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

CONDUCTED BY
SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.
AND
WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster

VOL. XL.—FOURTH SERIES.
JULY—DECEMBER 1870.

LONDON.
TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;
SOLD BY LONGMANS, GREEN, READER, AND DYER; SIMPKIN, MARSHALL AND CO.;
WHITTAKER AND CO.; AND KENT AND CO., LONDON:—BY ADAM AND
CHARLES BLACK, AND THOMAS CLARK, EDINBURGH;
SMITH AND SON, GLASGOW; HODGES AND
SMITH, DUBLIN; AND PUTNAM,
NEW YORK.
"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . . . . Admiratio generat questionem, questio investigationem, investigatio inventionem."—Hugo de S. Victore.

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phoebus 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. XL.

(FOURTH SERIES.)

NUMBER CCLXIV.—JULY 1870.

<table>
<thead>
<tr>
<th>Author</th>
<th>Title</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mr. J. Ball</td>
<td>on the Cause of the Descent of Glaciers</td>
<td>1</td>
</tr>
<tr>
<td>Archdeacon Pratt</td>
<td>Reply to M. Delaunay’s objection to the late</td>
<td></td>
</tr>
<tr>
<td>Mr. Hopkins</td>
<td>Method of determining the Thickness of the Earth’s Crust by the Precession and Nutation of the Earth’s Axis</td>
<td>10</td>
</tr>
<tr>
<td>Prof. E. Edlund</td>
<td>on the Path of Electrical Induction- and Disjunction-Currents through Gases of various Densities, and between Poles of different shapes</td>
<td>14</td>
</tr>
<tr>
<td>Mr. A. S. Davis</td>
<td>on a possible Cause of the Bright Line observed by M. Angström in the Spectrum of the Aurora Borealis</td>
<td>33</td>
</tr>
<tr>
<td>Mr. R. Moon</td>
<td>on the Solution of Linear Partial Differential Equations of the Second Order involving two Independent Variables</td>
<td>35</td>
</tr>
<tr>
<td>Mr. S. E. Phillips</td>
<td>on a Simple Method of Constructing high Electrical Resistance</td>
<td>41</td>
</tr>
<tr>
<td>Prof. von Bezold</td>
<td>Researches on the Electrical Discharge</td>
<td>42</td>
</tr>
<tr>
<td>Proceedings of the Royal Society:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mr. Warren De La Rue, Dr. Balfour Stewart, and Mr. B. Loewy</td>
<td>on the Positions and Areas of the Spots observed at Kew during the years 1864–66, also the Spotted Area of the Sun’s visible disk from the commencement of 1832 up to May 1868</td>
<td>53</td>
</tr>
<tr>
<td>Mr. H. T. Brown</td>
<td>on the Estimation of Ammonia in Atmospheric Air</td>
<td>54</td>
</tr>
<tr>
<td>Mr. H. E. Roscoe and Dr. T. E. Thorpe</td>
<td>on the Relation between the Sun’s Altitude and the Chemical Intensity of Total Daylight in a Cloudless Sky</td>
<td>56</td>
</tr>
<tr>
<td>Prof. C. Piazzi Smyth</td>
<td>on Supra-annual Cycles of Temperature in the Earth’s Surface-crust</td>
<td>58</td>
</tr>
<tr>
<td>The Rev. S. Haughton</td>
<td>on the Constituent Minerals of the Granites of Scotland, as compared with those of Donegal</td>
<td>59</td>
</tr>
<tr>
<td>Mr. H. E. Roscoe</td>
<td>on Vanadium</td>
<td>62</td>
</tr>
<tr>
<td>Proceedings of the Geological Society:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Prof. Huxley</td>
<td>on Hypsilophodon, a new Genus of Dinosauria</td>
<td>68</td>
</tr>
<tr>
<td>Prof. Huxley</td>
<td>on the Affinity between the Dinosaurian Reptiles and Birds</td>
<td>69</td>
</tr>
<tr>
<td>Prof. Huxley</td>
<td>on the Dinosauria of the Trias</td>
<td>70</td>
</tr>
</tbody>
</table>
Dr. P. M. Duncan on the Physical Geography of Western Europe during the Mesozoic and Cainozoic Periods... 71
Mr. T. Davidson on the Brachiopoda hitherto obtained from the Pebble-bed at Budleigh-Salterton 71
Mr. S. V. Wood, Jun., on the Relation of the Boulder-clay without Chalk of the North of England to the Great Chalky Boulder-clay of the South 72
Mr. R. Tate and Dr. J. S. Holden on the Iron-ores associated with the Basalts of the North-east of Ireland 73
Principal Dawson on the Structure of Sigillaria 74
Principal Dawson on some new Animal Remains from the Carboniferous and Devonian of Canada 75
Mr. J. W. Hulke on a Crocodilian Skull from Kimmeridge Bay, Dorset 75
Mr. J. W. Hulke on some Teeth associated with two fragments of a Jaw from Kimmeridge Bay 76
Experiments on the Velocity of the Propagation of Sound in Water in a Cast-iron Conduit 8 decimetres in diameter, by M. Fr. André 76
Experimental Researches on the Duration of the Electric Spark, by MM. Lucas and Cazin 78

NUMBER CCLXV.—AUGUST.

M. Achille Cazin on Internal Work in Gases. (With a Plate.) 81
Mr. W. M. Watts on the Spectra of Carbon 100
Dr. W. J. M. Rankine on Thermodynamics 103
M. W. Wernicke on the Refractive Indices and the Dispersion of Opaque Bodies 105
Mr. G. M. Seabroke on the Determination whether the Corona is a Solar or Terrestrial Phenomenon 117
Prof. R. Clausius on a Mechanical Theorem applicable to Heat 122
Proceedings of the Royal Society:—
Sir C. Wheatstone on a Cause of Error in Electroscopic Experiments 128
Mr. W. H. Barlow on the Cause and Theoretic Value of the Resistance of Flexure in Beams 130
Staff-Commander J. E. Davis on Deep-sea Thermometers 132
Dr. E. J. Mills on the Chemical Activity of Nitrates 134
Proceedings of the Geological Society:—
Mr. R. Etheridge on the Geological Position and Geographical Distribution of the Reptilian or Dolomitic Conglomerate of the Bristol Area 136
Mr. T. G. B. Lloyd on the Superficial Deposits of portions of the Avon and Severn Valleys 137
Mr. J. Prestwick on the Crag of Norfolk and associated Beds 137
Dr. P. M. Duncan on the Fossil Corals (Madreporaria) of the Australian Tertiary Deposits 139
Mr. J. W. Hulke on a new and undescribed Wealden Vertebra 140
CONTENTS OF VOL. XL.—FOURTH SERIES.

Mr. R. Tate on the Middle Lias in the North-east of Ireland. 140
Mr. J. W. Judd on the Neocomian Strata in Yorkshire and Lincolnshire ................................................. 140
On the use of the Electric Current in Calorimetry, by M. J. Jamin .......................................................................... 142
On the Fixed Notes characteristic of the various Vowels, by M. R. Koenig ................................................................. 145
On the Compressibility of Gases under High Pressures, by M. L. Cailléte ................................................................. 146
Note to Mr. Moon's Paper on the Solution of Linear Partial Differential Equations of the Second Order 149
On the Rapidity of the Absorption of Carbonic Oxide by the Lungs, by M. N. Gréhan .......................... 150

NUMBER CCLXVI.—SEPTEMBER.

Mr. J. Croll on the Cause of the Motion of Glaciers .......... 153
Mr. G. Gore on the Molecular Movements and Magnetic Changes in Iron &c. at different Temperatures .......... 170
Prof. W. Gibbs on the Measurement of Wave-lengths by means of Indices of Refraction ........................................ 177
Mr. A. S. Davis on the Probable Character of Cometary Orbits 183
Prof. G. Luvini's Experiments and Observations on the Adhesion between Solids and Liquids ............................ 190
M. Achille Cazin on Internal work in Gases .................. 197
The Hon. J. W. Strutt's Remarks on a Paper by Dr. Sondhaus “On the Tones of Heated Tubes and Aérial Vibrations in Pipes of various forms ” .................. 211
The Rev. J. M. Heath on the Principles of Thermodynamics 218
Proceedings of the Royal Society:—
Mr. C. Tomlinson on Supersaturated Saline Solutions .. 221
Proceedings of the Geological Society:—
Mr. W. Carruthers on the Structure of a Fern-stem from the Lower Eocene of Herne Bay ......... 225
Mr. S. Sharp on the Oolites of Northamptonshire ......... 226
On Liquids of High Dispersive Power, by Wolcott Gibbs, M.D. 229
On the Propagation of Sound in Tubes, by M. Ad. Seebeck .. 231

NUMBER CCLXVII.—OCTOBER.

Mr. J. Croll on Ocean-currents.—Part III. On the Physical Cause of Ocean-currents ................................. 233
Dr. E. J. Mills on Statical and Dynamical Ideas in Chemistry. — Part II. Chemical Substance and Chemical Functions 259
Mr. G. Gore on the Magnetism of Electrodynamic Spirals 264
M. A. Cazin on Internal Work in Gases 268
Dr. W. J. M. Rankine on the Thermodynamic Acceleration and Retardation of Streams 288
Dr. W. J. M. Rankine on Thermodynamics 291
M. S. Merz on an Object-glass Spectral apparatus. (With a Plate.) 294
Mr. C. Tomlinson on the Action of Low Temperatures on Supersaturated Saline Solutions 295
Mr. A. S. Davis's Addendum to a Theory of Nebulae and Comets. 300
Notices respecting New Books:—
Dr. Tyndall's Researches on Diamagnetism and Magnetocrystallic Action, including the question of Diamagnetic Polarity 301
Proceedings of the Royal Society:—
Dr. W. Huggins on the Spectra of Erbia and some other Earths 302
Proceedings of the Geological Society:—
Mr. R. J. L. Guppy on the Discovery of Organic Remains in the Caribbean Series of Trinidad 309
Mr. R. Tate on the Palæontology of the Junction-beds of the Lower and Middle Lias in Gloucestershire 309
Mr. T. H. C. Hood on the Waipara River, New Zealand. 310
Experimental Research on the Influence of Heat on Electromotive Force, by Dr. L. Bleekrode 310
On Tests for the Perfection and Parallelism of Plane Surfaces of Glass, by Wolcott Gibbs, M.D., Rumford Professor in Harvard University 311

NUMBER CCLXVIII.—NOVEMBER.

Prof. F. Zöllner on the Temperature and Physical Constitution of the Sun. (With a Plate.) 313
Mr. C. Tomlinson on a Salt that is invisible in its Mother-liquor. 328
Prof. Cayley on the Geodesic Lines on an Oblate Spheroid 328
Mr. J. C. Douglas's Reply to Mr. Templeton's "Remarks suggested by Mr. Douglas's Account of a New Optometer" 340
Mr. F. Guthrie on Approach caused by Vibration. (With a Plate.) 345
Prof. Plateau's Experimental and Theoretical Researches into the Figures of Equilibrium of a Liquid Mass without Weight. — Ninth, Tenth, Eleventh and last Series 355
Proceedings of the Royal Society:—
The Earl of Rosse on the Construction of Thermopiles. 369
CONTENTS OF VOL. XL.—FOURTH SERIES.

The Earl of Rosse on the Radiation of Heat from the Moon.  
—No. II. .......................................................... 372
Mr. A. Le Sueur's Observations with the Great Melbourne  
Telescope ......................................................... 377
Mr. J. Broughton's Chemical and Physiological Experi-  
ments on Living Cinchonæ ...................................... 379
Proceedings of the Geological Society:—
Prof. Owen on the Fossil Remains of Mammals in China. 380
Dr. A. A. Caruana on the Fossil Elephants of Malta ...... 381
Mr. G. Busk on the Species of Rhinoceros discovered in a  
Fissure-cavern at Oreston in 1816 .......................... 381
Mr. H. Y. Hind on two Gneissoid series in Nova Scotia  
and New Brunswick .............................................. 382
Mr. E. Billings on specimens of Lower-Silurian Trilobites. 383
Mr. H. Woodward on the palpus and other appendages of  
Asaphus, from the Trenton Limestone ........................ 384
Dr. J. W. Dawson on the Structure and Affinities of Si-  
gillaria, Calamites, and Calamodendron ..................... 384
The Rev. D. Honeyman on the Geology of Arisaig ......... 386
Mr. E. R. Lankester on the Newer Tertiaries of Suffolk  
and their Fauna ................................................. 386
Dr. Sutherland on an ancient Boulder-clay of Natal ...... 388
Prof. R. Harkness on the Distribution of Wastdale-Crag  
Blocks, or "Shap-Fell Granite Boulders," in West-  
moreland ......................................................... 388
Note on Spiral Nebulae, by T. S. Aldis, M.A. ............. 389
On the Molecular Theory and Laws of Electricity, by L. Lorenz  
of Copenhagen ................................................. 390
Easy preparation of a Liquid for producing Plateau's Figures, by  
Rudolph Böttger .................................................. 392

NUMBER CCLXIX.—DECEMBER.

Prof. A. De la Rive's Researches on the Magnetic Rotatory  
Polarization of Liquids ......................................... 393
Dr. J. Clerk Maxwell on Hills and Dales ................... 421
Prof. J. C. F. Zöllner on Solar Protuberances ................ 427
The Rev. J. M. Heath on the Principles of Thermodynamics 429
Mr. R. Moon on the Equation of Laplace's Coefficients ... 434
Dr. W. J. M. Rankine on the Meteor of November 19, 1870. 440
Mr. T. T. P. Bruce Warren on a New Method of determining  
Resistances ..................................................... 441
Notices respecting New Books:—
Mr. T. M. Goodeve's Text-books of Science.—The Ele-  
ments of Mechanism ........................................... 445
Proceedings of the Royal Society:

Mr. J. M. Heppel on the Theory of Continuous Beams .................................................. 446
Dr. J. W. M. Rankine’s Remarks on Mr. Heppel’s Theory of Continuous Beams.................. 457
On Leclanché’s Manganese elements, by J. Müller ...................................................... 460
On the Melting of Leaden Projectiles by their impact upon an Iron Plate, by Eduard Hagenbach ................................................................. 462
An Experiment on the Boiling in conjunction of two Liquids which do not mix, by August Kundt ................................................................. 463

Index ................................................................................................................................. 465

PLATES.

I. Illustrative of M. Achille Cazin’s Memoir on Internal Work in Gases.
II. Illustrative of Professor Zöllner’s Paper on the Temperature and Physical Constitution of the Sun.
III. Illustrative of M. Merz’s Paper on an Object-glass Spectral apparatus.
IV. Illustrative of Mr. F. Guthrie’s Paper on Approach caused by Vibration.
I. On the Cause of the Descent of Glaciers.
By John Ball, F.R.S. &c.*

AFTER a long period of rest, the controversy respecting the motion of glaciers which occupied so much of the attention of scientific men during the period between 1842 and 1858 has been lately reopened. The Rev. Henry Moseley, who had already in 1855 proposed a theory which failed to obtain the adhesion of men conversant with the facts of glacier-motion, made a communication to the Royal Society in January 1869, wherein he sought to establish the insufficiency of the theory generally accepted by men of science. The mathematical investigation on which he grounded the results given in that paper was communicated to this Journal in May 1869.

Having cleared the way by removing from his path the established theory, Canon Moseley proceeded to prepare for the admission of his own views by two papers which also appeared in this Journal. In August last he published an elaborate mathematical investigation of the problem of the "descent of a solid body on an inclined plane when subjected to alternations of temperature;" and this was followed in January last by a paper upon "The Mechanical Properties of Ice," embodying the results of observations and experiments made by himself and others upon the dilatation, tenacity, and shearing-force of ice. At a Meeting of the Bristol Naturalists' Society in December last, Canon Moseley gave a tolerably full exposition of his own theory, and has lately developed the same views, nearly in the same

* Communicated by the Author.
terms, before a London audience, at the theatre of the Royal Institution.

The arguments of Canon Moseley have not passed unquestioned. In March 1869 Mr. James Croll published in these pages a reply to Canon Moseley’s paper shortly before communicated to the Royal Society, wherein, after admitting that the argument directed against the “ordinary opinion” on the subject of glacier-motion must be considered “decisive,” he attempted to refute the general conclusion by ingenious considerations as to the condition of the molecules of a mass of ice when acted on by external heat.

In the ‘Alpine Journal’ for February last, Mr. William Mathews, well known as a successful explorer of the Alps, and familiar with the phenomena of glaciers, has given an able summary of the present state of our knowledge of the causes of glacier-motion, along with a careful analysis of the views of Canon Moseley as developed in his various writings. In the same paper several weighty objections to Canon Moseley’s views are urged by Mr. Mathews; and the result of an interesting experiment made by the writer, in conjunction with Mr. A. F. Osler, added a fact of considerable value to the materials available in the discussion. A further experiment, made by the same observers under circumstances that much enhanced its importance, was published by Mr. Mathews in the Number of ‘Nature’ for the 24th of March last. The latest contribution to the discussion with which I am acquainted is a reply by Canon Moseley to the strictures of Mr. Mathews, published in the ‘Alpine Journal’ for May 1870.

I trust that I shall be acquitted of any want of respect for the learned and ingenious author of what has been well termed the “crawling theory” of glacier motion, if I discuss it very briefly, and content myself with pointing out a few only of what appear to me insurmountable objections to its acceptance. It was suggested, as is well known, by the casual observation of the gradual descent of sheet lead on a roof of moderate pitch. Although the resistance of friction was far greater than the force of gravity acting in the direction of descent, it was found that the lead continued to slide or crawl down the slope, and was even able to draw out nails that had been driven through it into the rafters beneath with a view to hold it fast. Mr. Moseley detected the physical cause of the phenomenon. He showed that it was a necessary consequence of alternations of temperature. When a body lying upon an inclined plane expands under the influence of heat, the expansion is mainly in the direction of least resistance. When acting in the upward direction, the expansive force has to overcome the resistance opposed by friction, and, in addi-
tion to this, that of the weight of the portion of the body driven up the slope. In the opposite direction, the weight of that portion which moves down the inclined plane acts along with the expansive force to overcome the resistance of friction. The inverse process occurs when the body contracts under the influence of cold; the larger portion of the mass will descend, and the smaller part only will ascend. Hence every alternation of temperature must cause a proportional, however slight, movement of the centre of gravity in a downward direction, and a corresponding descent of the whole mass.

In order to deduce from this observation an explanation of the motion of glaciers, it was necessary to assume that a glacier lying in an alpine valley is a continuous solid, comparable in structure to the sheet of lead upon a roof, that under the influence of sunshine, shade, radiation, &c. its temperature is subject to frequent alternations, and, finally, that the consequence of such changes of temperature is to cause the entire mass of the glacier to expand and contract in the direction of its length.

I have never met any one practically conversant with the phenomena of glaciers who could be brought seriously to discuss this theory, for the simple reason that every one of these assumptions is at variance with the facts of nature. So far from being a continuous solid mass like a sheet of metal, the ordinary condition of glacier-ice is that of a mass of more or less imperfectly welded separate portions, traversed by fissures, and whose upper surface is very frequently cut by deep rents extending to a depth very much greater than that subject to the influence of external changes of temperature.

In the next place we have every possible ground, both from à priori reasoning and direct observation, for believing that the temperature of the interior of a glacier is very nearly constant, varying only by a small fraction of a degree from the freezing-point. Professor Forbes long ago made the obvious objection that the temperature of ice cannot rise above 32° F., while, on the other hand, no cause has been suggested that can tend sensibly to lower the temperature of the interior below that limit. Every one familiar with glaciers is aware that nothing is so rare as to find the surface of a glacier composed of moderately compact or nearly transparent ice. The first effect of the sun shining on the surface is to convert the superficial crust into a mass of crumbling ice filled with cavities, so nearly opaque that it protects the interior of the mass from any but the most trifling influence of luminous heat, and absolutely cuts off all obscure radiation. Only when the superficial crust has been washed away by heavy rain, and before the sun has again acted on the surface, do we find blue ice, more or less compact, appearing on the surface. It has
Mr. J. Ball on the Cause of the Descent of Glaciers.

not, I think, been remarked that, if it were proved that radiant heat did penetrate into the interior of glaciers, the only result would be to bring the mass to the fixed limit of 32° F. (above which it can never rise) at an earlier period in its life-history than really occurs, and thus to destroy at its source the supposed physical agency which forms the basis of the "crawling theory."

As the greater glaciers originate in reservoirs wherein the snow is converted into névé, and whose annual mean temperature is below the freezing-point, we have every reason to believe that, during the long period of its descent in the form of an ice-river, the interior of the mass undergoes a slow secular elevation of temperature until it finally attains that limit. We know that the winter cold does not penetrate the surface more than a moderate number of feet, and that of the night scarcely so many inches, while heat is gained (though with extreme slowness) by conduction and by infiltration.

