, while the differences between the exterior roots increase as the numbers 1, 3, 5, 7.

The other series, 1, 5, 13, 25, &c., gives a similar result. If the differences of the first 7 terms be added "inverso ordine," the new series, with its indices and the roots of the 4 squares which compose each term, will be as follows:

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>11</th>
<th>13</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>3</td>
<td>5</td>
<td>7</td>
<td>9</td>
<td>11</td>
<td>13</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>25</td>
<td>45</td>
<td>61</td>
<td>73</td>
<td>81</td>
<td>85</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>0,0,0,1</td>
<td>0,9,4,3</td>
<td>-2,4,4,3</td>
<td>-3,0,6,4</td>
<td>-4,4,4,5</td>
<td>-4,0,4,7</td>
<td>-6,0,0,7</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>+1,2,4,2</td>
<td>0,2,4,5</td>
<td>-2,4,4,5</td>
<td>-2,2,4,7</td>
<td>-5,2,4,6</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>+1,2,2,6</td>
<td>-1,2,2,8</td>
</tr>
</tbody>
</table>
Baron Pollock on some Properties of Numbers. 541

Here there is a middle term; the equidistant terms have the same middle roots, the exterior roots are (next to the middle term) the one 2 more, the other 2 less, and the differences increase by 2 as the terms are more distant from the middle term.

If the number of terms be 8, the resulting series with its indices and roots will be—

1  29  53  73  89  101 109 113
0,0,0,1 0,2,4,3 -2,2,6,3 -1,0,6,6 -2,0,6,7 -5,2,6,6 -5,2,4,8 -7,0,0,8
+2,0,0,5 -1,0,6,4 +1,2,2,8 9,2,3,9 -4,0,6,7 -3,0,0,10 -2,0,4,9
+1,0,4,6
+2,0,0,7

and the differences of the exterior roots will be 1, 3, 5, 7. The reason of these results is, that the equidistant terms are always equal to the original corresponding term in the series increased by the same number.

Thus, in the first example, if to the terms

1,  3,  9, 19, 33, 51, 73 there be added
0, 20, 32, 36, 32, 20, 0; the result is
1, 23, 41, 55, 65, 71, 73, which is the series with the differences added "inverso ordine." And in the last example, if to

1,  5, 13, 25, 41, 61, 85, 113 there be added
0, 24, 40, 48, 40, 24, 0, the result is
1, 29, 53, 73, 89, 101, 109, 113, that is, the series arising from the differences being added "inverso ordine."

It is worthy of observation that these numbers, 0, 24, 40, 48, 40, 24, 0, which, added to the first 8 terms, produce a series identical with the result of the differences being added "inverso ordine," have the same effect upon any other consecutive 8 terms of the series. Take the 2nd term as the 1st of 8 terms—

5, 13, 25, 41, 61, 85, 113, 145, to these add
8  12  16  20  24  28  32
0  24  40  48  48  40  24  0, the result is
5, 37, 65, 89, 109, 125, 137, 145,
32  28  24  20  16  12  8

in which last series the differences are reversed or added "inverso ordine." The appropriate roots of these numbers are—

3  5  7  9  11  13  15  17
5 37  65  89  109 125 137 145
-1,0,0,2 -1,2,4,4 -3,2,6,4 -2,0,6,7 -3,0,6,8 -6,2,6,7 -6,2,4,9 -8,0,0,9
+1,0,0,6 -2,0,6,5 0,2,2,9 -1,2,2,10 -5,0,6,8 -4,0,0,11 -3,0,4,10
0,0,4,7
+1,0,0,8

which may be immediately obtained from the former series, the middle roots being the same; and the exterior roots, one of them one less, the other one more. In this way any consecutive 8 terms, with the differences reversed, may be each divided into 4 squares throughout the whole series.
And the same is true of 4 terms, 5 terms, or any number of terms. If 3 terms have the differences reversed, the numbers added are—

<table>
<thead>
<tr>
<th>If 4 terms</th>
<th>0 8 8 0</th>
</tr>
</thead>
<tbody>
<tr>
<td>If 5 terms</td>
<td>0 12 16 12 0</td>
</tr>
<tr>
<td>If 6 terms</td>
<td>0 16 24 24 16 0</td>
</tr>
<tr>
<td>If 7 terms</td>
<td>0 20 32 36 32 20 0</td>
</tr>
</tbody>
</table>
&c.

The law under which these numbers are formed is obvious enough. The same numbers exactly are to be added to the other series (1, 3, 9, 19, &c.) to produce the same result.