The last assumption involved in Canon Moseley's theory is that the glacier does, as a matter of fact, increase and diminish in length with every alternation of temperature, and, as a necessary consequence, that there must be portions of an advancing glacier that stand still, and others that at certain times actually retrograde, or crawl up hill. The first impression of any one who had up to this point admitted Canon Moseley's conclusions would be that he had proved too much. If it be true that, in his own words, "glacier-ice being a solid, it cannot but dilate and contract under the variations of temperature to which it is subjected, and, dilating and contracting, it cannot but descend," we must follow the argument to its legitimate consequences. The mean range of a thermometer in the shade at a height of about 7000 feet in the Alps is usually not less than 25° F. in fine summer weather, but that of an instrument exposed to the sun and to radiation may often reach 80° or even 100°. If Canon Moseley's argument be sound, there is no reason for measuring the expansion by the smaller instead of the greater figure. If the surface of a glacier comports itself as though it were a solid sheet of ice, and ice expands for all temperatures, above as well as below the freezing-point, in the ratio of 0.0002855 for every degree of Fahrenheit, there is no escaping from the necessary conclusions. Leaving out of account the upper portions of the glacier, which are covered with névé, we should have on the Mer de Glace a space of 20,000 feet—on the Aletsch glacier a length of fully 50,000 feet—exposed, at least occasionally, to alternations of 100° of temperature. In other words, the length of the Mer de Glace would increase by 57 feet in the course of nine or ten hours, and diminish by the same amount between day and night; and on the Aletsch glacier the expansion and contraction
would amount to 140 feet. What is the value of a theory which, in order to escape the legitimate consequences of its own assumptions, is forced to invoke the intervention of modifying causes so considerable as to reduce the supposed effects to a small fraction (one-twentieth or perhaps one-fiftieth) of what should be their amount.

I pass over many obvious objections that must occur to any one familiar with the phenomena—one of them being that, according to the "crawling theory," the rate of advance of a glacier should, under similar conditions, be proportioned to its length—an inference utterly unsupported by observation—to note one simple fact which alone is sufficient to upset that theory. Universal experience taught mankind, long before thermometers were invented, that snow is one of the most perfect non-conductors of heat. Observations in the arctic regions have proved that a moderate thickness of snow is a protection against the utmost rigour of the polar winter; and we know it to be absolutely opaque to radiant heat (luminous or non-luminous). During the long alpine winter the glaciers are wrapped in a thick mantle of snow—absolutely protected against the alternations of external temperature. In winter the glaciers continue to advance at a rate about one-half their pace in summer. If there be any force in physical reasoning, we must admit that the winter motion of glaciers does not depend upon oscillations of temperature, and that the "crawling theory" is powerless to explain it.

As for the observations on the dilatation of ice, to which Canon Moseley so frequently refers, I simply deny their relevancy. The results were obtained by observing the effect of changes of temperature (always below the freezing-point) on blocks of solid ice, carefully prepared from water that had had the air expelled by boiling. It is conceivable, though I believe that no evidence is available to support the supposition, that ice may continue to expand to a limited extent at temperatures above freezing. Observations carefully made by exposing blocks of ice to non-luminous heat may possibly give interesting results; but they will have no application to glacier-ice and its phenomena.

When Canon Moseley triumphantly asks what becomes of the force which reaches the surface of a glacier under the form of radiant heat, I reply that if he had ever passed a fine morning upon a glacier, after rain had cleared away the crust of rotten ice and had exposed a moderately compact surface, he would have no difficulty in answering his own question. The beautiful observations of Professor Tyndall upon the condition of ice exposed to the sun's rays suggest the true solution. The first effect of the radiant heat, to whatever depth it may penetrate, is to unbuild the crystalline structure of the ice, wherever this is weak-
Mr. J. Ball on the Cause of the Descent of Glaciers.

est, by the formation of "liquid flowers"*. The internal surfaces of each of these, and also of the cells containing water and air that abound in glacier-ice, are so many surfaces of easy melting; and instead of the work of the sun’s rays in melting the ice being confined to the external surface, as Mr. Moseley imagines, it is going on simultaneously at thousands of separate centres, until in a few hours a thickness of several inches of ice is reduced to that crumbling condition familiar to alpine travellers.

I confess that, with all my respect for its author, I should not have thought that Canon Moseley’s theory of glacier-motion required so much consideration, if the managers of the Royal Institution had not invested it with a certain claim to notice by giving it a place in their programme for Friday evenings. It appears to me to be a further illustration of a now familiar truth, that learning and ingenuity, when divorced from a constant reference to the facts of nature, avail but little to interpret her operations.

I now venture to offer some remarks on the objections urged by Canon Moseley against the received theory of glacier-motion. These must be supposed to be weighty, since they have so much impressed such writers as Mr. James Croll and Mr. W. Matthews; but perhaps these gentleman have been overmuch intimidated by the mathematical apparatus brought to bear against their previous convictions; and when the assumptions that lie behind Mr. Moseley’s formulæ come to be carefully examined, it may be found that they do not correspond with the true conditions of the problem.

Let us remember what the facts are that Mr. Moseley maintains to be incompatible with the theory that the motion of glaciers is caused by gravitation acting upon a mass possessing those peculiar physical properties which have been shown to appertain to ice. The supposed difficulty arises from the fact that the motion of a glacier is not uniform throughout every point in its transverse section, but approximates to that of an imperfect fluid. The central portion moves faster than those near the banks; and the motion near the surface is more rapid than that of the deeper parts. It is true, as Mr. Moseley contends, that this involves a relative displacement of the particles of ice; but what is its

* In his lecture before the Royal Institution, Canon Moseley says “Tyn dall, having sent a beam of heat through a block of Wenhara Lake ice, saw its course starred by the dilatations of the ice.” This is a strange misconception. As Professor Tyndall pointed out in his paper read before the Royal Society, the water in the “liquid flowers” occupies less space than the ice-crystals that previously filled the same cavity, and it is to this fact that their visibility is due.
amount? The utmost amount of differential motion yet observed* shows a relative displacement of one inch per day in a distance of sixteen feet. In other words, if we take two points in the glacier one inch apart, it has been shown that the one may slip past the other in twenty-four hours to an extent measured by the sixteenth part of a line, or about the thickness of a sheet of note-paper. There is no doubt that, to accomplish this relative displacement, the force that urged the glacier forwards must overcome the resistance opposed by the mutual cohesion of the particles of the ice. To determine the amount of this resistance, or the shearing-force of ice, Canon Moseley devised the experiment to which he has repeatedly referred in his writings. A cylinder of solid ice is closely fitted into a hole passing through two blocks of hard wood whose faces are brought into close contact, one of which is fixed and the other moveable; and weights are applied to the moveable block until displacement is effected. In short, to ascertain the resistance opposed to very slow changes in the relative positions of the particles, so slight as to be insensible at short distances, Mr. Moseley measures the resistance opposed to rapid disruption between contiguous portions of the same substance. Without entering into a detailed examination of the question, I should think that so familiar a fact as the behaviour of sealing-wax at temperatures between 70° and 80° F. sufficiently shows the fallacy of this experiment. A cylindrical stick of sealing-wax half an inch in diameter requires a sharp blow to break it, and will bear for some time a considerable weight, placed close to the point at which it is horizontally supported, without apparent yielding; but if placed in the same position for twenty-four hours, with no other pressure acting on it than its own weight, you will find it unable to sustain that slight pressure. It will be bent or twisted, and relative displacements of the particles far greater than occur in glacier-ice will have ensued.

I venture to hold that the doubts expressed by Mr. Mathews in his excellent paper are more than justified. I am persuaded that, in attempting to estimate experimentally the resistances opposed by solid bodies to change of form, time is an essential element, and that this is especially true when such bodies are brought near to their melting-point. In the present case, without adverting particularly to minor sources of error, I think that it would be a sufficient answer to Canon Moseley to deny altogether the relevancy of his observations on the shearing-force of ice to the argument in hand.

* In Professor Tyndall's observations at the Tacul, where the middle stake, 31 feet above the lower one, advanced at the daily rate of 4'50 inches, while the motion of the lower one was 2'56 inches.
I desire, however, to go a step further, and to offer a still more fundamental objection to the method of investigation adopted by Canon Moseley—and the more so as it is frequently employed by eminent men of science, and, when not applied with great caution, leads to results which I believe to be altogether fallacious.

The device adopted in order to bring such problems within the grasp of strict mathematical reasoning is to break up the mass under consideration into imaginary elements, and to assume that the forces acting on each of these may be expressed by certain constants determined experimentally, and by functions of variables depending on their position with reference to fixed co-ordinates. The fundamental assumption that underlies the application of this method is that of the uniform structure of the entire mass. Already open to question when used in regard to imperfect fluids, this method is altogether fallacious when applied to solid bodies, for the simple reason that no solid body is homogeneous, and that pressures and tensions acting within it are modified by the varying constitution of the mass. But if this be true generally, the objection falls with tenfold force when it is proposed to treat a glacier as a body made up of uniform strata, slices, or cubes, identical in structure, and acted on by similar forces. Not to speak of wide openings and narrower fissures, of inequalities of composition made manifest by the veined structure, air-bubbles, &c., there is this special characteristic of ice at or very near the freezing-point—that under pressure it changes its form and is partly converted into water, of which some portion is carried off by infiltration. Hence nothing can be less like a natural glacier than the ideal object whose existence is assumed by Canon Moseley in order that he may be able to pass it through his mathematical mill. Instead of being a mass of homogeneous ice, lying on an even slope in a uniform rectangular channel, each portion or vertical slice of which advances with a uniform motion uninfluenced by lateral thrusts or tensions, the real glacier slides slowly on its bed, which is of broken and irregular form: the vast weight of the mass is thus made to exert pressure in various directions, by no means uniformly parallel to the direction of motion: forces of unknown but enormous amount act in succession on each portion of the mass, causing fracture in one direction, liquefaction in another; and the occurrence of each of these changes transfers the maximum pressure to a new point where similar results are produced. Like a huge snake whose movements are effected by the transmission of muscular energy from one point to another, the mass works its way downwards, straining and groaning audibly as, now here, now there, the pressure seems to crush its own vitals.

It is scarcely worth while to make the obvious remark, that if
Canon Moseley's objections to the received theory be admitted as valid, they will be found to apply with equal force to his own theory. Observation abundantly proves that the resistance offered by the bed of great glaciers to the sliding of the lower surface is very much less than even the smallest amount of shearing-force derived from his several discordant observations. If the mass of the glacier were dragged onwards, as he contends, by the alternate expansion and contraction of the superficial strata, it is certain that the lower surface would slide on its bed at the same rate as the upper part. It is equally easy to show that the differential motion found in going from the central part to either bank would be an impossibility. The whole would move forward as one mass, like the sheet of lead upon the roof, and only where some small lateral portion of the ice encountered a fixed obstacle would the resistance lead to fracture—just as in Canon Moseley's cylinder experiments. The larger mass would move on, the smaller fragment would be left behind.

If I might presume to estimate the net results of this renewed discussion of the causes of glacier-motion, I should say that they are not considerable, but yet are far from worthless. Canon Moseley's experiments have added something to our knowledge, and especially those on the tenacity of ice, which have some bearing on the origin of crevasses. Of far greater importance are the observations on ice-planks made by Mr. William Mathews. The first of these, published in the 'Alpine Journal,' gave prominence to a fact which had long been familiar to myself, and probably to many others. I have often found that long icicles placed in an inclined position, and supported only at the upper end, will gradually resume the vertical direction, and I had, perhaps too lightly, assumed that this was a particular instance of the process by which ice changes its form through fracture and regelation. In Mr. Mathews's first experiment, conducted during a thaw, a thick plank of ice supported at each end was deflected at the middle through a space of 7 inches in as many hours. Although none but very minute fissures were observed, the facts did not seem to me altogether inconsistent with that explanation. In the second series of observations, made during the severe frost of February last, Mr. Mathews found that at temperatures notably below the freezing-point a plank of ice, supported as before, subsides slowly between the points of support under the sole influence of its own weight. The deflection under these circumstances was about 1\(\frac{1}{2}\) inch in twenty-four hours. Taking this observation in connexion with a multitude of facts recently brought to light, and especially the researches of M. Tresca, we are led to admit that ice, in common with very many apparently rigid bodies, does possess a certain degree of plasticity which is
exhibited by changes of form effected very slowly under the action of forces of moderate amount, rather than by the rapid action of more powerful agencies.

The admission of this conclusion may slightly modify, but will not materially alter, the views now generally held as to the causes of glacier-motion, which are mainly derived from the remarkable researches of Professor Tyndall. Whatever may be the final judgment of men of science, I feel quite sure that it will not confirm the opinion expressed by Canon Moseley in his latest publication: that "the phenomena of glacier-motion belong rather to mechanical philosophy than to physics." Every real advance that has been made towards the explanation of those phenomena has been due to the application of increased knowledge of the physical properties of glacier-ice; and if any thing be wanting to complete the explanation now generally accepted, it must be derived from such additional acquaintance with those properties as may be derived from continued observation and experiment.

II. Reply to M. Delaunay's objection to the late Mr. Hopkins's Method of determining the Thickness of the Earth's Crust by the Precession and Nutation of the Earth's Axis. By Archdeacon Pratt, M.A., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It is only two days ago that I saw for the first time M. Delaunay's strictures* upon the late Mr. Hopkins's method of ascertaining the least thickness of the Earth's Crust by means of the phenomena of Precession and Nutation, although I had previously seen a notice that such strictures had been laid before the French Academy. Having now that gentleman's paper before me, I write to endeavour to convince your readers that the point of Mr. Hopkins's reasoning has been altogether missed, and that his method stands altogether unimpaired by these strictures.

2. I am ready to allow, and so would Mr. Hopkins have allowed, that if the crust of the earth revolved round a steady axis, always parallel to itself in space, and if at some particular epoch a difference existed between the rate of movement of the crust and of the fluid within it, the resulting friction would gradually destroy this difference and bring about a conformity in the motion of both parts. I will even go further, and allow that the effect of the internal friction and viscosity of the fluid may be such that the resulting rotary motion may be the same as that which the whole mass would have had at the epoch if it

* Translated in the Geological Magazine, November 1868, p. 507.
had suddenly become one solid body and thereby suddenly retarded the rotation. This, before proceeding, I will illustrate by an example for the use of your mathematical readers.

3. Suppose a spherical shell or crust of mass $C$ to have within it a solid spherical nucleus, of radius $b$ and mass $N$, fitting it exactly; and the crust to receive an angular velocity of rotation around an axis fixed in the crust, the nucleus at that moment having no angular velocity; but suppose that a slight force of friction between the surfaces gradually generates a rotary motion in the nucleus; and suppose this force to vary as the difference between the angular velocities of the crust and nucleus—that is, of the surfaces in contact. Let $\omega$ and $\omega'$ be the angular velocities at the time $t$, $k$ and $k'$ the radii of gyration of the two bodies, $F(\omega - \omega')$ the force at the equator of the nucleus which represents the friction between it and the crust. Then the equations of motion are

$$\frac{d\omega}{dt} = -\frac{Fb}{Ck^2}(\omega - \omega'), \quad \frac{d\omega'}{dt} = \frac{Fb}{Nk'^2}(\omega - \omega'). \quad (1)$$

Suppose also that $\beta$ would have been the angular velocity, when the primitive impulse was given, on the hypothesis of the crust and nucleus being rigidly connected so as to be one mass. Then

$$\beta(Ck^2 + Nk'^2) = \alpha Ck^2. \quad \ldots \ldots \quad (2)$$

Subtracting the second of equations (1) from the first, putting

$$Fb\left(\frac{1}{Ck^2} + \frac{1}{Nk'^2}\right) = c, \quad \ldots \ldots \quad (3)$$

and integrating, we have

$$\omega - \omega' = \text{const.} \times e^{-ct}.$$  

When $t=0$, $\omega = \alpha$ and $\omega' = 0$;

$$\therefore \omega - \omega' = \alpha e^{-ct}.$$  

Hence, by the first of equations (1),

$$\frac{d\omega}{dt} = -\frac{Fb\alpha}{Ck^2} e^{-ct} = -\frac{Nk'^2 \alpha}{Ck^2 + Nk'^2} e^{-ct}, \text{ by (3)};$$

$$\therefore \omega = \alpha - \frac{Nk'^2}{Ck^2 + Nk'^2} \alpha(1 - e^{-ct});$$

and also

$$\beta \left(1 + \frac{Nk'^2}{Ck^2} e^{-ct}\right), \text{ by (2)}.$$  

The first of these expressions shows that the angular velocity of the crust begins with $\alpha$; and when $ct$ becomes very large indeed, it is reduced to $\beta$. Hence the effect of the constant friction of the nucleus against the inner surface of the crust is at last to reduce the velocity of the crust to what it would have been at first if the crust and nucleus had been one solid mass.
We may conclude perhaps that the same effect would be produced, though in a much longer time, if the interior were not a solid sphere, but a fluid mass.

The above reasoning shows that if the disturbing force producing precession and nutation did not exist, and the interior of the earth were fluid (whatever the thickness of the crust), it may be fairly assumed that the motion of rotation of the crust would now, the earth having existed so many ages, be exactly what it would have been had the earth been one solid mass, all difference of motion having been long ago annihilated by the internal friction and viscosity.

4. But the disturbing force producing precession and nutation does exist. It consists of two parts, one constant and the other variable and periodical. The constant part is that which produces the steady precession of the axis (and which I will call for convenience the precessional force); the other produces the nutation. I will consider the precession first. Suppose now, for the sake of argument, that at the present moment, as M. Delaunay imagines, the crust and the fluid are revolving precisely as one mass, all previous differences of motion, even under the action of the disturbing force which produces precession and nutation, having been annihilated by friction and viscosity. I ask, What will be the action of the precessional force from this moment? It tends to draw the pole of the crust towards the pole of the ecliptic: and this tendency, as mathematical physicists well understand, combined with the rotary motion of the crust, produces this singular result, viz. the pole does not move towards the pole of the ecliptic, but shifts in a direction at right angles to the line joining the poles towards the west; so that the inclination of the axis to the ecliptic remains constant, but the axis shifts towards the west. The space through which it shifts in an infinitesimal portion of time varies as the length of the time and the force directly, and as the inertia of the mass to be moved inversely. The inertia of the mass depends upon the thickness of the crust only; for the friction of the fluid against the inner surface of the crust (which might, as I have shown, in the course of years produce a sensible effect) cannot do so during the infinitesimal portion of time I am considering before the precession is actually produced. The precessional force has its full effect in producing the precession of the solid crust, the fluid not having time to diminish that effect before the axis has assumed a new position; and in this new position of the axis the precessional force is precisely the same in amount as before, to go on causing the precession as before. The precessional force is, in fact, ever alive and active, and shows this in incessantly producing the effect I have described; and the precession goes on
steadily, the amount of it depending upon the mass of the crust thus moved, which the fluid has not time to retard or lessen. M. Delaunay says that "the additional motion due to the above-mentioned causes [the disturbing forces which give rise to precession and nutation] is of such slowness, that the fluid mass which constitutes the interior of the globe must follow along with the crust which confines it, exactly as if the whole formed one solid mass throughout." In reply to this, I say that it is not the slowness of the motion, but the want of solid connexion between the crust and the fluid in contact with it which affects the problem. The motion, whatever its amount, is incessantly being generated by the disturbing force; and, owing to this want of solid connexion, the friction of the fluid has not time during the successive moments during which the precession is generated, to stop or even sensibly to check it.

It will thus be seen that at every instant the precessional force proceeding from the action of the sun and moon on the protuberant part of the earth's mass will, if the earth be a solid mass, have to move the whole mass; and if the earth have a solid crust only with a fluid interior, the force will have to move only the crust, against the evanescent resistance of the fluid within during so short a space of time as it takes to produce precession. The resulting precessional motion will be different in the two cases; and therefore the actual amount of the precession which the earth's axis has (and which is a matter of observation) is a good test of the solidity or fluidity of the interior. This is Mr. Hopkins's method.

The force producing nutation is much smaller, even at its maximum, than the precessional force. Its effect, however, is precisely the same in this respect—that it depends upon the mass of the solid crust, and in no respect upon the friction of the fluid within it, which has not time to influence the nutation before the nutation is actually produced.

5. I do not here undertake to go into Mr. Hopkins's numerical calculations; I simply vindicate his method. I do not here consider what modification the elasticity of the solid material of the earth may have upon his numerical results. I conceive that it would have no effect, if the disturbing force were constant and there were no nutation. For, under the dragging influence (if I may so call it) of the constant precessional force, the solid material would be under a steady strain, and would communicate the effect of the force, continuously acting, from particle to particle of the solid part as if it were really rigid; and the resulting precessional motion would be greater or less as the mass of the solid part may be smaller or larger—that is, the solid crust thinner or thicker. But as the disturbing force is not constant,
but variable, and there is constantly nutation of the axis as well as precession, the action above described will be somewhat modified; and the elasticity of the solid material may be expected to have some influence on the result. This influence, however, will be minute, as the part of the disturbing force which is variable and produces nutation is very much smaller, even at its maximum, than the precessional force. The consideration of this matter, however, has no bearing upon the validity or not of Mr. Hopkins’s method, but simply upon the numerical value of his final result, not upon the question of the fluidity or solidity of the earth’s mass.

6. It will appear then, I think, to your readers that the strictures of M. Delaunay upon this method, which the genius of Mr. Hopkins devised, betray an oversight of the real point upon which the success of his method depends, and that this method stands unimpaired.

I am,

Yours faithfully,

Murree, Himalayas,
May 17, 1870.

John H. Pratt.

III. On the Path of Electrical Induction- and Disjunction-Cur- rents through Gases of various Densities, and between Poles of different shapes. By E. Edlund*.

1. For shortness’ sake I will, in what follows, apply the term electrical disjunction-currents to those currents which have their origin in the voltaic arc or in the electrical spark, and in the same connexion will speak of the force to which they owe their origin as the electromotive force of disjunction. This name indicates that to produce these currents the conduction must be broken, in order that a luminous arc or spark may be formed, as well as that the poles between which the luminous pheno- menon is formed are mechanically disintegrated†.

In the investigation I used the same electrophorus machine as that employed in my earlier experiments on these currents. An insulated copper wire, ac, is directly connected with the knob a (see the figure) on one absorber, while the insulated

* Translated from Poggendorff’s Annalen for March 1870, having been read before the Swedish Royal Academy of Sciences at Stockholm, October 13, 1869.
† In the sequel I shall use the term “disintegration of the poles” to denote the whole mechanical work which the current performs in the spark, although this work is consumed not only in disintegrating the poles, but also in imparting velocity to the detached particles, in putting masses of air in motion, &c.
copper wire \( de \) terminates in a brass knob in the vicinity of \( b \). From \( c \) and \( e \) insulated conducting wires pass to the knobs \( f \) and \( g \). Two other conducting wires pass from the joints \( c \) and \( e \) to the galvanometer \( g \), of which instrument I have already given a description. The bridge \( h \), which consists of a German-silver wire, connects the points \( i \) and \( k \) with one another; \( l \) is a conducting wire leading to the earth, and \( m \) a rheostat consisting of fine German-silver wire. The deflections indicated by the needle of the galvanometer when the machine is in action are produced by three different currents—namely, (1) that part of the discharge of the machine which passes through the coil of the galvanometer, (2) the disjunction-current formed in the spark between \( f \) and \( g \), and (3) the induction-currents which are produced by the discharge in the coil of the galvanometer. As regards the first current, it is under ordinary circumstances so inconsiderable as compared with the others that it need not be taken into account. The disjunction-current, on the contrary, produces a considerable deflection; but this is very materially diminished when the induction-currents from the galvanometer traverse the spark between \( f \) and \( g \). As I have already shown in a previous paper*, the spark acts like an electrical valve; that is, it allows one of the two equal but oppositely directed induction-currents to pass in larger quantity than the other. The difference in the two induction-currents thus occasioned acts therefore on the magnetic needle, and always in such a manner that the deflection produced by the disjunction-current is diminished. When, therefore, the magnitude of the disjunction-currents under various circumstances is to be investigated, the experiments must be so arranged that the action of the induction-currents on the magnetic needle shall be nullified as much as possible. This may be most easily effected by inserting a bridge between \( i \) and \( k \). In this case only part of the discharge goes through the coil, and the induction is therefore

weaker; moreover, of the two opposite induction-currents, equal parts pass through the bridge, by which their action on the needle is annulled. Only that part of the two currents which traverses the spark can affect the needle. In order to make this as small as possible, the resistance in the bridge must be small as compared with the sum of the resistance in the rheostat and in the spark. But in proportion as the resistance in the bridge is diminished, the deflection of the disjunction-current is also diminished; for this now takes its path more and more through the bridge instead of through the galvanometer. Hence the resistance in the bridge must not be made smaller than so that the action of the induction-currents upon the magnetic needle just becomes imperceptible.

Experiments specially made for this purpose showed that a German-silver wire 27 centims. in length and 0·7 in diameter fulfilled these conditions; and it was therefore used as a bridge in front of the galvanometer. That the action of the induction-currents upon the needle was thus rendered imperceptible was shown as follows:—An induction-coil, C, of exactly the same nature as the galvanometer-coil, was inserted between m and k, and in front of it a German-silver wire as bridge, of the same length and the same diameter as the previous one. The galvanometer-coil and the coil R were thus both in the same position; they must produce equal induction-currents; and equal parts of these must traverse the respective bridges. If, then, it can be proved that the induction-currents from the coil C have no influence on the deflection of the magnetic needle when the bridge is in front of this coil, the proof is valid also in the case of the galvanometer-coil. Of the observations made the following may be adduced, with regard to which it must be observed that the resistance in the conducting-wires and in the spark was so great, compared with that in the coil and in the German-silver wire just mentioned, that the latter need not be taken into account.

Experiment 1.—The German-silver wire inserted between m and k. When the machine was put in motion the following deflections were observed:—

35·5
divisions.

35·5

Mean . . 35·5

Experiment 2.—The coil C was inserted between m and k, so that the German-silver wire formed the bridge:—

Deflections.

35·8

37·6

Mean . . 36·7
Disjunction-currents through Gases of various Densities.

Experiment 3.—To ascertain whether any change had taken place in the machine, the first experiment was repeated:—

<table>
<thead>
<tr>
<th>Deflections</th>
</tr>
</thead>
<tbody>
<tr>
<td>34.3</td>
</tr>
<tr>
<td>37.3</td>
</tr>
</tbody>
</table>

Mean 35.8

When the coil C was accompanied by the bridge, no indication of induction was observed; but when, on the contrary, the bridge was removed, the deflection was diminished by more than one-half.

The following experiments were made in the same manner as the preceding, after the resistance in the rheostat had been doubled.

Experiment 4.—The German-silver wire in the circuit:—

<table>
<thead>
<tr>
<th>Deflections</th>
</tr>
</thead>
<tbody>
<tr>
<td>20.5</td>
</tr>
<tr>
<td>21.5</td>
</tr>
<tr>
<td>21.5</td>
</tr>
</tbody>
</table>

Mean 21.2

Experiment 5.—C inserted along with the German-silver wire, the latter as bridge to the former:—

<table>
<thead>
<tr>
<th>Deflections</th>
</tr>
</thead>
<tbody>
<tr>
<td>21.6</td>
</tr>
<tr>
<td>20.5</td>
</tr>
</tbody>
</table>

Mean 21.0

Experiment 6.—The same experiment as No. 4:—

<table>
<thead>
<tr>
<th>Deflections</th>
</tr>
</thead>
<tbody>
<tr>
<td>22.8</td>
</tr>
<tr>
<td>22.1</td>
</tr>
</tbody>
</table>

Mean 22.4

This series, therefore, gave the same result as the first.

In order to investigate the nature of the electromotive force of disjunction when the spark was formed in various more or less rarefied gases, a glass cylinder 12 centims. in length and 7 centims. in diameter was used. Brass caps could be screwed on air-tight at each end of the cylinder. In the centre of each cap was a stuffing-box, through which a round brass rod could be moved air-tight backwards and forwards. To the inner ends of these brass rods were firmly screwed the poles which were used in the experiments. The outer ends were provided with binding-screws for the conducting-wires; and on one rod was marked a scale of millimetres, to determine the distance between the poles. The brass rods were insulated from the caps and the stuffing-boxes.
On one of these caps was screwed a brass tube with a stopcock, the other end of which was provided with a screw-thread by which it could be screwed to an air-pump. The brass tube was bent at right angles, so that during the experiments the glass cylinder was in a horizontal position.

To convince myself whether the bridge acted efficiently when the spark was formed in rarefied air, the glass cylinder was inserted between the points $c$ and $e$, and the air rarefied until it was at a pressure of 15 millims.; the spark, therefore, at $fg$ was formed in rarefied air.

**Experiment 7.**—The German-silver wire in the circuit:

<table>
<thead>
<tr>
<th>Deflections</th>
<th>15.7</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>15.7</td>
</tr>
<tr>
<td>Mean</td>
<td>15.7</td>
</tr>
</tbody>
</table>

**Experiment 8.**—C inserted along with the German-silver wire, the latter as bridge to the former:

<table>
<thead>
<tr>
<th>Deflections</th>
<th>13.8</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>13.0</td>
</tr>
<tr>
<td>Mean</td>
<td>13.4</td>
</tr>
</tbody>
</table>

**Experiment 9.**—The same as No. 7:

<table>
<thead>
<tr>
<th>Deflections</th>
<th>12.0</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>12.7</td>
</tr>
<tr>
<td></td>
<td>15.5</td>
</tr>
<tr>
<td>Mean</td>
<td>13.4</td>
</tr>
</tbody>
</table>

Hence a distinct action of the induction-currents could not be observed; and the same result was obtained when the air was exhausted to a pressure of 6 millims. In all the subsequent experiments the bridge remained in the same position in front of the galvanometer; and thus the deflections obtained were independent of the induction-current of the galvanometer-coil. When in the sequel nothing is said to the contrary, the poles consisted of two equal-sized brass knobs.

2. Comparison between dry air and air saturated with aqueous vapour.

The air was dried before entering the cylinder by being passed slowly through two glass vessels which were filled with pieces of pumice impregnated with concentrated sulphuric acid, and then through a tube filled with chloride of calcium. The air was moistened by having to pass through a long glass tube which contained pieces of wetted paper.
Disjunction-currents through Gases of various Densities.

Experiment 10.—Air in the glass cylinder saturated with moisture:

<table>
<thead>
<tr>
<th>Deflections</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>36·8</td>
<td></td>
</tr>
<tr>
<td>37·3</td>
<td></td>
</tr>
<tr>
<td>42·3</td>
<td></td>
</tr>
<tr>
<td>40·3</td>
<td></td>
</tr>
</tbody>
</table>

Mean . . 39·2

Experiment 11.—Dry air:

<table>
<thead>
<tr>
<th>Deflections</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>49·7</td>
<td></td>
</tr>
<tr>
<td>52·0</td>
<td></td>
</tr>
<tr>
<td>53·0</td>
<td></td>
</tr>
</tbody>
</table>

Mean . . 51·6

Experiment 12.—The same as No. 10:

<table>
<thead>
<tr>
<th>Deflections</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>41·8</td>
<td></td>
</tr>
<tr>
<td>44·3</td>
<td></td>
</tr>
<tr>
<td>39·6</td>
<td></td>
</tr>
</tbody>
</table>

Mean . . 41·9

Another series of observations made, under slightly altered circumstances, gave the following results:

Experiment 13.—Air saturated with moisture:

<table>
<thead>
<tr>
<th>Deflections</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>35·8</td>
<td></td>
</tr>
<tr>
<td>35·3</td>
<td></td>
</tr>
<tr>
<td>38·3</td>
<td></td>
</tr>
</tbody>
</table>

Mean . . 36·5

Experiment 14.—Dry air:

<table>
<thead>
<tr>
<th>Deflections</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>44·5</td>
<td></td>
</tr>
<tr>
<td>42·0</td>
<td></td>
</tr>
<tr>
<td>42·0</td>
<td></td>
</tr>
</tbody>
</table>

Mean . . 42·8

Experiment 15.—The same as No. 13:

<table>
<thead>
<tr>
<th>Deflections</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>34·5</td>
<td></td>
</tr>
<tr>
<td>34·0</td>
<td></td>
</tr>
<tr>
<td>35·5</td>
<td></td>
</tr>
</tbody>
</table>

Mean . . 34·7
Hence the deflection is always greater in dry air than in that which is saturated with moisture. Even when dry air was compared with air in the working-room, which was far from being saturated with moisture, the deflection was greater in dry air. In an experiment on this point a deflection of 8 divisions was obtained for the dry air, and for the undried air a deflection of 6·1 divisions.

It does not seem very easy clearly to make out the causes why the greater deflection was obtained with the spark in dry air. The following circumstance would have to be taken into account. It is necessary for the formation of the sparks in dry air that the electric density upon the pole-surfaces be greater than when the air is moist. Before the formation of sparks, the electrical density increases upon the polar surfaces until it is great enough to traverse the layer of air between them. Hence in moist air the spark appears sooner and with a smaller electrical density. The disintegration of the polar surfaces thereby becomes smaller, and consequently there is a diminution in the electromotive force of the disjunction. As the conducting-power of the spark doubtless depends on the quantity of detached metallic particles, this will also be smaller. Now, if a diminution of the electromotive force and of the conducting-power take place when the air is moist, a decrease in the magnitude of the deflection must necessarily follow. The deportment with rarefied gases seems to speak in favour of the same mode of explanation.

Experiment 16.—The glass cylinder was filled with carbonic acid, exhausted, and these operations repeated several times until it was certain that no air was left. The carbonic acid, which was prepared from marble and hydrochloric acid, and was ascertained by testing to contain scarcely perceptible traces of foreign gases, was dried before entering the apparatus previously mentioned. The polar cones in the glass cylinder had to be pushed very near together, because otherwise the spark would not pass when the cylinder was filled with carbonic acid. This is the reason why the deflections were relatively small.

With carbonic acid the following deflections were observed:

<table>
<thead>
<tr>
<th>Deflections.</th>
<th>16·3</th>
<th>13·3</th>
<th>15·3</th>
<th>16·8</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean</td>
<td>15·4</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Experiment 17.—The glass cylinder was filled with air (undried):—
Disjunction-currents through Gases of various Densities.

Deflections.

Experiment 18.—The same as No. 16:

Deflections.

\[
\begin{align*}
\text{Deflections} & \\
6.2 & \\
6.2 & \\
7.2 & \\
\text{Mean} & = 6.5
\end{align*}
\]

A few other observations gave the same result—that is, a considerably greater deflection for carbonic acid than for air.

Experiment 19.—The glass cylinder was filled with hydrogen which had been dried before entering the cylinder. The following deflections were thereupon observed:

Deflections.

\[
\begin{align*}
\text{Deflections} & \\
13.5 & \\
11.5 & \\
11.5 & \\
\text{Mean} & = 12.2
\end{align*}
\]

Experiment 20.—The cylinder was filled with air:

Deflections.

\[
\begin{align*}
\text{Deflections} & \\
6.0 & \\
5.5 & \\
5.7 & \\
\text{Mean} & = 5.7
\end{align*}
\]

Experiment 21.—The same as No. 19:

Deflections.

\[
\begin{align*}
\text{Deflections} & \\
14.3 & \\
15.3 & \\
14.8 & \\
\text{Mean} & = 14.8
\end{align*}
\]

With hydrogen, then, the deflection was considerably greater than with air. When, on the contrary, the cylinder was filled with coal-gas, the deflection was but very little greater than with atmospheric air. With the former mixture the deflection 13.5 was observed, and with the latter 11.9.

3. In order to investigate the dependence of the disjunction-current on the density of the gas in which the spark is formed, experiments were made with atmospheric air, carbonic acid, and
Prof. E. Edlund on the Path of Electrical Induction and coal-gas. The two former were dried, but the latter not. The means only may here be given:—

<table>
<thead>
<tr>
<th>Pressure in the glass cylinder (atmospheric air)</th>
<th>1 atmosphere</th>
<th>80</th>
<th>40</th>
<th>20</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sphere mm.</td>
<td>140</td>
<td>70</td>
<td>15</td>
<td>8</td>
<td>20</td>
</tr>
<tr>
<td>Deflections</td>
<td>42.7</td>
<td>6.5</td>
<td>13.7</td>
<td>20.6</td>
<td>20.9</td>
</tr>
</tbody>
</table>

When the pressure was reduced from one atmosphere to 140 millims., the deflection diminished from 44.4 to 6.6 divisions, when it again increased until for a pressure of 4 millims. it was greater than for one atmosphere.

Experiments with dried carbonic acid led to an analogous result; the deflection was least for a pressure of 140 millims., after which it again began to increase:—

<table>
<thead>
<tr>
<th>Pressure in the glass cylinder (carbonic acid)</th>
<th>1 atmosphere</th>
<th>80</th>
<th>40</th>
<th>20</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sphere mm.</td>
<td>140</td>
<td>3.4</td>
<td>15</td>
<td>1.7</td>
<td>10</td>
</tr>
<tr>
<td>Deflections</td>
<td>23.6</td>
<td>5.0</td>
<td>19</td>
<td>8.7</td>
<td>16.5</td>
</tr>
</tbody>
</table>

With coal-gas there was also a diminution in the deflection when the pressure was diminished, although here the variations were not so great as in the two previous gases. For this mixture there was obtained the following result:—

<table>
<thead>
<tr>
<th>Pressure in the glass cylinder (coal-gas)</th>
<th>1 atmosphere</th>
<th>80</th>
<th>40</th>
<th>20</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sphere mm.</td>
<td>140</td>
<td>28</td>
<td>23</td>
<td>8</td>
<td>21.8</td>
</tr>
<tr>
<td>Deflections</td>
<td>40.1</td>
<td>27.1</td>
<td>26</td>
<td>3</td>
<td>24.9</td>
</tr>
<tr>
<td>Mean</td>
<td>39.7</td>
<td>27.9</td>
<td>25.1</td>
<td>24.9</td>
<td>23.4</td>
</tr>
</tbody>
</table>

That the deflections first decrease and afterwards again increase when the pressure is diminished, indicates that there are several causes for these variations. The magnitude of the deflection depends on the electromotive force, on the conducting-power of the spark, and on its duration. That the electromotive force decreases with the pressure follows from the fact that the disintegration of the polar surfaces becomes less as the gas is rarefied, because the electrical density of the polar surfaces which is requisite for the formation of sparks diminishes with the pressure.*

* In a previous paper I assumed, without any experimental proofs, that the electrical spark is subject to no perceptible change when a voltaic
If, now, the conducting-power of the gas, as is probable, increases, and the duration of the spark lengthens, when the gas is rarefied, the result obtained (that the deflections first decrease and then increase) contains nothing inexplicable. Further investigations, however, are needed to enable us to decide with certainty whether the mode of explanation which has been indicated is admissible.

In connexion with the above, experiments were also made with some Geissler's tubes, in order to ascertain whether the disjunction-current could be perceived in them or not. With three of them, one (according to the label) containing oxygen, another hydrogen, and the third chlorine, distinct deflections were exhibited; while another, which contained carbonic acid, as well as one without label, gave no distinct proof of the existence of a disjunction-current.

4. The current in the voltaic luminous arc is well known to occasion a greater disintegration of the positive than of the negative pole. When two equal polar surfaces, between which the discharge from an electrophorus machine has been for some time taking place, are closely examined, it is easy to discriminate the positive polar surface from the negative; for the former appears more altered than the latter. Hence positive electricity is most active in the disintegration. As positive electricity readily issues from a sharp point without producing there a more powerful disintegration, it must follow that, when one pole consists of a point and the other of a plane disk at right angles to the plane of discharge, the disintegration is greatest when the positive current goes from the disk to the point. When, therefore, the discharge goes through the spark from the disk to the point, it is to be expected that the disjunction-current will be stronger—partly because the electromotive force of the disjunction increases with the disintegration, and partly also because the quantity of particles detached from the poles is greater, and therefore the conducting-power of the spark is better.

To test the accuracy of what has here been said, a round brass disk 2·7 millims. in diameter was fastened on one of the two metal rods of the glass cylinder, and upon the end of the other rod a conically sharpened brass point was screwed.

current traverses it in either direction; and on this assumption I have based a method of directly measuring the electromotive force of disjunction. This assumption has been found to be incorrect. The spark undergoes a considerable change by the passage of the voltaic current, so that the determinations obtained can only be regarded as valid for the case in which a voltaic current traverses the spark.
### Experiment 22.

<table>
<thead>
<tr>
<th>Negative Disk</th>
<th>Positive Disk</th>
<th>Negative Disk</th>
</tr>
</thead>
<tbody>
<tr>
<td>28.9</td>
<td>36.6</td>
<td>27.4</td>
</tr>
<tr>
<td>27.9</td>
<td>34.1</td>
<td>25.9</td>
</tr>
<tr>
<td>24.0</td>
<td>34.1</td>
<td>25.4</td>
</tr>
</tbody>
</table>

**Deflections.**

<table>
<thead>
<tr>
<th>Mean</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>27.1</td>
<td>34.2</td>
</tr>
</tbody>
</table>

### Experiment 23.

The following observations were then made, after the machine had been somewhat altered and the conductions had been reversed, so that the deflections would be towards the opposite side:

<table>
<thead>
<tr>
<th>Negative Disk</th>
<th>Positive Disk</th>
<th>Negative Disk</th>
</tr>
</thead>
<tbody>
<tr>
<td>35.4</td>
<td>47.3</td>
<td>36.5</td>
</tr>
<tr>
<td>34.7</td>
<td>46.3</td>
<td>36.7</td>
</tr>
<tr>
<td>34.0</td>
<td>44.3</td>
<td>35.7</td>
</tr>
</tbody>
</table>

**Deflections.**

<table>
<thead>
<tr>
<th>Mean</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>34.7</td>
<td>45.0</td>
</tr>
</tbody>
</table>

### Experiment 24.

The conical brass point was removed, and in its place a glass tube 3 centims. in length enclosing a platinum wire a millimetre in diameter was screwed on. The wire reached exactly to the end of the glass tube. Three series of experiments were made with this, in which the length of the spark was 1, 2, and 3 millims. It is sufficient to give here only the last, mean numbers:

<table>
<thead>
<tr>
<th>Length of Spark</th>
<th>Negative Disk</th>
<th>Positive Disk</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 millim.</td>
<td>12.0</td>
<td>8.1</td>
</tr>
<tr>
<td>2 millims.</td>
<td>26.2</td>
<td>19.2</td>
</tr>
<tr>
<td>3 millims.</td>
<td>47.5</td>
<td>37.6</td>
</tr>
</tbody>
</table>

From Experiments 22 to 24, it follows with certainty that the disjunction-currents are strongest when the positive electrical discharge proceeds from the disk to the point. The cause, as has already been stated, is that in this case the disintegration of the pole-surfaces is most powerful. It is clear that the difference between the two disjunction-currents must become smaller if instead of a brass disk a similar one be used of another metal which is more easily disintegrated, so that the mechanical work which the discharge performs in producing the disintegration will be less considerable. If instead of the brass disk a mercury surface be used, the mechanical work which the discharge produces in the spark will consist mainly in imparting velocity to the par-
Disjunction-currents between Poles of different shapes.