If the 2 series be blended together, thus 1 1 3 5 9 13 19 25, &c., the differences will be 2, 2, 4, 4, 6, 6, 8, 8, &c.; and if an odd number of terms be taken (so as to begin and end with a number from the same series), and the differences be added *inverso ordine*, a similar result occurs. Take 11 terms.

1 3 5 9 13 19 25 33 41 51 61, and add the differences "*inverso ordine,*" the series becomes, with its indices and roots,—

<table>
<thead>
<tr>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
</tr>
</thead>
<tbody>
<tr>
<td>11</td>
<td>10</td>
<td>9</td>
<td>8</td>
<td>7</td>
</tr>
<tr>
<td>61</td>
<td>59</td>
<td>57</td>
<td>53</td>
<td>49</td>
</tr>
<tr>
<td>-5,0,0,6</td>
<td>-3,0,1,7</td>
<td>-4,0,4,5</td>
<td>-2,2,3,6</td>
<td>0,0,0,7</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>10</th>
</tr>
</thead>
<tbody>
<tr>
<td>11</td>
<td>19</td>
<td>23</td>
<td>29</td>
<td>35</td>
<td>43</td>
<td>51</td>
<td>61</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0,0,3,1</td>
<td>-1,0,3,3</td>
<td>-2,0,3,4</td>
<td>-3,0,3,5</td>
<td>-4,0,3,6</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>21</td>
<td>23</td>
<td>25</td>
<td>29</td>
<td>33</td>
<td>39</td>
<td>45</td>
<td>53</td>
<td>61</td>
<td></td>
</tr>
<tr>
<td>-4,2,4,5</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

In this case the additions are 0, 8, 16, 20, 24, 24, 20, 16, 8, 0; and if these be added to any other consecutive 11 terms (the 1st term having an *odd* index), they produce the same effect as if the differences were reversed; and the resulting numbers have the property of the terms equidistant from the centre, being connected by their roots, having the relation so frequently mentioned. It may be further remarked, that the numbers produced by reversing the differences are the initial numbers from which, by adding 2, 2, 4, 4, &c., 61 may be formed of the squares, which make the differences of its exterior roots 10, 9, 8, &c.

<table>
<thead>
<tr>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>10</th>
</tr>
</thead>
<tbody>
<tr>
<td>11</td>
<td>15</td>
<td>19</td>
<td>23</td>
<td>29</td>
<td>35</td>
<td>43</td>
<td>51</td>
<td>61</td>
<td></td>
</tr>
<tr>
<td>0,0,3,2</td>
<td>-1,0,3,3</td>
<td>-2,0,3,4</td>
<td>-3,0,3,5</td>
<td>-4,0,3,6</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>21</td>
<td>23</td>
<td>25</td>
<td>29</td>
<td>33</td>
<td>39</td>
<td>45</td>
<td>53</td>
<td>61</td>
<td></td>
</tr>
<tr>
<td>0,2,4,1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

and so of all the others.

The matter referred to in this Postscript tends to strengthen the suggestion already made, that the properties of numbers referred to are connected with the mysterious and abstruse properties to which Fermat referred as enabling him to prove the theorem he announced of the Polygonal Numbers.
The following communication was read:

"On the Geological Structure of the North of Scotland and the Orkney and Shetland Islands." Part II. By Sir R. I. Murchison, F.R.S., V.P.G.S.

In a paper read during the last Session (see "Abstracts," No. 10) the author described the general succession of rocks in the Northern Highlands, as observed by Mr. Peach and himself, aided by the researches of some other geologists.

The rocks were described in their ascending order, as, first, a fundamental gneiss traversed by granite-veins at Cape Wrath; secondly, a red or chocolate-coloured sandstone and conglomerate, of great thickness, and regarded by the author as of Cambrian age; thirdly, succeeding unconformably, is a series of quartzite, with intercalated limestone, both of them often highly crystalline,—from the limestone Mr. C. Peach had succeeded in obtaining, "near Durness," several fossils, shown to be of Lower Silurian age; fourthly, micaceous schists and flagstones occupying a wide extent of country to the east of Loch Eriboll, described as being of younger age than the foregoing, and older than the Old Red Sandstone series which occupies the North-eastern Highlands and a great portion of the eastern coast of Scotland; fifthly, the Old Red series, arranged by the author into three divisions, the middle being the Caithness Flags.

In the past autumn Sir Roderick, feeling that several points required stricter examination, revisited the country already described, extending his researches both east and west, and to the most northernly point of the Shetlands.

In this tour he not only confirmed his views previously announced with regard to the succession of the older rocks, but examined the structure of the Orkneys and Shetlands, more clearly defining the relations and physical characters of the beds there composing the Old Red Series.