...articles of mercury in their separation from the surface. To investigate this the following series of experiments were made.

Experiment 25.—The conical brass point previously used was placed vertically over a porcelain dish which was filled with mercury. The mercury was connected by a conducting-wire with the point $e$, and the sharp point with the point $c$ (see figure, p. 15). From this the following results were:

<table>
<thead>
<tr>
<th>Mercury</th>
<th>Mercury</th>
<th>Mercury</th>
</tr>
</thead>
<tbody>
<tr>
<td>positive</td>
<td>negative</td>
<td>positive</td>
</tr>
<tr>
<td>Mean deflections</td>
<td>37.2</td>
<td>43.0</td>
</tr>
</tbody>
</table>

Here, then, the deflection was greater when the mercury was negative; in the foregoing experiments the reverse was the case. Hence the electromotive force of disjunction must have been smaller when sparks were formed between mercury poles than between brass poles. This is confirmed by the following two series of experiments.

Experiment 26.—The conical brass point was removed and replaced by a brass knob 17 millims. in diameter, which, as special experiments showed, acted about the same as a disk of the above magnitude. There were thus obtained in two independent series of observations:

<table>
<thead>
<tr>
<th>Mercury</th>
<th>Mercury</th>
<th>Mercury</th>
</tr>
</thead>
<tbody>
<tr>
<td>negative</td>
<td>positive</td>
<td>negative</td>
</tr>
<tr>
<td>Mean deflections</td>
<td>51.8</td>
<td>35.9</td>
</tr>
</tbody>
</table>

Hence the deflections became considerably smaller when the positive discharge traversed the spark from the mercury surface to the brass knob.

It might be urged against the above experiments, that the mercury does not retain a level surface while the spark passes, but rises in the form of a peak towards the opposite pole. The smaller deflection when the positive discharge goes from the mercurial surface to the knob would, on this supposition, have been caused by the discharge taking place from the peak to the knob. But there was no such elevation of the surface perceptible. Moreover the deflection, even assuming that a point was formed of the same extent as the brass one, could not have been so small as was observed when the discharge took place from the mercury to the knob, unless we also assume that the mercury itself cooperated in making the deflection smaller. These experiments merely show that the electromotive force of disjunction between mercury poles is less than that between brass poles; the true ratio between these forces cannot be ascertained by them. In order to examine what takes place when both poles consist of mercury, the following method was adopted.
Two vessels, provided in the bottom with exit-tubes and stopcocks, were filled with mercury and placed so close together that, when the cocks were opened, the mercury-streams were so near each other that a spark could strike across. The mercury was received in the two separate compartments of a glass vessel placed below. When one vessel was connected with the point $c$ by means of a conducting-wire, and the other with the point $e$, deflections so distinct were obtained, as soon as the machine was set to work, that there could be no doubt in reference to the electromotive property of the mercury in the above respect.

The same experiments were repeated after the mercury had been replaced by water containing sulphuric acid. Although a slight spark, which was very distinct in the dark, passed between the two jets, no evident deflections were observed which could be ascribed to the disjunction-current. Notwithstanding this negative result, I doubt not that water is an electromotor in this respect, although the means at my disposal were not suitable to show this.

5. I have shown in a previous paper that, when induction-currents which result from voltaic induction have the opportunity of traversing the spark of an electrical discharge, those currents which tend to traverse the spark in the same direction as the discharge can most easily penetrate it. The spark acts thus like an electrical valve: of the two opposite induction-currents, that passes in greatest proportion which is in the same direction as the discharge. That the other also passes to some extent may be seen from the appearance of the spark. When the induction-coil is inserted in the circuit between $e$ and $g$ (see figure, p. 15) the spark is duller, and cannot strike across such a distance between the knobs as when the coil is removed—affording a proof that the induction-current, which arises at the commencement of the spark and goes in the opposite direction to the discharge, does in fact partially traverse the spark. In this case, therefore, the intensity of the spark is diminished by the induction; its curve of intensity is, as it were, flattened. When, on the contrary, the induction-coil is placed between $e$ and $k$, the intensity of the discharge increases in consequence of the fact that the induction-current formed at the commencement of the spark now traverses it in the same direction as the discharge; the spark has a greater striking-distance than when the coil is removed; its intensity is increased. Now it might possibly be maintained that the reason why the deflection of the disjunction-current is diminished by the introduction of the induction-coil between $e$ and $g$, or between $e$ and $k$, is not that that induction-current which has the same direction as the discharge traverses the spark in greatest proportion, but that it is to be sought in the fact that the curve
Disjunction-currents between Poles of different shapes.

of intensity of the spark undergoes a change in form, although both induction-currents pass with equal facility. That this cannot be the case can be easily seen from the following considerations.

We assume for a moment that both induction-currents traverse the spark in exactly equal proportions; the electricities which have passed are then exactly equal, whether the induction-coil is in the circuit or not; and therefore it is solely the change in the curve of intensity of the spark when the induction-coil is inserted between e and g or between e and k which causes the diminution in the deflection of the disjunction-current. But it is to be remarked that the intensity of the spark increases when the induction-coil is inserted between e and k, while it decreases when the coil is between e and g; two entirely opposite changes in the intensity would thus both have the same effect—that is, a diminution in the deflection produced by the disjunction-current. This would only be possible if the deflection which the disjunction-current produces when there is no induction were really a maximum, so that a change in the shape of the spark in either direction could not increase the deflection. But that this deflection is not a maximum follows from the fact that the introduction of the induction-coil in all cases diminishes the deflection, whether there is a greater or a smaller distance between the knobs, or whether there is or is not a bridge in front of the galvanometer, whether the condensers have greater or smaller coatings, or whatever be the other circumstances on which the form of the curve of intensity of the spark depends. Hence it follows that the two induction-currents cannot traverse the spark in equal proportion.

But, instead of this, we might say that it is not those induction-currents which traverse the spark in the same direction as the charge which pass most easily and in greatest proportion, but this is the case with the currents which go in the opposite direction to the discharge, and therefore in the same direction as the disjunction-currents. The latter, it is true, are added to the disjunction-current and thereby increase the deflection; but, on the other hand, they produce so great a diminution in the disintegration of the poles, that, on the whole, the deflection is thereby diminished. Many proofs may be adduced for the absurdity of this opinion; but the question is most easily settled by the following experiments.

We assume for the moment that that induction-current which is in the opposite direction to the discharge, or in the same direction as the disjunction-current, most readily penetrates the spark, and, in the manner just described, affects the diminution observed in the deflection. To acquire a better idea of this matter, we
may conceive it as follows:—The induction-current in question produces a disintegration of the polar surfaces; and this gives rise to a disjunction-current which is in the opposite direction to the former disjunction-current, and therefore diminishes the deflection of the magnetic needle. Now, from experiments 22 to 24, we know that the greatest disjunction-current is obtained when the valve has such a position that the discharge which the disintegration causes goes from the disk to the point. Hence it follows that the greatest diminution in the deflection must occur when the induction-current in question goes from the disk to the point, or, what is the same thing, when the discharge goes from the point to the disk. Subsequent experiments show, however, that the fact is quite the reverse, and therefore that the assumption, that that induction-current which is opposite in direction to the discharge is the one which traverses the spark most readily, cannot be correct. In these experiments the brass disk previously mentioned was fastened to one rod of the glass cylinder, and the platinum wire surrounded by a glass tube was firmly screwed to the other rod. The pressure of air in the glass cylinder was 1 atmosphere.

**Experiment 27.**—The valve was first of all so arranged that the positive discharge of the machine went from the wire to the disk. There was thus obtained:

<table>
<thead>
<tr>
<th>Without induction-coil.</th>
<th>Coil between e and k.</th>
<th>Without coil.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean deflections</td>
<td>37·1</td>
<td>6·2</td>
</tr>
<tr>
<td></td>
<td></td>
<td>37·6</td>
</tr>
</tbody>
</table>

The valve was then reversed, so that the discharge went from the disk to the wire:—

| Mean deflections       | 47·4                 | 8·5          |
|                        |                      | 46·2         |

In the former case, then, the induction produces a diminution in the deflection of $31·2 = \left(\frac{37·1 + 37·6}{2} - 6·2\right)$; and in the latter case, of 38·3 divisions. Hence the diminution in the deflection in the first case was not greater, but less than in the latter.

**Experiment 28.**—This experiment was in all respects like the preceding, except that the induction-coil was inserted between e and g. The discharge of the machine went first from the wire to the disk:—

<table>
<thead>
<tr>
<th>Without coil.</th>
<th>Coil between e and g.</th>
<th>Without coil.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean deflections</td>
<td>38·4</td>
<td>5·9</td>
</tr>
<tr>
<td></td>
<td></td>
<td>38·7</td>
</tr>
</tbody>
</table>

The valve reversed, so that the discharge went from the disk to the wire:—

| Mean deflections       | 45·0                 | 5·1          |
|                        |                      | 48·6         |
Disjunction-currents between Poles of different shapes.

This last experiment therefore confirms the preceding. Hence it cannot be the induction-current opposite in direction to the discharge which most readily traverses the spark, but must be the other.

As it is now certain that the induction-current which has the same direction as the discharge most readily traverses the spark, another conclusion can be drawn from the preceding experiments; for they show that the diminution in the deflection of the magnetic needle which the induction-current causes is greater when that current traverses the spark from the disk to the point than when its direction is opposite. An induction-current which traverses a spark does so most readily when it can go from the disk to the point. This result, which holds for the case in which the spark passes in a space filled with air, is essentially the same as that which Professor Riess found for the spark in rarefied air.

6. We now proceed to the case in which the electrical discharge traverses a spiral, and thereby produces induction in an adjacent spiral. When the latter spiral is connected with a galvanometer and its ends are in metallic connexion, no deflection is obtained, because the two induction-currents are equal and in opposite directions. If, on the contrary, the spiral is opened so wide that formation of sparks takes place, the magnetic needle makes a deflection which indicates that the inducing current is in the same direction as the discharge-current. When the spark is formed in a space containing air, this holds, under ordinary circumstances, whatever be the shape of the poles. We really have not less than four currents—that is, two induction-currents and two disjunction-currents. When the galvanometer is inserted in the conduction and not provided with a suitable bridge, the system of currents is still more complex. Both induction-currents have the same electromotive force; and if the spark opposed the same resistance to each, there would be no action upon the galvanometer. As regards the disjunction-currents, their electromotive forces can by no means be equal. The first induction-current, or that which is in the opposite direction to the discharge, must break through a dense layer of air; and as this cannot occur without a more considerable tension of the electricity, a powerful disintegration of the polar surfaces thereby ensues. The second induction-current, or that which goes in the same direction as the discharge, instantaneously follows the first, strikes therefore, in the spark, air already rarefied, and the disintegration is less. On this account the first induction-current must produce the most powerful disjunction-current. This latter current, which goes in the same direction as the second induction-current, produces the deflection of the magnetic needle. Hence the capacity of the first induction-cur-
rent to produce the strongest disjunction-current need not be ascribed to any special property of it, as it is sufficiently explained by the fact that the formation of sparks commences with this current. When the poles are in an enclosed space, from which the air can be exhausted, the electromotive force of the disjunction diminishes in proportion as the air is rarefied. Finally the electromotive force of induction, which does not depend on the density of the layer of air traversed by the spark, begins to be greater than the former, and then the deflection of the magnetic needle mainly depends upon the induction-currents.

Professor Riess* has shown, by means of the electric valve which he has devised, that when this is inserted in the path of an induction-current, the following relations take place when the density of the air and the position of the valve are altered. When the spark is formed under a pressure of one atmosphere, there is obtained on a galvanometer inserted in the circuit a deflection in the same direction as that which would be obtained with the second induction-current. Here, as regards the direction of the deflection, it is immaterial whether the second induction-current goes from the disk to the point, or vice versa. When the current in question goes from the disk to the point and the air is gradually exhausted from the valve, the deflections of the magnetic needle are always in the same direction, but their magnitude gradually diminishes at first, increasing again on subsequent rarefaction. When, on the contrary, the valve is so applied that the second induction-current goes from the point to the disk, the deflection diminishes more rapidly with the rarefaction, and afterwards changes to a deflection towards the opposite side, which increases when the rarefaction is increased.

These details could not well have been sufficiently explained before the discovery of disjunction-currents; but now the explanation follows spontaneously. The deflection obtained when the valve was full of air did not arise, as has been hitherto assumed, from the second induction-current, but from the disjunction-current, which is caused by the first induction-current. When the air is rarefied, the disjunction-current becomes feebler, and the induction-currents begin to have more and more effect; at last they determine the direction of the deflection. Now, from the results of experiments 27 and 28, we know that the induction-current can traverse the spark most readily when it goes from the disk to the point. If, therefore, the valve is so applied that the second induction-current goes from the disk to the point, the direction of the deflection must remain unaltered when the air is

Disjunction-currents between Poles of different shapes. 31

gradually rarefied. But the deflection is not caused during the whole time by the same current: in the unrarefied air the direction of the deflection is chiefly determined by the disjunction-current, and in the rarefied air by the second induction-current. If, on the contrary, the valve is applied so that the first induction-current goes from the disk to the point, this current acquires the upper hand and determines the direction of the discharge when the air is rarefied; hence in this case the deflection must alter its direction when the air is gradually rarefied. In the space filled with air the disjunction-current has the upper hand; in rarefied air the first induction-current is the more powerful.

By the foregoing investigations we have obtained a simple means of experimentally proving whether a given deflection of the magnetic needle is caused by a disjunction- or by an induction-current. Experiments 22 to 24 show that when the spark is formed between the disk and the point, the disjunction-current is most powerful when the discharge goes from the disk to the point, or, what is the same thing, when the disjunction-current goes from the point to the disk. Experiments 27 and 28, on the contrary, have shown that when an induction-current produces a deflection, this is greatest when the induction-current goes from the disk to the point. If, therefore, the current which produces the deflection first goes from the disk to the point, and thereupon by reversing the valve a greater deflection is obtained, it is a case of a disjunction-current; but if the deflection is smaller when the valve is reversed, an induction-current is the cause. This holds, without exception, when the deflection is produced either by a disjunction- or by an induction-current only. When both currents act simultaneously and to the same extent, this rule, as may be easily seen, may, under certain circumstances, be misleading.

M. Riess has already found that, when the valve was full of air, the greatest deflection was obtained when the second induction-current (which, in his opinion, determines the direction of the deflection) went from the point to the disk. This, however, as we have seen, is a proof that the deflection was caused by a disjunction-current.

The subsequent series of experiments confirm this observation, and prove afresh that the deflection in the case in question was caused by a disjunction-current. The expression "Disk positive," signifies that the second induction-current went from the disk to the point; and the expression the "Disk negative" signifies the contrary.
Experiment 29.—

Mean deflections . . 19·6

Disk positive.

Disk negative.

Disk positive.

Experiment 30.—

Mean deflections . . 12·3 19·2 12·2

We can hereby explain the peculiarity that, in that position of the valve which gives the smallest deflection in a space filled with air, the deflection remains unaltered towards the same side when the air in the valve is rarefied.

On a superficial consideration, it may seem absurd to suppose that the disjunction-current can produce upon the magnetic needle an action many times as powerful as the discharge by which it is caused. It might be thought that the direct action of the discharge upon the magnetic needle must be just as great as when this current first produces a disjunction-current which subsequently exerts magnetic action. Yet it may easily be seen that this discrepancy is only apparent. That electricity consists of motion is indubitable; but this presupposes something which moves, whether it is the smallest particles of a body, the æther, or any other body. Now, if the mass set in motion in the electrical discharge be called M, and its velocity V, MV\(^2\) is its *vis viva* in the discharge. If, in like manner, \(m\) denotes the mass in motion in the disjunction-current, and \(v\) its velocity, \(mv^2\) is the *vis viva* of the disjunction-current. This latter quantity cannot be greater than the former, but smaller; for the entire *vis viva* of the discharge never passes to the disjunction-current. If, now, the deflection of the magnetic needle were proportional to the *vis viva* of the current which acts upon it, the deflection which the disjunction-current causes could not possibly be greater than that which the discharge could directly produce; but the action upon the magnetic needle is not proportional to the *vis viva*, but to the intensity; that is, proportional to \(mv\); and this quantity may readily be many times as great as \(MV\), although \(mv^2\) must always be less than, or at most equal to \(MV^2\). If, for instance, \(M = 1\) and \(V = 100\), \(MV^2 = 10000\); if \(m = 10000\) and \(v = 1\), \(MV^2 = mv^2\), but \(mv = 100MV\). In the electrical discharge the mass moved is inconsiderable, but its velocity is large; in the disjunction-current the reverse is the case. By the mechanical work which the discharge performs in the spark, one of these forms of motion is changed into the other.

I will remark, in conclusion, that in my opinion it would be desirable to revise the electrical investigations which were instituted before the discovery of the electrical disjunction-current, and in which electrical sparks and an enclosed circuit occurred. However trustworthy the observations, the explanations could scarcely be either correct or complete when this mode of development of electricity was unknown.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a letter upon "A Theory of Nebulæ and Comets," published in your Magazine for last month, I endeavoured to show that there are grounds for believing that the solid or liquid bodies composing a meteoric band are surrounded by very rare and extensive atmospheres. I assumed that the meteoric bodies are so numerous and the atmosphere of each so extensive, that in the neighbourhood of the sun, and as far from it as the tails of comets are formed, the atmospheres of the different bodies encroach upon one another, and so form a continuous envelope of gaseous matter about the sun. As the tails of comets are known to extend to a much greater distance from the sun than the distance of the earth, we must admit that the earth is moving through this gaseous envelope. Though the matter which forms it is exceedingly rare, yet it must be much condensed in the neighbourhood of a large attracting body like the earth. The question then arises, is there any evidence of the existence of such gaseous matter in our atmosphere?

I think that the spectrum of the aurora borealis indicates the existence of this matter in the higher regions of the atmosphere.

The spectrum of an aurora observed by M. Angström* consists mainly of one bright line not belonging to any known terrestrial substance, besides several very faint atmospheric lines and some faint bands of light. This proves that there exists in the upper regions of the atmosphere a kind of matter not known to exist in an appreciable quantity in the lower strata. Moreover, M. Angström has found the same bright line in the spectrum of the zodiacal light, which shows that this matter is of the same kind as that which exists in the sun's envelope.

From the great superiority in the brightness of this line in the auroral spectrum, compared with the atmospheric lines, we might be led to suppose that the matter to which it is due either exists in larger quantities than the elements of the atmosphere in those regions from which the light comes, or else that the electrical currents render it more luminous than the other matter present.

This, however, is not necessarily the case; for the light due


to the luminosity of the atmospheric elements will, in its passage through the non-luminous lower strata of the atmosphere, be very greatly diminished, owing to absorption; whilst the light due to any substance not present in the lower strata will be almost wholly transmitted to the eye. Thus the relative intensity of the rays due to the different kinds of matter will, when they reach the eye, be very different from their relative intensity before their passage through the atmosphere*.

The absence of a corresponding dark line in the solar spectrum shows, I think, that the quantity of the gaseous matter is very small; and this, taken in conjunction with the brightness of the line in the auroral spectrum, shows that it is confined to the higher parts of the atmosphere.

Auroras are known to occur in the lower parts of the atmosphere, in the region of the clouds; and some have been observed so low as to appear against a mountain as a background. It would be interesting to know whether such auroras would exhibit the same spectrum as that observed by M. Ångström.

M. Ångström asserts, as a deduction from his observations, that the aurora is not due to electricity. The only other way in which it seems possible to account for luminous matter in the atmosphere is by supposing chemical action to be taking place, and, as all chemical action between the constituents of the atmosphere must have ceased long ago, if ever there was any chemical affinity between them, we must suppose that this chemical action is taking place between the elements of the atmosphere and the newly introduced gaseous matter. I do not, however, see that M. Ångström's observation affords any ground for believing that the aurora is not the light due to electrical discharges.

Roundhay Vicarage.
June 10, 1870.

A. S. Davis.

* I wish here to point out that the light from a nebula may be due to only one of the different gases composing it; for the light due to the other gas may be wholly absorbed in passing outwards through its non-luminous portions. The presence of lines in the spectrum of a nebula due only to one kind of matter, is therefore no proof that it is not composed of more than one kind of matter.
V. On the Solution of Linear Partial Differential Equations of the Second Order involving two Independent Variables. By R. Moon, M.A., Honorary Fellow of Queen's College, Cambridge*

The following method of treating the problem offered to us in the solution of the General Linear Partial Differential Equation of the Second Order, in the cases which are not amenable to Monge's method, may be found to possess both interest and value.

Let

\[
0 = R \frac{d^2z}{dx^2} + S \frac{d^2z}{dxdy} + T \frac{d^2z}{dy^2} + P \frac{dz}{dx} + Q \frac{dz}{dy} + Uz + V, \tag{1}
\]

where \(R, S, T, P, Q, U, V\) are functions of \(x\) and \(y\) only; and assume

\[
z = \bar{A} + \Lambda \phi(u) + \Lambda_1 \phi^{-1}(u) + \Lambda_2 \phi^{-2}(u) + R, \&c., \tag{2}
\]

where \(u, \bar{A}, A, A_1, A_2, \&c.\) are functions of \(x\) and \(y\) only, and

\[
\phi^{-1}(u) = \int \phi(u) \, du, \quad \phi^{-2}(u) = \int \int \phi(u) \, du^2, \quad \&c. \quad \&c.
\]

Our assumption gives us

\[
\frac{dz}{dx} = \frac{d\bar{A}}{dx} + \Lambda \frac{du}{dx} \phi(u)
\]

\[
+ \left( A_1 \frac{du}{dx} + \frac{d\Lambda}{dx} \right) \phi(u) + \left( A_2 \frac{du}{dx} + \frac{d\Lambda_1}{dx} \right) \phi^{-1}(u) + \&c.
\]

\[
\frac{dz}{dy} = \frac{d\bar{A}}{dy} + \Lambda \frac{du}{dy} \phi(u)
\]

\[
+ \left( A_1 \frac{du}{dy} + \frac{d\Lambda}{dy} \right) \phi(u) + \left( A_2 \frac{du}{dy} + \frac{d\Lambda_1}{dy} \right) \phi^{-1}(u) + \&c.
\]

\[
\frac{d^2z}{dx^2} = \frac{d^2\bar{A}}{dx^2} + \Lambda \frac{d^2u}{dx^2} \phi''(u)
\]

\[
+ \left( A_1 \frac{d^2u}{dx^2} \right)^2 + 2 \frac{d\Lambda}{dx} \frac{du}{dx} + \Lambda \frac{d^2u}{dx^2} \phi(u)
\]

\[
+ \left( A_2 \frac{d^2u}{dx^2} \right)^2 + 2 \frac{d\Lambda_1}{dx} \frac{du}{dx} + \Lambda_1 \frac{d^2u}{dx^2} + \frac{d^2\Lambda_1}{dx^2} \phi(u)
\]

\[
+ \left( A_3 \frac{d^2u}{dx^2} \right)^3 + 2 \frac{d\Lambda_2}{dx} \frac{du}{dx} + A_2 \frac{d^2u}{dx^2} + \frac{d^3\Lambda_2}{dx^2} \phi^{-1}(u)
\]

\[
+ \&c.
\]

* Communicated by the Author.
\[ \frac{d^2 z}{dx dy} = \frac{d^2 \overline{A}}{dx dy} + A \frac{du}{dx} \frac{du}{dy} \phi''(u) \]
\[ + \left( A_1 \frac{du}{dx} \frac{du}{dy} + \frac{dA}{dx} \frac{du}{dy} + \frac{dA}{dy} \frac{du}{dx} + A_1 \frac{d^2 u}{dx dy} \right) \phi'(u) \]
\[ + \left( A_2 \frac{du}{dx} \frac{du}{dy} + \frac{dA_1}{dx} \frac{du}{dy} + \frac{dA_1}{dy} \frac{du}{dx} + A_2 \frac{d^2 u}{dx dy} + \frac{d^2 A_1}{dy dx} \right) \phi(u) \]
\[ + \left( A_3 \frac{du}{dx} \frac{du}{dy} + \frac{dA_2}{dx} \frac{du}{dy} + \frac{dA_2}{dy} \frac{du}{dx} + A_3 \frac{d^2 u}{dx dy} + \frac{d^2 A_2}{dy dx} \right) \phi^{-1}(u) \]
\[ + \text{&c.} \]

and we shall have an expression for \( \frac{d^2 z}{dy^2} \) identical with that for \( \frac{d^2 z}{dx^2} \) when in the latter we put \( y \) for \( x \).

Substituting these values in (1) and equating to zero the coefficients of \( \phi''(u), \phi'(u), \phi(u), \phi^{-1}(u), \&c. \), we get

\[ 0 = R \frac{d^2 \overline{A}}{dx^2} + S \frac{d^2 \overline{A}}{dx dy} + T \frac{d^2 A}{dy^2} + P \frac{dA}{dx} + Q \frac{dA}{dy} + U \overline{A} + V, \quad (3) \]

\[ 0 = R \frac{d_A}{dx} + S \frac{d_A}{dx dy} + T \frac{d_A}{dy dy}, \quad \ldots \ldots \ldots \ldots \quad (4) \]

\[ 0 = \left( 2R \frac{du}{dx} + S \frac{du}{dy} \right) \frac{dA}{dx} + \left( S \frac{du}{dx} + 2T \frac{du}{dy} \right) \frac{dA}{dy} \]
\[ + \left( R \frac{d^2 u}{dx^2} + S \frac{d^2 u}{dx dy} + T \frac{d^2 u}{dy^2} + P \frac{du}{dx} + Q \frac{du}{dy} \right) A, \]

\[ 0 = \left( 2R \frac{du}{dx} + S \frac{du}{dy} \right) \frac{dA_1}{dx} + \left( S \frac{du}{dx} + 2T \frac{du}{dy} \right) \frac{dA_1}{dy} \]
\[ + \left( R \frac{d^2 u}{dx^2} + S \frac{d^2 u}{dx dy} + T \frac{d^2 u}{dy^2} + P \frac{du}{dx} + Q \frac{du}{dy} \right) A_1 \]
\[ + R \frac{d^2 A}{dx dy} + S \frac{d^2 A}{dy dy} + T \frac{d^2 A}{dy^2} + P \frac{dA}{dx} + Q \frac{dA}{dy} + UA, \]

\[ 0 = \left( 2R \frac{du}{dx} + S \frac{du}{dy} \right) \frac{dA_2}{dx} + \left( S \frac{du}{dx} + 2T \frac{du}{dy} \right) \frac{dA_2}{dy} \]
\[ + \left( R \frac{d^2 u}{dx^2} + S \frac{d^2 u}{dx dy} + T \frac{d^2 u}{dy^2} + P \frac{du}{dx} + Q \frac{du}{dy} \right) A_2 \]
\[ + R \frac{d^2 A_1}{dx dy} + S \frac{d^2 A_1}{dy dy} + T \frac{d^2 A_1}{dy^2} + P \frac{dA_1}{dx} + Q \frac{dA_1}{dy} + UA_1, \]
\[ \&c. \quad \&c. \]
If \( a_1, a_2 \) be the roots of

\[
0 = Ra^2 + Sa + T,
\]

we shall have

\[
0 = \frac{du}{dx} - a_1 \frac{du}{dy}, \quad 0 = \frac{du}{dx} - a_2 \frac{du}{dy}.
\]

Hence if the integrals of the equations

\[
0 = dy + a_1 dx, \quad 0 = dy + a_2 dx
\]

be respectively

\[
\omega _1 = \text{const.}, \quad \omega _2 = \text{const.},
\]

\( \omega _1 \) or \( \omega _2 \) may be substituted at pleasure for \( u \) in (2).

The equations which succeed (4) may be written

\[
0 = \frac{dA}{dx} + \alpha \frac{dA}{dy} + \beta A,
\]

\[
0 = \frac{dA_1}{dx} + \alpha \frac{dA_1}{dy} + \beta A_1 + \gamma _1, \quad \ldots \quad (5)
\]

\[
0 = \frac{dA_2}{dx} + \alpha \frac{dA_2}{dy} + \beta A_2 + \gamma _2,
\]

\&c. \quad \&c.

The integral by Lagrange's method of any one of these equations, as (5), will be of the form

\[
A_1 = L \cdot \chi (\omega) + M,
\]

where \( L, M \) are functions of \( x \) and \( y \); \( \chi \) is arbitrary; and \( \omega = \text{const.} \) is the integral of

\[
0 = dy - adx.
\]

Hence if these values were substituted in (2), and we then put \( \phi (u) = \) a constant, as we may do, we should have the given equation (1) satisfied by an integral of the form

\[
z = F \{ x, y, \chi (\omega) \},
\]

where \( F \) is a definite, and \( \chi \) an arbitrary function; from which it would inevitably follow that the given equation is soluble by Monge's method, whereas by hypothesis it is not so soluble.

We must assume, therefore, the arbitrary function in each case to be zero; so that, in order to find \( A, A_1, \&c. \), we may take the equations
\[ 0 = \frac{dA}{dx} + \beta A, \quad \ldots \quad (6) \]

\[ 0 = \frac{dA_1}{dx} + \beta A_1 + \gamma_1, \quad \ldots \quad (7) \]

\[ 0 = \frac{dA_2}{dx} + \beta A_2 + \gamma_2, \]

\&c. \quad \&c.

where

\[
\beta = \frac{R \frac{d^2u}{dx^2} + S \frac{d^2u}{dy^2} + T \frac{d^3u}{dy^2} + P \frac{du}{dx} + Q \frac{du}{dy}}{2R \frac{du}{dx} + S \frac{du}{dy}},
\]

\[
\gamma_1 = \frac{R \frac{d^3A}{dx^2} + S \frac{d^2A}{dy^2} + T \frac{d^3A}{dy^2} + P \frac{dA}{dx} + Q \frac{dA}{dy} + UA}{2R \frac{dA}{dx} + S \frac{dA}{dy}},
\]

\[
\gamma_2 = \frac{R \frac{d^2A_1}{dx^2} + S \frac{d^2A_1}{dy^2} + T \frac{d^3A_1}{dy^2} + P \frac{dA_1}{dx} + Q \frac{dA_1}{dy} + UA_1}{2R \frac{dA_1}{dx} + S \frac{dA_1}{dy}},
\]

\&c. \quad \&c.

(6) gives us

\[ \Lambda e^{\int \beta dx} = c, \]

where \( c \) is constant, it being understood that, if \( \beta \) contains \( y \), the latter variable must be eliminated before the integration by means of the equation

\[ \omega = \text{const.}, \]

and the constant so introduced must be eliminated after integration by means of the same equation.

Hence we shall have

\[ \Lambda = c e^{-\int \beta dx}, \]

\[ \Lambda_1 = c_1 e^{-\int \beta dx} e^{-\int \beta dx} \int dx \cdot \gamma_1 e^{\int \beta dx}, \]

\[ \Lambda_2 = c_2 e^{-\int \beta dx} e^{-\int \beta dx} \int dx \cdot \gamma_2 e^{\int \beta dx}, \]

\&c. \quad \&c.

It is clear, however, that the substitution of these values would give rise to a term in \( z \)

\[ = e^{-\int \beta dx} \cdot \{c\phi(u) + c_1 \phi^{-1}(u) + c_2 \phi^{-2}(u) + \&c.\}, \]

\[ = e^{-\int \beta dx} \cdot \varphi(u); \]
so that, in effect, we may in the above values of \( A, A_1, \&c. \) put \( c=1 \), and \( c_1=c_2=\&c.=0 \); \( \therefore \) we shall have

\[
A = e^{-\int \beta dx},
\]
\[
A_1 = -e^{-\int \beta dx} \int dx \cdot \gamma_1 e^{\int \beta dx},
\]
\[
A_2 = -e^{-\int \beta dx} \int dx \cdot \gamma_2 e^{\int \beta dx},
\]
\&c. \&c.

as to which it is to be observed that no constants are to be introduced in the integrations, all such having been accounted for.

Take the case where \( R=1 \), and \( S \) and \( T \) are constant, and we get

\[
u = y + a_1 x, \text{ or } u = y + a_2 x;
\]

the first of which values gives

\[
\beta = \frac{P a_1 + \phi}{a_1 - a_2}.
\]

If we take

\[
\frac{P a_1 + \phi}{a_1 - a_2} = -\frac{h}{x},
\]

we shall have

\[-\int \beta dx = \int \frac{h}{x} dx = \log x^h; \]

\[
\therefore \ A = x^h.
\]

And the substitution of this value in (8) gives us

\[
\gamma_1 = \frac{h(h-1)x^{h-2} + Phx^{h-1} + Ux^h}{a_1 - a_2}
\]

\[
= \frac{k + h(h-1)}{a_1 - a_2} x^{h-2},
\]

if we assume

\[
P h + U x = \frac{k}{x},
\]

where \( k \) is constant;

\[
\therefore \ A_1 = -x^h \int dx \cdot e^{-h\gamma_1}
\]

\[
= \frac{k + h(h-1)}{a_1 - a_2} \cdot \frac{x^{h-1}}{1};
\]

Also

\[
\gamma_2 = -\frac{k + h(h-1)}{a_1 - a_2} \cdot \frac{(h-1)(h-2) + P(h-1)x + Ux^2}{1};
\]

\[
= -\frac{k + h(h-1)}{a_1 - a_2} \cdot \frac{(h-1)(h-2) + k - Px}{1};
\]

\[
= -\frac{k + h(h-1)}{(a_1 - a_2)^2} \cdot \frac{k + (h-1)(h-2) - l}{1};
\]
On Linear Partial Differential Equations of the Second Order.

if $P = \frac{l}{x}$, where $l$ is constant;

$$A_2 = \frac{(k + h(h - 1)) \{k + (h - 1)(h - 2) - l\}}{(a_1 - a_2)^2} \cdot \frac{x^{h-2}}{1 \cdot 2}.$$ 

Similarly we shall arrive at

$$A_3 = \frac{(k + h(h - 1)) \{k + (h - 1)(h - 2) - l\} \{k + (h - 1)(h - 2) - 2l\}}{(a_1 - a_2)^3} \cdot \frac{x^{h-3}}{1 \cdot 2 \cdot 3}$$

&c. &c.

$$A_r = \frac{(k + h(h - 1)) \{k + (h - 1)(h - 2) - l\} \ldots \{k + (h - r + 2)(h - r + 1) - (r - 1)l\}}{(a_1 - a_2)^r} \cdot \frac{x^{h-r}}{1 \cdot 2 \ldots r}.$$ 

If $h$ is fractional or negative, we shall, on the particular assumptions above introduced, always have an integral of the assumed form, the number of terms being finite or infinite according to circumstances, though as to the practical value of the integral so obtained in the latter case I am not prepared to express an opinion.

The condition to be satisfied in order that $A_r$ may vanish and that the expression for $z$ may have a finite number of terms when $h$ is not a positive integer, is, that we have

$$0 = k + (h - r + 2)(h - r + 1) - (r - 1)l,$$

the only conditions limiting the quantities $h, k, l, r$ being that they are all constant, and that $r$ is, and $h$ is not, a positive integer.

When $h$ is a positive integer and $r = h$, we shall have $A_r$ constant; whence it follows that upon this supposition the series will always terminate when $U = 0$.

The well-known equations

$$0 = \frac{d^2z}{dx^2} - a^2 \frac{d^2z}{dy^2} - \frac{n(n + 1)}{\varepsilon^2} z,$$

$$0 = \frac{d^3z}{dx^2} - a^2 \frac{d^2z}{dy^2} - \frac{2n}{x} \frac{dz}{dx}$$

are readily solvable by the foregoing method.

It remains to be remarked with respect to the first term of the series for $z$, that since (3) is of precisely the same form as (1), any value assigned to $A$ must be different from any derivable from the general expression for $z$—as, for instance, a solution obtained upon a particular hypothesis not necessarily implied by
the terms of the problem submitted to us—for example, on the assumption that one of the partial differential coefficients $p, q, r, s, t$ vanishes.

6 New Square, Lincoln’s Inn,  
June 20, 1870.

VI. On a Simple Method of Constructing high Electrical Resistance. By Samuel E. Phillips, Jun.*

At present resistance-coils are mostly made with German-silver wire; and a set of coils equalling 10,000 B.A. units forms a box of convenient size. But in electrical research resistances of several million units are often useful; and to produce such with wire in the ordinary way would be both expensive and cumbrous.

Latterly Mr. Hockin has used selenium for this purpose. Fine glass tubes with a platinum wire blown in at each end and filled with different fluids, according to the resistance required, have also been largely used; but these latter necessarily give very variable results, owing to polarization and electrolysis; and the former, I believe, are somewhat difficult to construct.

Requiring a high resistance for some experiments, I made one as follows:—Upon a strip of vulcanite, 6 inches long by 1 inch wide, I ruled several pencil lines with an ordinary H.B. pencil in such a manner as to produce a continuous line about $\frac{3}{16}$ of an inch wide and 4 inches long. At the extremities of the line I rubbed the pencil plentifully over a space as large as a six-penny piece, upon which I firmly screwed two binding-screws by means of “nuts” underneath, and, gently dusting off all superfluous plumbago, varnished the whole with several coats of pure shellac varnish.

The above arrangement gave me a resistance of slightly over two million B.A. units. It was constructed three months ago, and up to the present time the resistance has remained very constant. I have tested it repeatedly with 100, 200, and 300 cells, and have always obtained the same result within very small limits. It is also beautifully steady with prolonged battery contact.

Mr. G. Preece has kindly tested a resistance made in the above manner for me, and finds it very constant, only getting an alteration of about 0.5 per cent. for 5°C F. I have mounted a vulcanite slab with twenty binding-screws, giving a wide range of varied resistances by combination or otherwise, and hope shortly to make some experiments with the view of determining the ratio of its alteration by difference of temperature.

* Communicated by the Author.
VII. Researches on the Electrical Discharge.

By Professor von Bezold*.

In the course of the further investigation on the connexion I have recently described† as existing between the mode of discharge and the character of the dust-figures thereby produced, I strongly felt the necessity of producing the phenomena in question by a more simple apparatus than Ruhmkorff's.

The first experiments with charged Leyden jars, as well as with the ordinary electrical machine without condensing-arrangements, soon showed that with these means only simple figures (that is to say, discharges) could be obtained.

The observation of the spark is sufficient to prove that the discharge which, with a good conducting circuit only interrupted by a break, is alternating, is changed into a simple one by inserting a test-plate‡; for while in the first case the spark is brilliantly luminous, it appears in the second only as a narrow purple line with a bright point towards the positive electrode.

In order, therefore, to obtain alternating discharges even when the test-plate was interposed, no way was left but the use of a suitable branch or return conductor.

If this conductor (which goes to earth) is continuous (that is, nowhere interrupted by a break), it is to be expected that the discharge of the conductor through which the electricity is led to the plate will ensue directly after the charge,—that is, that in this conductor one or more alternations of electricity will take place.

In experiments made with such return conductors, various entirely new and surprising facts were observed which seem suited to serve as starting-points for new inquiries.

But, before I begin the description of these new facts, I must first mention a simple experiment, which indeed teaches nothing essentially new, but yet contributes materially to the understanding of the following.

If the otherwise insulated coating of the test-plate is placed in conducting communication with the source of electricity, while the needle which at other times serves as conductor is connected with the earth, a positive discharge upon the glass surface produces a negative figure, and conversely.

If the coating be perfectly insulated, while two conductors (A

* Communicated by the Author, having been read before the Bavarian Royal Academy of Sciences, February 5, 1870.
‡ By test-plate (Probeplatte) I shall in the sequel mean the plate, coated on one side, on which the figures are formed.
and B) are placed upon the upper uncoated surface, one of which is connected with the source of electricity Q (fig. 1), and the other by a wire E with the earth, at each discharge a positive and a negative figure will be simultaneously formed.

These experiments teach that a positive (negative) figure is obtained when either positive (negative) electricity is added, or negative (positive) taken away.

§ 1. This being premised, the above-mentioned experiments shall now be described. One of the first was made according to the following scheme (fig. 2). From the positive conductor of an electrical machine a wire was led to one knob of a spark-micrometer F. From the other knob two wires conducted, one (E) directly to the earth, the other (D) to the conductor A. The lower coating of the plate was also connected with the earth by means of the wire E'. In my opinion, two kinds of results were by this means to be expected. For it was conceivable that either no figure at all would be formed upon the plate, and the whole of the electricity would be immediately conveyed to the earth by the good conducting-wire, or that at most a small part would reach the plate and then again pass back through E to the earth. I therefore expected either no figure at all, or a small positive compound figure—that is, a yellow star with a red spot.

The result was nevertheless the exact opposite. A figure was formed; it was not positive, however, but negative, a red irregular jagged ring with a yellow radiating centre.

Hence the discharge had not only not divided at the two branches, but the electricity, flowing to the earth in the shortest way through E, took with it electricity of the same kind from the branch A E'.
also the circumstance that it was not always unequivocally successful (for the figures were at times scarcely perceptible) made it desirable to repeat the experiment with another source of electricity. The electrical machine was therefore replaced by the inductorium, one pole being connected with the spark-micrometer and the other with the earth. The knobs of the electrometer were gradually moved apart.

As long as the striking-distances were small, figures were formed which were of the same kind as that of the electricity passing across the break; that is, when the negative pole of the inductorium was connected with the micrometer, negative figures were formed, and vice versa. But as the striking-distance became greater, the diameter of these figures diminished. While, for instance, in one series with a striking-distance of 1 millim. negative figures of about 15 millims. diameter appeared, when the striking-distance was 10 millims. this diameter diminished to 2 millims. On continuing to increase the distance between the knobs, the figures ceased for a while, until when the striking-distance was more than 15 millims. they again occurred, and were decidedly positive in character.

Hence there was here a complete transformation of phenomena. While with small striking-distances the path of the current is that represented in fig. 2 by the dotted arrows, with greater striking-distances another path appears, denoted by the perfect arrows.

Working with positive electricity, positive figures are first obtained, which, when the distance is increased, continually diminish, then disappear for awhile, and are ultimately replaced by negative ones. The transformation first occurs in this case with greater striking-distances than is the case in working with negative electricity.