The present memoir comprised the details of these later observations; and Sir Roderick acknowledged the aid he had derived from Mr. Peach (who accompanied him throughout the journey), Mr. John Miller, Rev. Mr. Gordon, and others; and he referred to the previous memoirs of Mr. Cunningham and Hugh Miller on Sutherland, &c., and Dr. Hibbert on the Shetland Islands.

The principle points dwelt upon in this paper were—

1. The evidence obtained at various points, that the Lower Silurian limestone is intercalated in quartz-rock (east of Loch Eriboll, Assynt, &c.).

2. That the Durness limestone lies in a basin supported by quartz rock on the east as well as on the west.

3. That certain igneous rocks, connected with the Durness trough, are protruded near Smo, which had not before been noticed.

4. On this occasion corroborative evidence was adduced of the
conformable superposition of the micaceous schists or gneissose flagstones to the quartzite series,—the succession being visible at intervals in all the intermediate country between Loch Eriboll and Ledmore, and the passage upwards from the quartzites and their associated limestones into the schists and micaceous flags being both clear and persistent, with some local interruptions only of igneous rocks.

5. That the protrusion of porphyry, hypersthene, greenstone, &c. is not peculiar to any one line, but occurs in the purple or Cambrian sandstone, in the overlying Silurian limestone of Durness, and again in the still higher micaceous flagstones; and that the latter, when intruded upon by granite, much resemble the old gneiss.

6. With regard to the Old Red Series of the east coast, Sir Roderick pointed out the extension of the middle set of deposits, namely the Caithness flags,—their great thickness in Caithness compared with their development in the south,—and their range over the Orkneys into the Shetlands, where they also thin out, putting on a somewhat different lithological character, and where the Old Red Series is chiefly represented by sandstones, the upper part containing plants. He dwelt upon the great value of the Caithness Flags as paving-stones, their extraordinary durability being due to a certain admixture of lime and bitumen (the latter derived from fossil fishes) with silica and alumina, whilst in some parts they contain bitumen enough to render them of economic value. The author next pointed out the passage of the Caithness flags upwards into light-coloured sandstones, which eventually form the great headlands of Dunnet and Hoy, where such overlying sandstones cannot be of less thickness than 1200 to 1500 feet.

With regard to the micaceous rocks of the North-east of Scotland and the Shetland Isles, they are, according to the author, portions of the series which is younger than the fossiliferous Lower Silurian rocks of the west of Sutherland,—the so-called gneiss of the Sutors of Cromarty belonging, in Sir Roderick’s opinion, to the micaceous-flag series of Eastern Ross-shire; and the gneissic rock extending southwards to Flowerburn, Kinordy and Rosemarkie, near Fortrose, is regarded by him as a member of that series, altered by the intrusion of granitic and felspathic rocks.

The paper was illustrated by a large series of rocks and fossils collected during the author’s last tour, and by geological maps and coloured views and sections.

LXI. Intelligence and Miscellaneous Articles.

ON SOME PROPERTIES OF ICE NEAR ITS MELTING-POINT.

BY PROFESSOR FORBES.

During the last month of March, I made some experiments on the properties of ice near its melting-point, with particular reference to those of Mr. Faraday, published in the ‘Athenæum’ and ‘Literary Gazette’ for June 1850, to which attention has been more lately called by Dr. Tyndall and Mr. Huxley in relation to the phenomena of glaciers.
Owing to indisposition, I have been obliged to leave my experiments for the present incomplete. But I am desirous, before the session of the Royal Society closes, to place on record some facts which I have observed, and also some conclusions which I deduce from these and other recent experiments and discussions.

Mr. Faraday's chief fact, to which the term "regelation" has been more lately applied, is this, that pieces of ice, in a medium above 32°, when closely applied, freeze together; and flannel adheres apparently by congealing to ice under the same circumstances.

1. These observations I have confirmed. But I have also found that metals become frozen to ice when they are surrounded by it, or when they are otherwise prevented from transmitting heat too abundantly. Thus, a pile of shillings being laid on a piece of ice in a warm room, the lowest shilling, after becoming sunk in the ice, was found firmly attached to it.

2. Mere contact, without pressure, is sufficient to produce these effects. Two slabs of ice, having their corresponding surfaces ground tolerably flat, were suspended in an inhabited room upon a horizontal glass rod passing through two holes in the plates of ice, so that the plane of the plates was vertical. Contact of the even surfaces was obtained by means of two very weak pieces of watch-spring. In an hour and a half the cohesion was so complete, that, when violently broken in pieces, many portions of the plates (which had each a surface of twenty or more square inches) continued united. In fact, it appeared as complete as in another experiment where similar surfaces were pressed together by weights. I conclude that the effect of pressure in assisting "regelation," is principally or solely due to the larger surfaces of contact obtained by the moulding of the surfaces to one another.