These, as well as many similar differences in the phenomena, according to the kind of electricity used, doubtless owe their origin to the circumstance that equally intense discharges of the two electricities produce figures of entirely different sizes. Hence also it may arise that, so frequently, alternating discharges of a decidedly negative character* produce figures which at first sight might be taken for positive, while the converse never occurs. For if we imagine a negative and a positive discharge passed successively to the same position on the plate, the former must far exceed the latter in intensity if it be not concealed by traces of the latter.

Yet though there are so many minute points to be discussed

* By an alternating discharge of a positive character I mean one in which the algebraic sum of the quantities of electricity discharged is positive, and vice versa.
in reference to this experiment, what has been communicated shows sufficiently that in electrical currents phenomena may occur similar to those observed in the motion of liquids and named suction-phenomena, practically applied, for instance, in Giffard's injector, or in the well-known inhalation-apparatus.

§ 2. These curious observations gave rise to further experiments on the division of discharge-currents.

Here also alternating discharges gave more constant results than simple ones, and care was therefore always taken to procure a suitable return-current. The above experiments prove that a single wire cannot serve for this purpose; and therefore the secondary coil of a Ruhmkorff's instrument was used (fig. 3).

Fig. 3.

When now the electrical machine was slowly worked, until a spark passed, the compound positive figures appeared on the plate in great regularity.

When the current was branched off by a short wire D, and the branch current also led to the plate by a conductor B, two perfectly like figures appeared, as was to be expected. When, on the contrary, the branch wire had a length at all considerable (something more than a yard), the figures exhibited a decided difference in magnitude; for as soon as the length of the wire exceeded this limit the figure at B was always greater than that at A, even when the branching commenced quite near the end of the conducting-wire (1 centim. above the plate). As the branch wire D was lengthened, the difference in size of the two figures became more striking, until, when D = 6·4 millims. and F = 4·0 millims. (F being the striking-distance), the figure at A was reduced to a small star, and many times was not even formed.

This experiment obviously shows that Ohm's laws hold for stationary currents but not for the electrical discharge, as indeed all theoretical investigations have hitherto shown; for while no electricity at all passes to the plate by the very short branch A,
it takes, at any rate apparently, the path, of many hundred times that length, through the wire $D$.

If the wire $D$ be still more lengthened, the phenomenon at first remains, within tolerably wide limits, unchanged; and not until the length has nearly doubled does the figure at $A$ become again larger, until with still more considerable lengths the difference in magnitude of the two figures again quite disappears. In this case it was immaterial whether a thick or a thin, a good or a bad conducting-wire was used, or whether it was tied in a tight knot or made to describe an arc of a circle. I have not yet worked with spirals.

The phenomenon being so entirely novel, I thought it would be interesting to investigate the deportment of the wire $D$ in various positions. Hence an alteration was made which is represented in fig. 4. The conductors $A$, $B$, $C$ are placed upon the plate and are connected with each other by two wires, $D$ and $D'$. If, now, the lengths are so chosen that at $C$ as large, and at $A$, on the contrary, as small a figure as possible is formed, the figure at $B$ is larger than that at $A$ and smaller than that at $C$. But if the length of the wire is more considerable, the sizes of the figures $A$ and $C$ become more nearly equal, while $B$, with a suitable choice of the ratio $D : D'$, becomes very small or even quite disappears. With a striking-distance of 4·3 millims., and the lengths $AF = 50$ centims., $D = 6·2$ metres, $D' = 8·1$ metres, the figures at $A$ and $C$ were large, while at $B$ very small stars only appeared.

If any one of the conductors be raised from the plate, the figures at the other conductors will not be in the least altered.

This experiment teaches the new fact that the connexion of the conductor with a blind-ending wire is sufficient materially to alter the figure formed at the conductor, or to make it disappear. The experiment becomes most instructive when close to the conductor $A$ a second spark-micrometer ($f$, fig. 5) is introduced, one of whose knobs is connected with $A$, while the other leads to the wire $D$ (fig. 5). If, then, the spark-micrometer $f$ be first of all adjusted for a great distance and this distance be gradually diminished, it will be seen how from the moment the spark passes at $f$ the figure at $A$ becomes different.
—relatively disappears. But if we consider that with alternating discharges the wire D is again immediately discharged, it follows that in such a process electricity first passes into the extreme end of the wire D and is again immediately expelled—that, in short, motions take place which are perfectly comparable to a reflection.

This consideration leads to an hypothesis on the peculiar changes in magnitude which the dust-figures undergo with the branchings described; for if electrical waves are driven into a wire, and after reflection at its end return the same way, the progressing must interfere with the reflected waves and produce phenomena which are analogous to those observed with organ-pipes. The observations hitherto communicated exhibit this analogy in a high degree; and we may well venture to compare the places in which maximum and minimum figures appear with vibrating loops and nodes.

The hypothesis that we have here to do with phenomena of interference acquires probability from the circumstance that the experiments only succeed satisfactorily with alternating discharges, while with simple discharges differences in magnitude of the various figures are observed, but not, by far, to the same extent.

§ 3. A slight modification was made of the above-described experiments, which formed another starting-point for new investigations. If the end of the wire D (fig. 3) be again connected with the first conductor A as represented in fig. 6, with a suitable length of the wire, the figure can also be made to disappear. This experiment formed really the starting-point for all those I have previously mentioned; yet I have deferred its description till now, because it is not suited to facilitate the comprehension of the above experiments. I thought at first I had met with an analogue of Savart's interference-experiment for sound-waves, and supposed the path of the current to be that represented by the dotted
Prof. von Bezold's Researches on the Electrical Discharge.

The experiments with the blind-ending wire, as well as the circumstance that the distance of the two branching-points upon A exerted no decided influence, necessarily militated against this view. To remove all doubt as to this, I made several breaks in the wire, so as to form a succession of sparks. The knobs of this second micrometer were here approached to within a distance of from 0.01 to 0.03 millim.; for I thought that in the case in which the current rushes from both sides into the wire there must be a place where both sets of waves meet. When the micrometer is placed just at this point, the tension on both sides must simultaneously reach the same height, and there is therefore no reason for the formation of a spark here, while one may be expected in all other positions.

The spark indeed failed to appear when the micrometer was inserted in the middle of the loop, and reappeared as soon as it was moved by only a few decimeters on either side. It is thus proved that the path of the current is represented by the perfect arrows; and, on the other hand, the small retardation which the electrical discharge-current experiences by traversing a wire of a few decimetres is made visible.

I first of all sought the conditions under which this experiment on retardation succeeds most strikingly. I found it best to use directly the discharge of a Ruhmkorff's apparatus, on the plan represented in fig. 7. The inducing current was produced...
oy a Grove's element, and the distance in the spark-micrometer was made = 2 millims., as neither larger nor smaller distances gave such good results.

Under these circumstances it was sufficient, in order to produce a spark, if one wire D was even only 1 decimeter longer than the other. When, on the contrary, they were of the same length, a spark never appeared. Yet it can be instantaneously evoked if, by touching one of the wires with the knob of a Leyden jar, the symmetry of the two current-paths is disturbed.

In these experiments also the material and thickness of the wire exerted not the smallest influence. Whether I used a silvered copper wire of 0·06 millim. diameter, or an iron wire of 0·23, or a copper wire of 0·8 millim. diameter, the spark never appeared when both wires were of the same length. Hence the velocity of the propagation of electricity is the same for all stretched wires.

Yet in the form above described, the experiment is not very striking, as we can only work with very small distances in the accessory micrometer. I endeavoured therefore to alter it in such a manner that it would be visible to a whole audience.

Experiments with small Geissler's tubes have led to no decisive result. On the contrary, with lengths of some metres at least, the retardation may be very beautifully shown in the following manner:

![Diagram](image)

If a discharge (negative) (best of all, of a Ruhmkorff's apparatus) be divided, as above, just behind the spark-micrometer into two branches, and if one of them be connected with the coating of the perfectly insulated test-plate, while the other is led by the conductor A to the upper uncoated surface, a positive, a negative, or no figure at all can be made to appear, according as the upper branch is larger, smaller, or as long as the lower one. Indeed the experiments must succeed one another in a definite order if they are intended to support the opinion that they owe their origin to differences in time. For if we remember that it is immaterial whether positive electricity be imparted to the plate

* Spirally-coiled wires will, it may be presumed, give a different result.

or negative be extracted, it will be understood that a positive charge produces a positive figure when the electricity reaches the point of the conductor before it reaches the coating—that is, when $D_1$ is shorter than $D_2$. If, on the other hand, the discharge reaches the coating first, the conductor will be traversed by the induced electricity in the opposite direction; and hence a negative figure must be formed upon the glass surface when $D_2$ is shorter than $D_1$. In the course of the motion this induction discharge must meet, in the wire $D_1$, the electricity coming direct from $F$, and thereby a compound character will be imparted to the figure.

Between these two arrangements with entirely opposite results there must obviously be some in which no figures are formed, as there is no reason why one should be formed in preference to the other. This must be the case when the electricities from both sides arrive simultaneously—that is, when $D_1$ and $D_2$ are of the same length*.

The experiments completely fulfilled these theoretical anticipations. With either kind of electricity, figures of both kinds are obtained when the lengths of the wires are rightly chosen.

To many a one who makes the experiment under not quite favourable circumstances this statement may appear incorrect, apart from the case in which, owing to perfect equality of the two branches, no figures at all are formed; for it may occur that the whole of the figures seem at first sight positive, whatever be the circumstances and whatever be the kind of electricity worked with.

The cause lies simply in the circumstance that the compound negative figures belong in this case to that group which have already a strongly positive character, and even, at first, can scarcely be recognized as negative; but the considerable difference in magnitude which occurs after a change of poles is sufficient at once to remove any doubt as to the true nature of the figures, and to prove the agreement of the experiments with theoretical anticipations.

To sum up, the following results were obtained:

1. *When an electrical discharge, after traversing a spark-interval, is offered two paths to the earth (a short one, and a long one interrupted by a test-plate), with small striking-distances the discharge is divided. With greater distances the electricity takes only the shorter path, and even carries with it electricity of the same kind from the other branch.*

2. *If electrical waves be sent into a wire insulated at the end,*

* There may probably in this case be a small difference in favour of the upper wire, since the electricity coming from below has to spread over the entire coating.
they will be reflected at that end. The phenomena which accompany this process in alternating discharges appear to owe their origin to the interference of the entering and reflected waves.

3. An electrical discharge travels with equal rapidity in wires of equal length, without reference to the materials of which these wires are made.

VIII. On the Interchangeability of Heat and Mechanical Action.
By the Rev. J. M. Heath*.

The doctrine of the equivalence of heat and mechanical action, and that of the conservation of energy, are the expression of one and the same thing to those who believe that heat is motion, and therefore itself a form of force or energy. They both alike express this—that when the action of force, continued through a space, results in motion or heat, or vice versa, there has been a true conversion, a change of one form into another, and that when no motion results there is no conversion. The selfsame expression \( \int f ds \) expresses indifferently the pressure accumulated at any point in a fluid mass by the action of all the particles situated upon a given line upon it, or the accumulation of the same force upon a single particle which should move through the line for which the integral is taken. But the results are not identical. The forces which accelerated the moving particle have done their work and are extinguished; they now exist only in the form of the motion they have created. But the corresponding fluid pressures have done no work (if the creation of motion is work), and have never become any thing else than the pressures they were at first.

If we bear this in mind, the great problem of the science of thermodynamics (how much out of a given gross amount of force \( P \) applied as a load to the piston of a gas-chamber will generate its mechanical equivalent in heat or motion) becomes one of extreme simplicity. The force \( P \) divides itself into two parts, \( p = \) the pressure of the gas below employed in neutralizing the resistance opposed by the gas to motion, and \( P - p \) the remainder, which is wholly effective in producing motion. The separate functions which these two portions of the force respectively discharge are given by the two equations

\[
\int (P-p) \, dv = \frac{1}{2} \Sigma m v^2 + C \quad \text{and} \quad \int (p-p) \, dv = \text{const.},
\]

where \( v \) is the volume of the gas. And it appears very obvious that the first of these is the answer to the question, How much of the whole force \( P \) is converted into motion or heat?

* Communicated by the Author.
But this conclusion is now generally ignored, or rather set aside, in favour of a very different one, founded, as I am forced to think, upon a misconception of the very important elementary question in kinetics, How is a weight lifted up? The quantity of heat gained or lost, we are told, in the supposed case depends upon the work expended upon its generation, or upon that done by its destruction. In the first case, if the piston descends through the space $\delta v$, the work expended is $\int P\,dv$ or $P\delta v$. In the second case, if the gas drives the piston up before it, the work done is $\int_{v}^{v+\delta v} p\,dv$. In which it may be observed, en passant, that the constancy of $P$ affects the form of the result very materially when it descends and causes condensation, but has no influence upon it when it is raised. But the far more serious objection to the doctrine is, that weight raised or resistance overcome is precisely that kind of work done by a force in which no conversion into heat or motion takes place at all; and it excludes the only case in which such conversion does take place, which, as we have shown, is that in which force acting upon matter free to move, itself passes into motion. From the second of the above equations, $\int (p-p)\,dv = c$, we see that so long as the forces above and below the piston remain equal to each other no vis viva is generated. The piston may rise or it may descend, but the motion will not be due to either of the antagonistic forces, whose function it is to reduce each other to nullity. The office of the elasticity of gas in raising a superincumbent weight is simply and exclusively that of giving it statical support at every point of the rise. The force $P-p$ generates acceleration, the forces $p-p$ make unaccelerated rise or fall possible: and it is the singular infelicity of the modern doctrine that heat is created by the expenditure of work, that the definition of the work expended does not include the case in which alone motion or heat is created, and does include only the cases where no motion or heat is created. I do not think I can more distinctly contradict every part of the received doctrine on this subject than by stating simply what appears to me to be unquestionable, as truth—that no force employed in equilibrating resistances ever becomes converted into heat, and that no heat is ever generated except by forces acting on bodies verging on the state of motion, and offering no resistance to the action of the forces.

Milland, June 21, 1870,
IX. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from vol. xxxix. p. 462.]

March 10, 1870.—Warren De La Rue, Esq., Vice-President, in the Chair.

The following communication was read:—

"Researches on Solar Physics.—No. II. The Positions and Areas of the Spots observed at Kew during the years 1864-66, also the Spotted Area of the Sun's visible disk from the commencement of 1832 up to May 1868." By Warren De La Rue, Esq., Ph.D., F.R.S., F.R.A.S., Balfour Stewart, Esq., LL.D., F.R.S., F.R.A.S., &c., and Benjamin Loewy, Esq., F.R.A.S.

The paper commences with a continuation for the years 1864-66 of Tables II. and III. of a previous paper by the same authors; it then proceeds to a discussion of the value of the pictures of the sun made by Hofrath Schwabe, which had been placed at the disposal of the authors; and the result is that these pictures, when compared with simultaneous pictures taken by Carrington and by the Kew heliograph, are found to be of great trustworthiness. From 1832 to 1854 the pictures discussed are those of Schwabe, who was the only observer between these dates; then follows the series taken by Carrington, and lastly the Kew series, which began in 1862.

A list is given of the values of the sun's spotted area for every fortnight, from the beginning of 1832 up to May 1868, and also a list of three-monthly values of the same, each three-monthly value being the mean of the three fortnightly values which precede and of the three which follow it. These three-monthly values are also given for every fortnight.

A plate is appended to the paper, in which a curve is laid down representing the progress of solar disturbance as derived from the three-monthly values; and another curve is derived from this by a simple process of equalization, representing the progress of the ten-yearly period. The values of the latter curve, corresponding to every fortnight, are also tabulated. From this Table are derived the following epochs of maxima and minima of the longer period:—

Minimum Nov. 28, 1833.
Maximum ............ Dec. 21, 1836.
Minimum Sept. 21, 1843.
Maximum ............ Nov. 14, 1847.
Minimum April 21, 1856.
Maximum ............ Sept. 7, 1859.
Minimum Feb. 14, 1867.

This exhibits a variability in the length of the whole period.

Thus we have between 1st and 2nd minimum 9.81 years.
2nd and 3rd do. 12.58 "
3rd and 4th do. 10.81 "

Mean of all the periods 11.07 years.
Another fact previously noted by Sir J. Herschel is brought to light—namely, that the time between a minimum and the next maximum is less than that from the maximum to the next minimum.

Thus the times from the minimum to the maximum are for the three periods 3·06, 4·14, and 3·37, while those from the maximum to the minimum are 6·75, 8·44, and 7·44 years.

In all the three periods there are times of secondary maxima after the first maximum; and in order to exhibit this peculiarity, statistics are given of the light-curve of R. Sagittae and of β Lyrae, two variable stars which present peculiarities similar to the sun.

Finally, the results are tested to see whether they exhibit any trace of planetary influence; and for this purpose the conjunctions of Jupiter and Venus, of Venus and Mercury, of Jupiter and Mercury, as well as the varying distances of Mercury alone in its elliptical orbit, have been made use of; and the united effect is exhibited in the following Table, the unit of spotted area being one-millionth of the sun's visible hemisphere:

<table>
<thead>
<tr>
<th>Angular separation</th>
<th>Jupiter and Venus</th>
<th>Venus and Mercury</th>
<th>Mercury alone (Perihelion=0)</th>
<th>Mercury and Jupiter</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 to 30</td>
<td>+ 881</td>
<td>+1675</td>
<td>- 380</td>
<td>-227</td>
</tr>
<tr>
<td>30 to 60</td>
<td>- 60</td>
<td>- 139</td>
<td>-1188</td>
<td>-317</td>
</tr>
<tr>
<td>60 to 90</td>
<td>- 452</td>
<td>-1665</td>
<td>-1287</td>
<td>-594</td>
</tr>
<tr>
<td>90 to 120</td>
<td>- 579</td>
<td>-2355</td>
<td>-1262</td>
<td>-714</td>
</tr>
<tr>
<td>120 to 150</td>
<td>- 705</td>
<td>-2318</td>
<td>-1208</td>
<td>-508</td>
</tr>
<tr>
<td>150 to 180</td>
<td>- 759</td>
<td>-1604</td>
<td>-1027</td>
<td>-491</td>
</tr>
<tr>
<td>180 to 210</td>
<td>- 893</td>
<td>- 481</td>
<td>- 519</td>
<td>-416</td>
</tr>
<tr>
<td>210 to 240</td>
<td>- 752</td>
<td>+ 547</td>
<td>+ 430</td>
<td>-189</td>
</tr>
<tr>
<td>240 to 270</td>
<td>- 263</td>
<td>+ 431</td>
<td>+1082</td>
<td>- 25</td>
</tr>
<tr>
<td>270 to 300</td>
<td>+ 70</td>
<td>+ 228</td>
<td>+1436</td>
<td>+154</td>
</tr>
<tr>
<td>300 to 330</td>
<td>+ 480</td>
<td>+1318</td>
<td>+1282</td>
<td>+164</td>
</tr>
<tr>
<td>330 to 0</td>
<td>+1134</td>
<td>+2283</td>
<td>+586</td>
<td>+ 45</td>
</tr>
</tbody>
</table>

March 17.—Captain Richards, R.N., Vice-President, in the Chair.

The following communication was read:

"On the Estimation of Ammonia in Atmospheric Air." By Horace T. Brown, Esq.

In the attempts that have been hitherto made to estimate the ammonia present in atmospheric air, the results arrived at by the various experimenters have differed so widely that it is still a matter of uncertainty what the quantity really is. That it is a very small amount all agree, but the extreme results on record vary as much as from 13·5 to 101 part of carbonate of ammonium per 100,000 of air. It may therefore not be without interest to give an account of a simple method affording very concordant and, I believe, accurate results, at the same time being easy of performance and requiring but little time for an experiment.

The apparatus used consists of two glass tubes, each of about 1
of Ammonia in Atmospheric Air.

55

metre in length and 12 millims. bore. These are connected air-tight by means of a smaller glass tube, and inclined at an angle of 5° or 6° with the horizon. Into each of the larger tubes are introduced 100 cub. centims. of a mixture of perfectly pure water and two drops of dilute sulphuric acid (sp. gr. 1·18). Through this acidulated water a measured quantity of the air under examination is slowly drawn, in small bubbles, by means of an aspirator.

No porous substance must be used to filter the air, for reasons to be stated hereafter. The air is conducted into the absorption liquid through a small piece of quill tubing drawn out to a small aperture at the end immersed. This tube must be kept quite dry throughout the experiment. Great care must be taken to cleanse perfectly every part of the apparatus with water free from ammonia, and the caoutchouc plugs, or corks, used must be boiled for a short time in a dilute solution of caustic soda.

The stream of air is so regulated as to allow about 1 litre to pass through the apparatus in an hour.

By directing the point of the delivery-tube laterally, each bubble has imparted to it on rising an oscillatory movement which facilitates complete absorption of the ammonia.

When from 10 to 20 litres of air have passed, the liquid is emptied from the tubes into upright glass cylinders, an excess of a perfectly pure solution of potash added, and then 3 cub. centims. of a Nessler solution. The standard of comparison is made in the ordinary way, only using acidulated in place of pure water, and neutralizing with potash after adding the standard solution of ammonium salt. Beyond somewhat retarding the point of maximum coloration, a little potas-
sium sulphate does not interfere with the delicacy of Nessler's reaction.

If the experiment has been conducted with proper care, at least \( \frac{3}{5} \) of the total ammonia ought to be found in the first tube. Four or five litres of air are generally quite sufficient to give a decided reaction, but it is better to use not less than 10 litres, as before mentioned*.