3. Masses of strong ice, which had already for a long time been floating in unfrozen water-casks, or kept for days in a thawing state, being rapidly pounded, showed a temperature 0°-3 Fahrenheit below the true freezing-point, indicated by delicate thermometers (both of mercury and alcohol), carefully tested by long immersions in a considerable mass of pounded ice or snow in a thawing state.

4. Water being carefully frozen into a cylinder several inches long, with the bulb of a thermometer in its axis, and the cylinder being then gradually thawed, or allowed to lie for a considerable time in pounded ice at a thawing temperature, showed also a temperature decidedly inferior to 32°, not less, I think, than 0°-15 Fahr.

I think that the preceding results are all explicable on the one admission, that Person's view of the gradual liquefaction of ice is correct (Comptes Rendus, 1850, vol. xxx. p. 526*), or that ice gradually absorbs latent heat from a point very sensibly lower than the zero of the Centigrade scale.

I. This explains the permanent lower temperature of the interior of ice.

Let AB be the surface of a block of ice contained in water at what is called a freezing temperature. That temperature is marked by the level of the line QP above some arbitrary zero. LM is, in

* Quoted by me in 1851, in my sixteenth letter on Glaciers.
like manner, the permanent but somewhat lower temperature possessed by the interior of the ice. The space, partly water, partly ice, or partaking of the nature of each, MNOP, has a temperature which varies from point to point, the portion NO corresponding to what may be called the physical surface of the ice between A'B' and ab, which is "plastic ice," or "viscid water," having the most rapid variation of local temperature.

II. Such a state of temperature, though it is in one sense permanent, is so by compensation of effects. Bodies of different temperatures cannot continue so without interaction. The water must give off heat to the ice, but it spends it in an insignificant thaw at the surface, which therefore wastes, even though the water be what is called ice-cold, or having the temperature of a body of water enclosed in a cavity of ice*.

This waste has yet to be proved; but I have little doubt of it; and it is confirmed by the wasting action of superficial streams on the ice of glaciers, though other circumstances may also contribute to this effect.

III. The theory explains "regelation." For let a second plane surface of ice, A'B', be brought up to nearly physical contact with the first surface, AB. There is a double film of "viscid water" isolated between two ice surfaces colder than itself. The former equilibrium is now destroyed. The films ABba and A'B'b'a' were kept in a liquid or semiliquid state by the heat communicated to them by the perfect water beyond. That is now removed, and the film in question has ice colder than itself on both sides. Part of the sensible heat it possesses is given to the neighbouring strata which have less heat than itself, and the intercepted film of water in the transition state becomes more or less perfect ice.

Even if the second surface be not of ice, provided it be a bad conductor, the effect is practically the same. For the film of water is robbed of its heat on one hand by the colder ice, and the other badly conducting surface cannot afford warmth enough to keep the water liquid.

This effect is well seen by the instant freezing of a piece of ice to a worsted glove, even when on a warm hand. But metals may act so, provided they are prevented from conveying heat by surrounding them with ice. Thus, as has been shown, metals adhere to melting ice.


* I incline to think that water, in these circumstances, may, though surrounded by ice, have a fixed temperature somewhat higher than what is called 32°. But I have not yet had an opportunity of verifying the conjecture.

My idea is, that the invasion of cold from the surrounding ice is spent in producing a very gradual "regelation" in the water which touches the ice, leaving the interior water in possession of its full dose of latent heat, and also of a temperature which may slightly exceed 32°. By similar reasoning, a small body of ice, enclosed in a large mass of water, will preserve its proper internal temperature below 32°; but, instead of regelation taking place, the surface is being gradually thawed. This is the case contemplated in the paragraph of the text to which this note refers.
INDEX TO VOL. XVI.

ABICH (Dr. H.) on some points in the history and formation of Etna, 395.
Acetamide, on some compounds of, 135, 136.
Acetones, on the, 363.
Ethylodibromallylammonia, 261.
Airy (G. B.) on the mechanical conditions of the deposit of a submarine cable, 1; on the substitution of methods founded on ordinary geometry for methods based on the general doctrine of proportions, 231.
Alcoholic fermentation, on the theory of, 367.
Aldehydes, on the, 363; on the action of pentachloride of phosphorus on, 366.
Alison (Dr. S. S.) on the differential stethophone, 385.
Alkalies and alkaline earths, on a photochemical method of recognizing the non-volatile, 328.
Alloys, on the heat-conducting power of, 381; analysis of Chinese, 420.
Allyle, on the action of ammonia on the terbromide of, 257.
Amyle-glycol, on the preparation and properties of, 297.
Anchoic acid, on, 361.
Aneroid barometer, on the practical use of, as an orometer, 304.
Aniline, on the action of chloroform on, 532.
Antimony, on the properties of electro-deposited, 441.
Atkinson (Dr. C.), chemical notices from foreign journals by, 135, 209, 295, 361, 516; on monoacetate of glycol, and on the preparation of glycol, 433.
Atlantic cable, on the deposit of the, 1, 34.
Atmosphere, on the influence of heated terrestrial surfaces in disturbing the, 531.

Atomic weights of the elements, on the, 209, 216.
Atropine, on crystallized valerianate of, 480.
Barium, on the equivalent of, 216.
Baral (M.) on nicotic acid, 361.
Baxter (H. F.) on organic polarity, 318.
Bergemann (Prof.) on the native phosphates of copper, 239.
Bertin (A.) on the electro-magnetic rotation of liquids, 319.
Bigsby (J. J.) on the palæozoic fossils of the State of New York, 234.
Binney (E. W.) on the structure of Stignaria ficoides, 73.
Blance (F. Le) on the gaseous emanations which accompany the boracic acid in the lagoons of Tuscany, 284.
Bodies, on equally attracting, 161, 266.
Boracic acid lagoons of Tuscany, on the gaseous emanations from the, 284.
Bridge (J.) on the diffraction of light, 321.
Broun (J. A.) on certain results of magnetic observations, 81.
Buekton (G. B.) on anchoic acid, 361.
Cadmium, on the equivalent of, 218.
Callmann (H.) on crystallized valerianate of atropine, 480.
Calvert (F. C.) on the relative power of metals and their alloys to conduct heat, 381.
Cambridge Philosophical Society, proceedings of the, 69, 150, 229, 312.
Candy (F. J.) on a physiological alphabet, 150.
Cartwell (R.) on a photochemical method of recognizing the non-volatile alkalies and alkaline earths, 329.
Cayley (A.) on the theory of matrices, 223; on the surface which is the envelope of planes through the points of an ellipsoid at right an-
gles to the radii vectores from the centre, 383; on a theorem relating to hypergeometric series, 356.
Cellulose, on a solvent for, 523.
Chemical affinity, on the influence of pressure on, 160.
— combination, on a new theory of, 108.
— notices from foreign journals, 135, 209, 295, 361, 516.
Chlorine, on the compound nature of, 178.
Chloro-arsenious acid and compounds, on, 358.
Chinon alkaloids, researches on, the, 55.
Claudet (A.) on the stereomonomoscope, 462.
Clausius (Prof. R.) on the nature of ozone, 45; on the increase of the electrical resistance of the metals with the temperature, 400.
Cleavage, remarks on, 75.
Coal-measures, Lower Carboniferous, in British America, on the, 72.
Cobalt, on the separation of, from manganese, 197.
Combustion, on the alleged influence of solar light on, 182.
Comets, on the nature of, 96.
Conical refraction, on internal, 159.
Consonances, on the beats of imperfect, 158.
Conte (Prof. J. Le) on the alleged influence of solar light on the process of combustion, 182.
Copper, on native, 237; on the native phosphates of, 239.
Couper (A. S.) on a new chemical theory, 104.
Crotonic acid, on, 298.
Crystallized furnace-products, on some, 292, 480.
Curves of the third degree, on the algebraical theory of derivative points of, 116.
Dawson (J. W.) on the Lower Carboniferous coal-measures of British America, 72.
Debus (Dr. II.) on the action of caustic potash on glycerate of potash, 438.
Dessaignes (M.) on malonic acid, 362.
Deville (C. St.-Clair), on the gaseous emanations which accompany the boracic acid in the lagoons of Tuscany, 284; on the disassociation of compound bodies, 516.
Dibromallylammonia, on the formation of, 260.
Dolomites near Cork, on, the, 235.
Donaldson (Dr.) on the state of Solon mentioned by Eschines and Demosthenes, 317.
Dumas (M.) on the equivalents, 209.
Earthquake, notice of the occurrence of an, along the northern edge of the Dartmoor district, on September 28, 1858, 473.
Electric discharge in vacuo, on the strike seen in the, 18; on the action of the magnet upon, in rarefied gases, 119, 408; on the stratifications and dark bands in, 305.
— spark, observations on the, 77, 503.
— light currents, on the spectra of the, 414.
— resistance of the metals, on the increase of the, with the temperature, 400.
Electricity, on the development of, between metals and heated salts, 101.
Enodyle, on the formation and constitution of, 381.
Equations, dynamical, new methods of treating, 466.
Equilibrium-figures of a liquid mass without weight, experimental and theoretical researches on the, 23.
Equivalents, on the doctrine of, 37, 209.
Etna, on some points in the history and formation of, 395.
Euphodites of Monte Rosa, on the, 240.
Fabbri (R.), microscopic observations on the electric spark, 77.
Fairbairn (W.) on the resistance of tubes to collapse, 534.
Faraday (Prof.) on the irregular fusibility of ice, 354.
Feddersen (B. W.) on the electric spark, 503.
Felspars, on the origin of, 78.
Fern, on a new fossil, from Worcestershire, 74.
Fisher (Rev. O.) on some natural pits on the heaths of Dorsetshire, 473.
Fluids, on the theory of internal re-
INDEX.