Very many experiments have been made by this method, both on air from the town of Burton-on-Trent, and that of the adjoining country. The air from the town, as might be expected, varies somewhat in composition; much more so than that taken from the open country, as may be seen from the following Tables, in which are given some of the numerous results obtained.

The ammonia is calculated in every case as carbonate \((\text{NH}_4)_2\text{CO}_3\); for although nitric acid is sometimes found in air, yet its presence must be looked upon as accidental.

In the immediate vicinity of towns some of the ammonia must also be in the form of sulphate, sulphite, or ammonium chloride.

* When the air to be examined is highly charged with ammonia, as that from stables &c., a perfectly dry bottle of 3 or 4 litres capacity should be carefully filled with a pair of bellows, 100 cub. centims. of acidulated water introduced, and, after closing securely, the whole well agitated at intervals for three or four hours. The liquid is then poured out, and the \( \text{NH}_3 \) estimated by the Nessler solution as usual.
**Royal Society** — Messrs. Roscoe and Thorpe on the

(1) *Air taken from town.* (Taken at a height of 2 metres from ground.)

<table>
<thead>
<tr>
<th>Date of Experiment</th>
<th>((\text{NH}_4)_2\text{CO}_3) as grammes per 100,000 litres of air at 0°C and 760 mm. barom.</th>
<th>((\text{NH}_4)_2\text{CO}_3) in parts by weight per 100,000 of air.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1869. September 30</td>
<td>1.1294</td>
<td>8732</td>
</tr>
<tr>
<td>October 4</td>
<td>0.62117</td>
<td>4801</td>
</tr>
<tr>
<td>6</td>
<td>0.5251</td>
<td>4059</td>
</tr>
<tr>
<td>8</td>
<td>0.62117</td>
<td>4801</td>
</tr>
<tr>
<td>November 26</td>
<td>1.0729</td>
<td>8293</td>
</tr>
<tr>
<td>28</td>
<td>1.1000</td>
<td>8503</td>
</tr>
</tbody>
</table>

(2) *Air from country.* (Taken at a height of 2 metres.)

<table>
<thead>
<tr>
<th>Date of Experiment</th>
<th>((\text{NH}_4)_2\text{CO}_3) as grammes per 100,000 litres of air at 0°C and 760 mm. barom.</th>
<th>((\text{NH}_4)_2\text{CO}_3) in parts by weight per 100,000 of air.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1869. December 6</td>
<td>0.7620</td>
<td>5890</td>
</tr>
<tr>
<td>8</td>
<td>0.7826</td>
<td>6085</td>
</tr>
<tr>
<td>9</td>
<td>0.6601</td>
<td>5102</td>
</tr>
<tr>
<td>11</td>
<td>0.6635</td>
<td>5121</td>
</tr>
<tr>
<td>1870. February 12</td>
<td>0.7639</td>
<td>5904</td>
</tr>
</tbody>
</table>

The direction of the wind does not seem to have any influence on the ammonia found; immediately after heavy rain, however, the quantity falls somewhat below the average, but the air is again restored to its normal condition after a lapse of two or three hours.

Attempts were made to make the method more delicate still by absorbing the ammonia in pure water and then distilling, but the nitrogenous organic matter suspended in the air was found to interfere with the results.

When the air is passed through cotton-wool before entering the absorption-tubes, it is found to be entirely deprived of its ammonia by the filter. This is also the case with air artificially charged with ammonia to a large extent. This absorption is not due to the presence of hygroscopic moisture, since cotton-wool, when absolutely dry, is capable of taking up 115 times its own bulk of dry ammonia (confined over mercury) at 10°C and 755.7 millims. barom., the gas being again slowly evolved when the wool is left in contact with the air at 100°C.

All other porous substances that were tried for filtering agents were found to possess this property more or less; even freshly ignited pumice-stone is not entirely without absorptive effect upon the gas.

March 31.—Lieut.-General Sir Edward Sabine, K.C.B.,
President, in the Chair.

The following communication was read:

"On the Relation between the Sun’s Altitude and the Chemical Intensity of Total Daylight in a Cloudless Sky." By Henry E. Roscoe, F.R.S., and T. E. Thorpe, Ph.D.

In this communication the authors give the results of a series of
determinations of the chemical intensity of total daylight made in the autumn of 1867 on the flat tableland on the southern side of the Tagus, about 8½ miles to the south-east of Lisbon, under a cloudless sky, with the object of ascertaining the relation existing between the solar altitude and the chemical intensity. The method of measurement adopted was that described in a previous communication to the Society*, founded upon the exact estimation of the tint which standard sensitive paper assumes when exposed for a given time to the action of daylight. The experiments were made as follows:—

1. The chemical action of total daylight was observed in the ordinary manner.

2. The chemical action of the diffused daylight was then observed by throwing on to the exposed paper the shadow of a small blackened brass ball, placed at such a distance that its apparent diameter, seen from the position of the paper, was slightly larger than that of the sun’s disk.

3. Observation No. 1 was repeated.

4. Observation No. 2 was repeated.

The means of observations 1 and 3 and of 2 and 4 were then taken. The sun’s altitude was determined by a sextant and artificial horizon, immediately before and immediately after the observations of chemical intensity, the altitude at the time of observation being ascertained by interpolation.

It was first shown that an accidental variation in the position of the brass ball within limits of distance from the paper, varying from 140 millims. to 230 millims., was without any appreciable effect on the results. One of the 134 sets of observations was made as nearly as possible every hour, and they thus naturally fall into seven groups, viz.:

(1) Six hours from noon, (2) five hours from noon, (3) four hours from noon, (4) three hours from noon, (5) two hours from noon, (6) one hour from noon, (7) noon.

Each of the first six of these groups contains two separate sets of observations,—(1) those made before noon, (2) those made after noon. It has already been pointed out†, from experiments made at Kew, that the mean chemical intensity of total daylight for hours equidistant from noon is the same. The results of the present series of experiments prove that this conclusion holds good generally; and a Table is given showing the close approximation of the numbers obtained at hours equidistant from noon.

Curves are given showing the daily march of chemical intensity at Lisbon in August, compared with that at Kew for the preceding August, and at Pará for the preceding April. The value of the mean chemical intensity at Kew is represented by the number 94·5, that at Lisbon by 110, and that at Pará by 313·3, light of the intensity 1 acting for 24 hours being taken as 1000.

† Phil. Trans. 1867, p. 558.
The following Table gives the results of the observations arranged according to the sun's altitude.

<table>
<thead>
<tr>
<th>No. of observations</th>
<th>Mean altitude</th>
<th>Chemical Intensity</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td><strong>Sun.</strong></td>
</tr>
<tr>
<td>15</td>
<td>9 51</td>
<td>0·000</td>
</tr>
<tr>
<td>18</td>
<td>19 41</td>
<td>0·023</td>
</tr>
<tr>
<td>22</td>
<td>31 14</td>
<td>0·052</td>
</tr>
<tr>
<td>22</td>
<td>42 13</td>
<td>0·100</td>
</tr>
<tr>
<td>19</td>
<td>53 09</td>
<td>0·136</td>
</tr>
<tr>
<td>24</td>
<td>61 08</td>
<td>0·195</td>
</tr>
<tr>
<td>11</td>
<td>64 14</td>
<td>0·221</td>
</tr>
</tbody>
</table>

Curves are given showing the relation between the direct sunlight (column 3) and diffuse daylight (column 4) in terms of the altitude. The curve of direct sunlight cuts the base line at 10°, showing that the conclusion formerly arrived at by one of the authors is correct, and that at altitudes below 10° the direct sunlight is robbed of almost all its chemically active rays. The relation between the total chemical intensity and the solar altitude is shown to be represented graphically by a straight line for altitudes above 10°, the position of the experimentally determined points lying closely on to the straight line.

A similar relation has already* been shown to exist (by a far less complete series of experiments than the present) for Kew, Heidelberg, and Pará; so that although the chemical intensity for the same altitude at different places and at different times of the year varies according to the varying transparency of the atmosphere, yet the relation at the same place between altitude and intensity is always represented by a straight line. This variation in the direction of the straight line is due to the opalescence of the atmosphere; and the authors show that, for equal altitudes, the higher intensity is always found where the mean temperature of the air is greater, as in summer, when observations at the same place at different seasons are compared, or as the equator is approached, when the actions at different places are examined. The differences in the observed actions for equal altitudes, which may amount to more than 100 per cent. at different places, and to nearly as much at the same place at different times of the year, serve as exact measurements of the transparency of the atmosphere.

The authors conclude by calling attention to the close agreement between the curve of daily intensity obtained by the above-mentioned method at Lisbon, and that calculated for Naples by a totally different method.

April 7.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

The following communications were read:—

"On Supra-annual Cycles of Temperature in the Earth’s Surface-crust." By Professor C. Piazzi Smyth, F.R.S.

The author presents and discusses the completely reduced obser-

* Phil. Trans. 1867, p. 555.
The Rev. Samuel Haughton on the Granites of Scotland.

Observations, from 1837 to 1869 inclusive, of the four great earth-thermometers sunk into the rock of the Calton Hill, at the Royal Observatory, Edinburgh, by the late Principal Forbes, pursuant to a vote by the British Association for the Advancement of Science.

Leaving on one side the several natural-philosophy data which have been investigated from smaller portions of the same series of observations both by Principal Forbes and Sir William Thomson, the author applies himself solely to trace the existence of other cycles than the ordinary annual one, in the rise and fall of the different thermometers.

Of such cycles, and of more than one year’s duration, he considers that he has discovered three; and of these the most marked has a period of 11.1 years, or practically the same as Schwabe’s numbers for new groups of solar spots. Several numerical circumstances, however, which the author details, show that the sun-spots cannot be the actual cause of the observed waves of terrestrial temperature, and he suggests what may be, concluding with two examples of the practical use to which a knowledge of the temperature cycles as observed may at once be turned, no matter to what cosmical origin their existence may be owing.

"On the Constituent Minerals of the Granites of Scotland, as compared with those of Donegal." By the Rev. Samuel Haughton, F.R.S., M.D. Dubl., D.C.L. Oxon.

During the past summer (1869) I completed my investigation of the constituent minerals of the Scotch Granites, and secured specimens, from the analysis of which I obtained the following results:

I. Orthoclase.

<table>
<thead>
<tr>
<th>No.</th>
<th>Composition</th>
<th>Vein Origin</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Orthoclase</td>
<td>Stirling</td>
<td>Occurs in an eruptive Granite, in veins, in well-developed reddish-pink opaque crystals, encrusted with crystals of Albite.</td>
</tr>
<tr>
<td>2</td>
<td>Orthoclase</td>
<td>Rubislaw</td>
<td>Large beautiful reddish-pink opaque crystals, in veins, associated with white Mica. The Granite of Rubislaw is of metamorphic origin, and different in character from the eruptive Granite of Peterhead. No Albite has been found in it.</td>
</tr>
<tr>
<td>3</td>
<td>Orthoclase</td>
<td>Peterculter</td>
<td>In metamorphic Granite; white, translucent, large crystals.</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Silica</th>
<th>Alumina</th>
<th>Peroxide of iron</th>
<th>Lime</th>
<th>Magnesia</th>
<th>Soda</th>
<th>Potash</th>
<th>Water</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. 1</td>
<td>65·40</td>
<td>19·04</td>
<td>0·80</td>
<td>0·22</td>
<td>trace</td>
<td>3·63</td>
<td>11·26</td>
<td>0·20</td>
</tr>
<tr>
<td>No. 2</td>
<td>64·44</td>
<td>18·64</td>
<td>trace</td>
<td>0·66</td>
<td>0·66</td>
<td>2·73</td>
<td>12·15</td>
<td>0·80</td>
</tr>
<tr>
<td>No. 3</td>
<td>64·48</td>
<td>20·00</td>
<td>none</td>
<td>1·01</td>
<td>trace</td>
<td>1·72</td>
<td>12·81</td>
<td>0·64</td>
</tr>
<tr>
<td>No. 4</td>
<td>64·48</td>
<td>20·00</td>
<td>none</td>
<td>none</td>
<td>trace</td>
<td>2·19</td>
<td>12·10</td>
<td>0·08</td>
</tr>
</tbody>
</table>

<p>| | | | | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>99·75</td>
<td>100·22</td>
<td>100·66</td>
<td>99·63</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>


No. 2. Rubislaw, Aberdeen. Large beautiful reddish-pink opaque crystals, in veins, associated with white Mica. The Granite of Rubislaw is of metamorphic origin, and different in character from the eruptive Granite of Peterhead. No Albite has been found in it.

No. 3. Peterculter, Aberdeen. In metamorphic Granite; white, translucent, large crystals.
No. 4. Callernish, extreme west of Lewis. In metamorphic Granite; in large grey crystals, with a slight shade of pink, translucent.

The oxygen ratio of these felspars is as follows:

<table>
<thead>
<tr>
<th></th>
<th>No. 1</th>
<th>No. 2</th>
<th>No. 3</th>
<th>No. 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>33·956</td>
<td>33·456</td>
<td>33·478</td>
<td>33·477</td>
</tr>
<tr>
<td>Alumina &amp;c.</td>
<td>8·898</td>
<td>8·950</td>
<td>9·348</td>
<td>9·348</td>
</tr>
<tr>
<td>Lime</td>
<td>0·061</td>
<td>0·187</td>
<td>0·286</td>
<td>0·221</td>
</tr>
<tr>
<td>Soda</td>
<td>0·929</td>
<td>0·699</td>
<td>0·440</td>
<td>0·561</td>
</tr>
<tr>
<td>Potash</td>
<td>1·908</td>
<td>2·059</td>
<td>2·171</td>
<td>2·051</td>
</tr>
<tr>
<td></td>
<td>45·752</td>
<td>45·351</td>
<td>45·723</td>
<td>45·658</td>
</tr>
</tbody>
</table>

From this Table we find the oxygen ratios:

<table>
<thead>
<tr>
<th></th>
<th>No. 1</th>
<th>No. 2</th>
<th>No. 3</th>
<th>No. 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>11·37</td>
<td>11·35</td>
<td>11·55</td>
<td>11·82</td>
</tr>
<tr>
<td>Peroxides</td>
<td>3·06</td>
<td>3·04</td>
<td>3·22</td>
<td>3·30</td>
</tr>
<tr>
<td>Protoxides</td>
<td>1·00</td>
<td>1·00</td>
<td>1·00</td>
<td>1·00</td>
</tr>
</tbody>
</table>

The Granites of central and western Scotland are metamorphic rocks, like those of Donegal and Norway, with which they are geologically identical; and truly eruptive Granite occurs at only a few localities, as, for example, near Peterhead.

The second felspar associated with Orthoclase in the Metamorphic Granites is Oligoclase, as in Donegal; while the second felspar associated with Orthoclase in the eruptive Granites is Albite, as in Mourne, Leinster, and Cornwall. The fact thus indicated by the Scotch Granites is completely in accordance with the mode of occurrence of Oligoclase and Albite in the Irish Granites.

II. Oligoclase.

<table>
<thead>
<tr>
<th></th>
<th>No. 1</th>
<th>No. 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>62·00</td>
<td>61·88</td>
</tr>
<tr>
<td>Alumina</td>
<td>23·20</td>
<td>24·80</td>
</tr>
<tr>
<td>Magnesia</td>
<td>trace.</td>
<td>trace.</td>
</tr>
<tr>
<td>Lime</td>
<td>4·71</td>
<td>4·93</td>
</tr>
<tr>
<td>Soda</td>
<td>9·20</td>
<td>8·12</td>
</tr>
<tr>
<td>Potash</td>
<td>0·43</td>
<td>0·98</td>
</tr>
<tr>
<td></td>
<td>99·54</td>
<td>100·71</td>
</tr>
</tbody>
</table>

No. 1. This Oligoclase occurs in the Granite of Craigie-Buckler, near Aberdeen; it is white and opaque, and so much resembles Cleavelandite in appearance as to have been mistaken for that variety of Albite; its analysis proves it to be Oligoclase. The crystals do not exhibit striation.

No. 2. From the Granite of Rhiconich, in the west of Sutherland-shire; it is greyish white, semitranslucent, in large striated crystals, and resembles the Oligoclase of Ytterby, in Sweden.
The oxygen ratios of the Oligoclase are as follow:

<table>
<thead>
<tr>
<th></th>
<th>No. 1</th>
<th>No. 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>32·191</td>
<td>32·128</td>
</tr>
<tr>
<td>Alumina</td>
<td>10·843</td>
<td>11·590</td>
</tr>
<tr>
<td>Lime</td>
<td>1·339</td>
<td>1·400</td>
</tr>
<tr>
<td>Soda</td>
<td>2·360</td>
<td>2·082</td>
</tr>
<tr>
<td>Potash</td>
<td>0·072</td>
<td>0·165</td>
</tr>
</tbody>
</table>

46·805  47·365

Hence we obtain:

<table>
<thead>
<tr>
<th></th>
<th>No. 1</th>
<th>No. 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>8·54</td>
<td>8·82</td>
</tr>
<tr>
<td>Peroxides</td>
<td>2·88</td>
<td>3·18</td>
</tr>
<tr>
<td>Protoxides</td>
<td>1·00</td>
<td>1·00</td>
</tr>
</tbody>
</table>

These oxygen ratios prove the felspars to be Oligoclase.

### III. Albite.

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>68·00</td>
</tr>
<tr>
<td>Alumina</td>
<td>20·00</td>
</tr>
<tr>
<td>Lime</td>
<td>0·35</td>
</tr>
<tr>
<td>Magnesia</td>
<td>trace.</td>
</tr>
<tr>
<td>Soda</td>
<td>10·88</td>
</tr>
<tr>
<td>Potash</td>
<td>0·68</td>
</tr>
</tbody>
</table>

99·91

This Albite occurs at Stirling Hill, near Peterhead, in eruptive Granite, and is found associated with red Orthoclase in veins; it encrusts the large crystals of Orthoclase, and is semitranslucent, and is generally stained on the surface by peroxide of iron.

### Oxygen Ratios.

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>35·306</td>
</tr>
<tr>
<td>Alumina</td>
<td>9·348</td>
</tr>
<tr>
<td>Lime</td>
<td>0·099</td>
</tr>
<tr>
<td>Soda</td>
<td>2·790</td>
</tr>
<tr>
<td>Potash</td>
<td>0·114</td>
</tr>
</tbody>
</table>

This mineral is evidently a typical Albite.

There are two kinds of Mica found in the Scotch Granites, and both Micas resemble very closely the corresponding minerals of the Donegal Granites.
IV. White Mica.

Silica ........................................... 44·40
Fluosilicon (SiF₃) .......................... 0·16
Alumina ............................... 37·36
Peroxide of iron ......................... 2·04
Lime .................................... 0·78
Magnesia ............................. 0·57
Soda ................................... 0·93
Potash .................................. 9·87
Protoxide of manganese ............. 0·24
Water ................................... 1·84

98·19

The specimen of Mica here analyzed came from veins in the Granite quarry of Rubislaw, near Aberdeen, and occurs in large plates, associated with red Orthooclase. It was carefully examined for lithia, but no trace of this alkali could be found in it.

The angles of the rhombic plates were 60° and 120° exactly, and the angle between its optic axes was found to be 72° 30'.

The black Mica in large crystals is very rare, but it seems abundantly disseminated in minute scales through most of the Scotch Granites. The following analysis was made on specimens found near Aberdeen by Prof. Nicol, and kindly forwarded to me by him, for the purposes of this paper:—

V. Black Mica.

Silica ........................................... 36·50
Alumina ............................... 16·50
Peroxide of iron ......................... 18·49
Lime .................................... 1·11
Magnesia ............................. 7·44
Soda ................................... 0·92
Potash .................................. 8·77
Protoxide of iron ................. 6·76
Protoxide of manganese ....... 1·80
Water ................................... 1·60

99·89

This mica was carefully examined for fluorine, and found not to contain any.

"Researches on Vanadium."—Part III. Preliminary Notice. By Henry E. Roscoe, B.A., F.R.S.

I. METALLIC VANADIUM*.

In the second part of "Researches on Vanadium," it was stated

* Phil. Trans. 1869, p. 679.
that the metal absorbs hydrogen. This conclusion has been fully borne out by subsequent experiment; and it appears that the amount of absorbed or combined hydrogen taken up by the metal varies according to the state of division, first, of the chloride (\(\text{VCl}_3\)) from which the metal is prepared, and secondly, and especially, of the metal itself. The metal containing absorbed hydrogen slowly takes up oxygen on exposure to dry air, water being formed and the metal undergoing oxidation to the lowest oxide, \(\text{V}_2\text{O}_3\). At this point the oxidation stops, but in moist air it proceeds still further.

The difficulty of obtaining metallic vanadium free from admixture of oxide has been again rendered evident. Perfectly pure tetra-chloride was prepared in quantity; and from this, pure dichloride was made. On heating this to whiteness in dry hydrogen for 48 hours a substance was obtained which gained on oxidation \(70.7\) per cent. (vanadium requiring \(77-79\) percentage increase), and therefore still contained a slight admixture of oxide.