sistance and internal friction in, 528.
Fluorescence, on intermittent, 399.
Forbes (Prof.) on some properties of ice near its melting-point, 544.
Fraxine, on the fluorescence of a solution of, 159.
Friedel (M.) on the aldehydes and acetones, 363.
Gases, on the measurement of, in analysis, 524.
Gassiot (J. P.) on the stratifications and dark bands in electrical discharges, 305.
Genth's (Dr. F. A.), contributions to metallurgy, 420.
Geological Society, proceedings of the, 72, 233, 395, 473, 543.
Geometrical problems, on the treatment of some, 231.
Geometry of the elliptic equation, on the, 198.
Geuther (Dr.) on glycol, 297; on the action of pentachloride of phosphorus on aldehyde, 366.
Glacial phenomena, on some of the of Canada and of the north-eastern provinces of the United States during the drift-period, 74.
Gladstone (Dr. J. H.) on the chemical action of water on soluble salts, 66.
Glycerate of potash, on the action of caustic potash on, 438.
Glycol, on, 297; on the preparation of, 433; on monoacetate of, 434.
Glycolic acid, on the formation of, from acetic acid, 138.
Godwin-Austen (Lieut. H. H.) on the lacustrine or Karéwah deposits of Kashmir, 395.
Gore (G.) on the properties of electro-deposited antimony, 441.
Gravatt (W.) on the Atlantic cable, 34.
Groschans (J. A.) on the relations between the tensions and temperatures of vapours, 398.
Grove (W. R.) on the stric seen in the electrical discharge in vacuo, 18; on the influence of light on the polarized electrode, 426.
Haidinger's brushes, on the theory of, 69.
Hankel (W. G.) on the development of electricity between metals and heated salts, 101.
Hargreave (Dr. C. J.) on the problem of three bodies, 466.
Harkness (Prof.) on jointings, and on the dolomites near Cork, 235.
Hauer (M. von) on the equivalents of cadmium and manganese, 218.
Haughton (Rev. Prof. S.) on the physical structure of the old red sandstone of the county of Waterford, 224; on the black mica of the granite of Leinster and Donegal, 396.
Hawkes (W.) on an experiment in melting and cooling some of the Rowley rag, 236.
Heat, on the distribution of, over islands, 240; on the relative power of metals and their alloys to conduct, 381.
Hennessy (Prof. H.) on the distribution of heat over islands, and especially over the British Isles, 241.
Henry (T. H.) on the separation of nickel and cobalt from manganese, 197.
Herapath (Dr. W. B.) on the cinchona alkaloids, 55.
Hirst (Dr. T. A.) on equally attracting bodies, 161, 266.
Hofmann (Dr. A. W.), on the poly-ammonias, 309.
Hopkins (T.) on the influence of heated terrestrial surfaces in disturbing the atmosphere, 531.
Horner (L.) on the alluvial land of Egypt, 225.
Hunt (T. S.) on the origin of felspars, and on some points of chemical lithology, 78; on the euphotides of Monte Rosa, 240; on the probable origin of some magnesian rocks, 376.
Hypergeometric series, on a theorem relating to, 356.
Ice, on the interior melting of, 303; on some physical properties of, 333, 544; on the stratification of vesicular, by pressure, 463.
Iron-ores of Exmoor, on the, 236.
Jago (J.) on the functions of the tympanum, 298.
Johnson (R.) on the relative power of metals and their alloys to conduct heat, 381.
Joule (J. P.) on the expansion of wood by heat, 54.
Kekulé (A.) on the formation of glycolic acid from acetic acid, 138; observations on Mr. Couper’s new chemical theory, 478.

Kirkman (Rev. T. P.) on the partitions of the r-pyramid, 54.

Kölliker (Prof. A.) on the poison of the Upas Antiar, 140; on the physiological action of the Tanghinia venenifera, 384.

Kopp (Prof.) on unusual vapour densities, 510.

Kündig (M.) on acetamide, 135.

Lamination and cleavage, on, 75.

Lead, on the equivalent of, 217.

Lech (Dr. P.) on internal conical refraction, 159.

Lepargyric acid, on, 361.

Light, on the diffraction of, 321; on the influence of, on the polarized electrode, 426.

Liquids, on the electro-magnetic rotation of, 319.

Logic in general, on, 313.

Magnesian rocks, on the probable origin of some, 376.

Magnetic declination, on hourly observations of, 51.

—— light, on, 408.

—— observations, remarks on some, 143; on certain results of, 81.

Malonic acid, on, 362.

Manganese, on the equivalent of, 219; on the silicuret of, 522.

Marignac (M.) on the equivalents of barium, strontium, and lead, 216.

Matrices, on the theory of, 223.

Matthiessen (Dr. A.) on the electro-conducting power of the metals, 219.

Meridian, Indian, on the figure of the, 401.

Merrifield (C. W.) on the geometry of the elliptic equation, 198.

Metallurgy, contributions to, 420.

Metals, on the electro-conducting power of the, 219; on the heat-conducting power of, 381; on the increase of the electrical resistance of the, with the temperature, 400.

Meteoric stone of Borkut, analysis of the, 479.

Meteors, on luminous, 500.

Meyer (Dr. L.) on the influence of pressure on chemical affinity, 160.

Mica, on the black, of the granite of Leinster and Donegal, 396.

Miller (Prof.) on the planimeters of Wettl, Decher, and Amsler, 230; on some crystallized furnace-products, 292.

Mineral springs, on some, at Teheran, Persia, 477.

Moon (R.) on the theory of internal resistance and internal friction in fluids, and on the theories of sound, 528.

Moorsom (Capt. W. S.) on the practical use of the aneroid barometer as an orometer, 304.

Morgan (Prof. De) on the beats of imperfect consonances, 158; on a proof of the existence of a root in every equation, 232; on the syllogism, No. III., and on logic in general, 313.

Morris (J.) on a new fossil fern from Worcestershire, 74.

Müller (J.) on intermittent fluorescence, 399.

Murchison (Sir R. I.) on the geological structure of the North of Scotland, 543.

Murray (Hon. C. A.) on some mineral springs at Teheran, Persia, 477.

Nickel, on the separation of, from manganese, 197.

Nicol (Prof. J.) on the slate-rock and trap-veins of Easdale and Oban, 238.

Nicot acid, on, 361.

Northcote (A. B.) on the constitution of thermophyllite, 263.

Numbers, on some properties of, 537.

Odling (Dr. W.) on the doctrine of equivalents, 37.

Ormerod (G. W.) on the occurrence of an earthquake along the northern edge of the granite of Dartmoor district, on September 28, 1858, 473; on veins of granite in the carbonaceous rocks on the north and east of Dartmoor, 474.

Oxygen, on the allotropic modifications of, 178.

Ozone, on the nature of, 45.

Paget (Dr. J.) on some remarkable defects in the voluntary muscles, 317.

Palaeozoic fossils of the State of New York, on, the, 234.
Partition, compound, on the general theory of, 371.
Partition of the \( \tau \)-pyramid, on, 54.
Pasteur (M.) on the alcoholic fermentation of sugar, 367.
Pettenkofer (M.) on the atomic weights of the elements, 216.
Planimeters, 230.
Plateau (M. J.) on the equilibrium figures of a liquid mass without weight, 23.
Pleistocene sea-bed of the Sussex coast, on, 233.
Plücker (M.) on the action of the magnet upon the electrical discharge in rarefied gases, 119, 408.
Polarity, on organic, 318.
Polarized fasciculi, on the theory of, 69.
Pollock (Sir F.) on some remarkable properties of numbers, 537.
Poly-ammonias, notes of researches on, the, 309, 532.
Potter (Prof.) on the properties of compound double-refracting rhombs, 419.
Power (Rev. J.) on the theory of polarized fasciculi, commonly known as Haidinger's brushes, 69.
Pratt (Archdeacon) on the figure of the Indian meridian, 401.
Prestwich (J.) on the Pleistocene sea-bed of the Sussex coast, 233.
Propyle-glycol, on, 295.
Pullen (Capt.) on deep-sea soundings, 458.
Ramsay (Prof. A. C.) on some of the glacial phenomena of Canada during the drift-period, 74.
Ranieri (M.) on volcanic formation of sal-ammoniac, 368.
Rankine (W. J. M.) on the resistance of ships, 238.
Rhombs, on the properties of compound double-refracting, 419.
Roots, on Cauchy's theorem of imaginary, 232.
Royal Society, proceedings of the, 51, 140, 219, 298, 376, 458, 524.
Rubidge (Dr. R. N.) on the geology of South Africa, 475.
Rue, on the constitution of the essential oil of, 380.
Russell (Dr. W. J.) on the measurement of gases in analysis, 524.
Sabine (Maj.-Gen. Ed.) on hourly observations of the magnetic declination, 51; on magnetic observations transmitted from York Fort, Hudson's Bay, 143.
Sal-ammoniac, on volcanic formation of, 368.
Salm-Horstmar (Prince) on the fluorescence of a solution of fraxine, 159.
Salmon (Rev. G.) on the double tangents to plane curves, 318.
Schlippe (M.) on crotonic acid, 298.
Schlossberger (Dr.) on a solvent for cellulose, 523.
Schönbein (Prof. C. F.) on the allotropic modifications of oxygen, and on the compound nature of chlorine, bromine, &c., 178.
Schweizer (Prof.) on a solvent for cellulose, 523.
Serope (G. P.) on lamination and cleavage, 75.
Secchi (A.) on solar spots, and the method of determining their depth, 495.
Sedgwick (Prof.) on dislocations of rocks between Leven and Duddon Sands, 155.
Ships, on the resistance of, 238.
Siliciuretted hydrogen, on inflammable, 520.
Simpson (Dr. M.) on a new base obtained by the action of ammonia on terbromide of allyle, 257.
Smyth (W.) on the iron ores of Exmoor, 236.
Solar light, on the alleged influence of, on the process of combustion, 182.
spots, observations on, 95, 100, 495; method of determining their depth, 495.
Solon, on the statue of, mentioned by Æschines and Demostenes, 317.
Sophista of Plato, on, 229.
Sound, on the theories of, 481, 528.
Soundings, on deep-sea, 458.
Städelier (Dr.) on the alleged production of urea from albumen, 139.
Stercomonomoscope, on the, 462.
Stethophone, on the differential, 385.
Stigmia ficoides, on the structure of, 73.
Stokes (Prof.) on the discontinuity of arbitrary constants which appear in divergent developments, 71.
INDEX.

Stow (C. W.) on some fossils from South Africa, 474.

Strecker (Prof.) on some compounds of acetamide, 136.

Striae seen in the electrical discharge in vacuo, on the, 18.

Strontium, on the equivalent of, 217.

Sugar, on the alcoholic fermentation of, 367.

Sylvester (Prof. J. J.) on the algebraical theory of derivative points of curves of the third degree, 117; on the problem of the Virgins, and the general theory of compound partition, 371; on the equation in numbers of the first degree, 369.

Tanghinia venenifera, on the physiological action of the, 384.

Terrestrial magnetism, on, 81.

Thermophyllite, on the constitution of, 263.

Thompson (Prof.) on the Sophista of Plato, 229.

Thomson (Prof. W.) on the interior melting of ice, 303; on the stratification of vesicular ice by pressure, 463.

Tympanum, on the functions of the, 298.

Tyndall (Prof. J.) on some physical properties of ice, 333.

Upas Antiar, on the poison of the, 140.

Urea, on the alleged production of, from albumen, 139.

Vapour densities, on unusual, 519.

Vapours, on the relations between the tensions and temperatures of, 398.

Vaughan (D.) on luminous meteors and temporary stars, 500.

Virgins, on the problem of the, 371.

Vivian (Capt. W.) on native copper in the Llandudno mine, North Wales, 237.

Voluntary muscles, on some remarkable defects in the, 317.

Wallace (Dr. W.) on chloro-arsenious acid, and some of its compounds, 358.

Water, on the chemical action of, on soluble salts, 66.

Waterston (J. J.) on the theory of sound, 481.

Williams (C. G.) on the constitution of the essential oil of rue, 380.

Williamson (Dr.) on the measurement of gases in analysis, 524.

Wirz (M.) on lepargylic acid, 361.

Wöhler (Prof.) on inflammable silicurretted hydrogen, 520; on silicuret of manganese, 522.

Wood, on the expansion of, by heat, 54.

Wurtz (M.) on propyle-glycol, 295.

END OF THE SIXTEENTH VOLUME.

PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET-STREET.