The reducing action of sodium on the solid chlorides was next examined; in this case the reduction takes place quietly in an atmosphere of hydrogen at a red heat, and is best conducted in strong iron tubes. Explosions occur when sodium acts on the liquid tetrachloride. The substance thus obtained was found, after lixiviation, to be free from chlorine, and on washing it separated into two portions — (1) a light and finely divided black powder (tribromide), which remains in suspension, and is soluble in hydrochloric acid, and (2) a heavier grey powder, insoluble in hydrochloric acid, which soon deposits, and can, by repeated washing, be completely freed from the lighter tribromide. This bright grey powder consists of metallic vanadium, mixed with more or less oxide. If this metallic powder, after drying \textit{in vacuo}, be reduced at a low red heat in a current of pure hydrogen, it takes fire spontaneously, even when cold, on exposure to air or oxygen, water being formed, whilst the vanadium undergoes oxidation, forming the blue oxide, \(\text{V}_2\text{O}_5\). A portion of metal exposed for some weeks to the air also slowly absorbed oxygen, passing into the oxide, \(\text{V}_2\text{O}_5\).

II. \textbf{Vanadium and Bromine.}

1. \textit{Vanadium Tribromide}, \(\text{VBr}_3\), molec. wt. = 291.3.—When excess of bromine is passed over vanadium mononitride heated to redness, a vivid action occurs, and dense dark-brown vapours are formed, condensing in the cooler portions of the tube to a greyish-black, opaque, amorphous mass of the tribromide. The tribromide is a very unstable compound, losing bromine even when kept sealed up in glass tubes; it is very deliquescent, and on heating in the air rapidly loses all its bromine and takes up oxygen, with formation of vanadic acid. On being thrown into water, the tribromide readily dissolves, forming a brown liquid (in this respect resembling the trichloride), which, on addition of a few drops of hydrochloric acid, turns of a bright green colour, showing the presence of a solution of an hypovanadic salt. No free bromine or hydrobromic acid is given off on dissolving the tribromide in water. That a more volatile higher
bromide was not formed in this reaction was shown, inasmuch as, on distilling the excess of liquid which had collected in the receiver, it was found to consist of free bromine, containing mere traces of the tribromide mechanically carried over. The tribromide is likewise formed when bromine is passed over a red-hot mixture of vanadium trioxide and pure charcoal, as in the preparation of the tetrachloride; but this method is not one to be recommended, as the tube becomes constantly stopped up by the formation of the solid tribromide. The analysis of the tribromide was made by dissolving the compound in water, and precipitating the bromine with excess of nitrate of silver, the vanadium being estimated as $V_2O_5$, either in the filtrate from the bromide of silver or in a separate portion. The bromine in the above determinations, obtained by precipitation as silver-salt, was invariably found to be too high, whilst the vanadium nearly agreed with the theoretical percentage. This is due to the fact pointed out by Stas, in his 'Recherches,' p. 156, that bromide of silver, when boiled, encloses mechanically a portion of the precipitant, which then cannot be washed out. The loss of weight obtained by reducing the bromide to metallic silver in a current of hydrogen, taken as bromine, gave more nearly agreeing numbers:

<table>
<thead>
<tr>
<th>Substance</th>
<th>Calculated</th>
<th>Mean of 6 determinations.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vanadium</td>
<td>V = 51.3</td>
<td>17.61</td>
</tr>
<tr>
<td>Bromine</td>
<td>Br$_3$ = 240.0</td>
<td>82.39</td>
</tr>
</tbody>
</table>

2. *Vanadium Oxytribromide*, or *Vanadyl Tribromide*, VOB$_3$, molec. wt. = 307.3. — The oxytribromide is a dark-red transparent liquid, evolving white fumes on contact with the air, obtained by passing pure and dry bromine over vanadium trioxide ($V_2O_5$) heated to redness. Moisture prevents the formation of the oxytribromide; and it not only undergoes sudden decomposition when heated to 180°, but also slowly decomposes at the ordinary atmospheric temperatures. The boiling-point of the tribromide can, however, be brought below its temperature of decomposition by distillation in vacuo, and the liquid can then be freed completely from bromine by passing a current of dry air through the liquid. Under a pressure of 100 millims. the oxytribromide boils from 130° to 135°, and may be distilled almost without decomposition. Vanadium oxytribromide dissolves in water, yielding a yellow-coloured solution, in which both vanadium and bromine were determined, after reduction with sulphurous acid:

<table>
<thead>
<tr>
<th>Substance</th>
<th>Calculated</th>
<th>Mean of several analyses.</th>
</tr>
</thead>
<tbody>
<tr>
<td>V</td>
<td>51.3</td>
<td>16.69</td>
</tr>
<tr>
<td>Br$_3$</td>
<td>240.0</td>
<td>78.10</td>
</tr>
<tr>
<td>O</td>
<td>16.0</td>
<td>7.21</td>
</tr>
</tbody>
</table>

The specific gravity of the oxytribromide at 0° is 2.967.
3. Vanadium Oxydibromide, or Vanadyl Dibromide, \( \text{VOBr}_2 \), molec. wt. = 227·3.—This is a solid substance, of a yellowish-brown colour, obtained by the sudden decomposition of the foregoing compound at temperatures above 100°, or by its slow decomposition at the ordinary temperature.

The oxydibromide is very deliquescent, dissolving in water, with formation of a blue solution of a vanadious salt. When heated in the air it loses all its bromine, and is converted into \( \text{V}_2\text{O}_3 \).

Analysis gave:

\[
\begin{align*}
\text{V} & = 51·3 \\
\text{Br}_2 & = 160·0 \\
\text{O} & = 16·0
\end{align*}
\]

Mean of several analyses:

\[
\begin{align*}
\text{V} & = 22·57 \\
\text{Br}_2 & = 70·39 \\
\text{O} & = 7·104
\end{align*}
\]

III. Vanadium and Iodine.

Iodine-vapour does not attack either the trioxide or the nitride at a red heat; both these substances remain unchanged, and no trace of vanadium can be detected in the iodine which has passed over them.

IV. The Metallic Vanadates.

In the first part of these Researches (Phil. Trans. 1868) it was pointed out (1) that the salts analyzed by Berzelius must be considered as meta- or monobasic vanadates, (2) that the so-called biva-


adates analyzed by Von Hauer are anhydro-salts, and (3) that the ortho-


or tribasic vanadates contain 3 atoms of monad metal, the sodium salt being formed artificially by fusing 1 molecule of vanadium pentoxide with 3 molecules of carbonate of soda, when 3 molecules of carbon dioxide are expelled, whilst the orthosalts occur native in many metals. The present communication contains a description of these classes of salts, as well as of a new class of salts, the tetra-


basic or pyro-vanadates.

Sodium Vanadates.

1. Ortho- or Tri-Sodium Vanadate, \( \text{Na}_3\text{VO}_4 + 16\text{H}_2\text{O} \).—When a mixture of 3 molecules of \( \text{Na}_3\text{CO}_3 \) and 1 molecule of \( \text{V}_2\text{O}_3 \) is fused until no further evolution of \( \text{CO}_2 \) is observed, a tribasic vanadate remains as a white crystalline mass. This mass dissolves easily in water, and on addition of absolute alcohol to the solution two layers of liquid are formed; the lower one solidifies after a time, forming an aggregation of needle-shaped crystals, which possess a strongly alkaline reaction. These having been washed with alcohol, and dried on a porous plate over sulphuric acid in \( \text{vacuo} \), were analyzed with the following results:

<table>
<thead>
<tr>
<th>Element</th>
<th>Calculated</th>
<th>Mean of several analyses</th>
</tr>
</thead>
<tbody>
<tr>
<td>V</td>
<td>22·57</td>
<td>22·45</td>
</tr>
<tr>
<td>Br</td>
<td>70·39</td>
<td>70·93</td>
</tr>
<tr>
<td>O</td>
<td>7·104</td>
<td></td>
</tr>
<tr>
<td></td>
<td>227·3</td>
<td>100·00</td>
</tr>
</tbody>
</table>
The sodium in this and in the following compounds was separated from the vanadium by precipitating the vanadic acid as the perfectly insoluble basic lead salt hereafter described. This was dried at 100° and weighed, then dissolved in nitric acid and decomposed by sulphuric acid, and the solution of $V_2O_5$ in excess of this acid gave on evaporation a finely crystalline mass. The filtrate from the lead precipitate freed from lead yielded on evaporation sodium sulphate. Full analytical details of this method, as well as of the other by precipitation as the insoluble ammonium metavanadate, are given in the memoir. By frequent crystallizations the trisodium vanadate is slowly decomposed into the tetrasodium salt, caustic soda being formed. This singular reaction was most carefully examined and the amount of sodium hydroxide liberated determined volumetrically.

2. Tetrasodium Vanadate, $Na_4V_2O_7 + 18H_2O$.—This salt crystallizes in beautiful six-sided tables; it is easily soluble in water, insoluble in alcohol, and is precipitated by the latter liquid from aqueous solution in white scales of a silky lustre. As long as the salt contains free alkali or tribasic salt, it forms, on precipitation with alcohol, oily drops which solidify after some time. The tetrasodium vanadate is always formed by the first fusion of vanadic acid with excess of carbonate of soda, and can be easily prepared in the pure state by recrystallization.

<table>
<thead>
<tr>
<th></th>
<th>Calculated</th>
<th>Found (mean of many determinations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$Na_4$</td>
<td>92·0</td>
<td>14·58</td>
</tr>
<tr>
<td>$V_2$</td>
<td>102·6</td>
<td>16·27</td>
</tr>
<tr>
<td>$O_7$</td>
<td>112·0</td>
<td>17·27</td>
</tr>
<tr>
<td>$18H_2O$</td>
<td>324·0</td>
<td>51·38</td>
</tr>
<tr>
<td></td>
<td></td>
<td>630·6</td>
</tr>
<tr>
<td></td>
<td></td>
<td>99·99</td>
</tr>
</tbody>
</table>

The salt loses 17 molecules of water at 100°.

The corresponding Calcium and Barium Vanadates, $Ca_2V_2O_7$ and $Ba_2V_2O_7$, are white precipitates obtained by adding the chlorides to a solution of tetrasodium vanadate. If calcium chloride be added to a solution of the trisodium salt, dicalcium vanadate is precipitated, the solution becoming strongly alkaline from formation of calcium hydroxide and absorbing carbonic acid from the air. Complete analysis showed that the calcium salt contains $2\frac{1}{3}$ molecules of water of crystallization, whilst the barium salt is anhydrous.

Lead Vanadates.

1. Tribasic or Ortho-Lead Vanadate, $Pb_2(VO_3)_3$.—Obtained as
a light yellow insoluble powder on precipitating the tribasic sodium salt with a soluble lead salt; it yielded on analysis 11.75 per cent. of vanadium, the calculated quantity being 12.04 per cent.

2. Vanadinite, the Double Orthovanadate and Chloride of Lead, $3\text{Pb}_2\text{VO}_4 + \text{PbCl}_2$, can be artificially prepared by fusing for a few hours a mixture of vanadic acid, oxide of lead, and chloride of lead, in the above proportions, together with an excess of sodium chloride. After cooling, a greyish crystalline mass is left, containing cavities filled with long crystals having the same colour as the mass, which under the microscope could be distinguished as six-sided prisms. The crystalline powder is then boiled with water until no further traces of soluble chlorides are extracted.

The following analysis shows that this substance has the same composition as the vanadinites from Zimapan and Windischkappel, analyzed by Berzelius and Rammelsberg*:

<table>
<thead>
<tr>
<th></th>
<th>Natural vanadinite.</th>
<th>Zimapan, Windischkappel,</th>
<th>Artificial vanadinite.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lead</td>
<td>73.08</td>
<td>70.4</td>
<td>71.20</td>
</tr>
<tr>
<td>Vanadium</td>
<td>10.86</td>
<td>9.77</td>
<td>11.11</td>
</tr>
<tr>
<td>Chlorine</td>
<td>2.50</td>
<td>2.54</td>
<td>2.23</td>
</tr>
<tr>
<td>Oxygen</td>
<td>13.55</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The specific gravity of the artificial vanadinite at 12° C. is 6.707, that of the natural being 6.886.

3. Basic Di-Lead Vanadate, $2(\text{Pb}_2\text{V}_2\text{O}_7) + \text{PbO}$.—This salt is precipitated as a pale yellow powder when acetate of lead is added to a solution of disodium vanadate, the liquid acquiring an acid reaction. It is completely insoluble in water and in dilute acetic acid, but dissolves readily in nitric acid.

<table>
<thead>
<tr>
<th></th>
<th>Calculated.</th>
<th>Mean found.</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\text{Pb}_5$</td>
<td>1035.0</td>
<td>69.92</td>
</tr>
<tr>
<td>$\text{V}_4$</td>
<td>205.2</td>
<td>13.86</td>
</tr>
<tr>
<td>$\text{O}_{15}$</td>
<td>240.0</td>
<td>16.22</td>
</tr>
</tbody>
</table>

1480.2

Silver Vanadates.

1. The Ortho-Silver Vanadate, $\text{Ag}_3\text{VO}_4$, is obtained as an orange-coloured precipitate by mixing a freshly prepared solution of the trisodium salt with a solution of silver nitrate, in which every trace of free acid has been neutralized; unless these precautions are attended to, the precipitate consists of a mixture of the ortho- and pyro-salt. The trisilver vanadate is insoluble in water, but readily dissolves in ammonia and nitric acid. Analysis gave the following results:

* Pyromorphite and apatite have already been artificially prepared by Deville and Caron, and also by Debray, whilst mimetesite has been obtained artificially by Lechartier.
2. The Tetrabasic Silver Vanadate, $\text{Ag}_4\text{P}_2\text{O}_7$, is prepared by mixing a solution of the corresponding sodium salt with a neutral solution of nitrate of silver. It falls as a yellow dense crystalline precipitate, resembling in colour the ordinary phosphate of silver. On dissolving the salt in nitric acid, the silver is precipitated as chloride, and the vanadium determined as $\text{V}_2\text{O}_5$.

Analysis gave:

<table>
<thead>
<tr>
<th></th>
<th>Calculated.</th>
<th>Found (mean).</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\text{Ag}_4$</td>
<td>324.0</td>
<td>73.75</td>
</tr>
<tr>
<td>$\text{V}$</td>
<td>51.3</td>
<td>11.67</td>
</tr>
<tr>
<td>$\text{O}_7$</td>
<td>64.0</td>
<td>14.58</td>
</tr>
<tr>
<td></td>
<td>439.3</td>
<td>100.00</td>
</tr>
</tbody>
</table>

The reactions of the tri- and tetrabasic vanadates of the other metals are then described.

The author has to thank Messrs. Oelhoefer and Finkelstein for the valuable assistance which they have given him in the above investigation.

GEOLOGICAL SOCIETY.

[Continued from vol. xxxix. p. 463.]

November 10th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—


The author described the characters presented by the skull of a small Dinosaurian reptile obtained by the Rev. W. Fox from a Wealden bed at Cowleaze Chine in the Isle of Wight. One of the most striking peculiarities of this skull was presented by the pre-maxillary bone, which seems to have been produced downwards and forwards into a short edentulous beak-like process, the outer surface of which is rugose and pitted. The author remarked upon the known form of the symphysial portion of the lower jaw in the Dinosauria, and indicated that its peculiar emargination was probably destined to receive this beak-like process of the premaxillaries, which may have been covered either by fleshy lips or by a horny beak. The dentigerous portion of the premaxilla bears five small conical teeth. The alveolar margin of the maxilla bears ten teeth,
On the Affinity between the Dinosaurian Reptiles and Birds. 69

which are imbedded by single fangs, and apparently lodged in distinct alveoli. The summit of the crown, when unworn, is sharp and presents no trace of the serrations characteristic of Iguanodon; but it is sinuated by the terminations of the strong ridges of enamel which traverse the outer surface of the crown. The teeth thus present some resemblance to those of Iguanodon; but the author regarded the two forms as perfectly distinct, and named the species under consideration Hypsilophodon Foxii. Of the lower jaw the right ramus is present; but its distal extremity is broken off, and its teeth are concealed. On the outer surface of the lower jaw the centrum of a vertebra is preserved.

The author then referred to a fossil skeleton in the British Museum, which has been regarded as that of a young Iguanodon; it is from the same bed as the skull previously described. The author remarked that, in form and proportions, the vertebrae were quite different from those of Iguanodon, and apparently identical with those of his new genus, as shown by the centrum preserved with the skull: the animal had at least four well-developed toes; and other peculiarities were indicated, which seem to prove that it was quite distinct from Iguanodon. This skeleton the author identified with his Hypsilophodon Foxii, and described its characters in detail, dwelling especially upon the peculiarities of the pelvic bones, which are singularly avian in their structure.

4. "Further Evidence of the Affinity between the Dinosaurian Reptiles and Birds." By Professor Huxley, F.R.S., President.

In this paper the author reviewed the evidence already cited by himself and others (especially Professor E. D. Cope) in favour of the ornithic affinities presented by the Dinosauria, and discussed at length the recently ascertained facts which bear upon this question, some of the most important of which are derived from the species described by him in the preceding paper under the name of Hypsilophodon Foxii. He summed up his paper by a comparison of the different elements of the pelvic arch and hind limb in the ordinary reptiles, the Dinosauria and Birds, and maintained that the structure of the pelvic bones (especially the form and arrangement of the ischium and pubis), the relation between the distal ends of the tibia and the astragalus (which is perfectly ornithic), and the strong enemial crest of the tibia and the direction of its twist furnish additional and important evidence of the affinities between the Dinosauria and Birds.

Mr. Seeley doubted whether these animals should be called Reptiles at all, as they seemed to him to form a group distinct alike from reptiles, birds, and mammals, but occupying an intermediate position. In the hinder limbs of Pterodactylus the analogies were closer with mammals than with birds. He thought it possible that the peculiar structure of the hinder limbs of the Dinosauria was due to the functions they performed rather than to any actual affinity with birds.

The President, in reply, stated that Hypsilophodon, from the
character of its teeth, probably subsisted on hard vegetable food. He expressed a hope that Mr. Fox would allow a closer examination of his specimens to be made. He was unable to agree with Mr. Seeley’s views. He was inclined to think that the progress of knowledge tended rather to break down the lines of demarcation between groups supposed to be distinct than to authorize the creation of fresh divisions.

November 24th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—


The author commenced by referring to the bibliographical history of the Dinosauria, which were first recognized as a distinct group by Hermann von Meyer in 1830. He then indicated the general characters of the group, which he proposed to divide into three families, viz.:—

I. The Megalosauridae, with the genera Teratosaurus, Palaeosaurus, Megalosaurus, Poikilopleuron, Lælap, and probably Euskelosaurus;

II. The Scelidosauridae, with the genera Thecodontosaurus, Hylæosaurus, Pholacanthus, and Acanthopholis; and

III. The Iguanodontidae, with the genera Cetiosaurus, Iguanodon, Hypsilophodon, Hadrosaurus, and probably Stenopelys.

Compsognathus was said to have many points of affinity with the Dinosauria, especially in the ornithic character of its hind limbs, but at the same time to differ from them in several important particulars. Hence the author proposed to regard Compsognathus as the representative of a group (Compsognatha) equivalent to the true Dinosauria, and forming, with them, an order to which he gave the name of Ornithoscelida.

The author then treated of the relations of the Ornithoscelida to other Reptiles. He indicated certain peculiarities in the structure of the vertebrae which serve to characterize four great groups of Reptiles, and showed that his Ornithoscelida belong to a group in which, as in existing Crocodiles, the thoracic vertebrae have distinct capillary and tubercular processes springing from the arch of the vertebra. This group was said to include also the Crocodilia, the Anomodontia, and the Pterosauria, to the second of which the author was inclined to approximate the Ornithoscelida. As a near ally of these reptiles, the author cited the Permain Parasauro, the structure of which he discussed, and stated that it seemed to be a terrestrial reptile leading back to some older and less specialized reptilian form.

With regard to the relation of the Ornithoscelida to birds, the
Mr. Duncan on the Physical Geography of Western Europe. 71

author stated that he knew of no character by which the structure of birds as a class differs from that of reptiles which is not fore-shadowed in the Ornithoscelida, and he briefly discussed the question of the relationship of Pterodactyles to birds. He did not consider that the majority of the Dinosauria stood so habitually upon their hind feet as to account for the resemblance of the hind limbs to those of birds by simple similarity of function.

The author then proceeded to notice the Dinosauria of the Trias, commencing with an historical account of our knowledge of the occurrence of such reptilian forms in beds of that age. He identified the following Triassic reptilian forms as belonging to the Dinosauria:—Teratosaurus, Plateosaurus, and Zanclodon from the German Trias; Thecodontosaurus and Palaeosaurus from the Bristol conglomerate (the second of these genera he restricted to P. cylindrodon of Riley and Stutchbury, their P. platyodon being referred to Thecodonto-saurus); Cladiodon from Warwickshire; Deuterosaurus from the Ural; Ankistrodon from Central India; Clepsysaurus and Bathynathus from North America; and probably the South-African Proteosaurus.

2. "The Physical Geography of Western Europe during the Mesozoic and Cainozoic periods, elucidated by their Coral-faunas." By P. Martin Duncan, M.B.Lond., F.R.S., Secretary.

The author commenced with a notice of the typical species of the coral-fauna of the deep seas which bound continents remote from coral-reefs, and then made some remarks upon the littoral corals. The peculiarities of reef, lagoon, and shallow-water species were then explained, with the relations of the two faunas to one another. The author then referred to certain exceptional species, indicated the genera the species of which constitute the existing reefs and contributed to form those of the past, and noticed the representatives of some modern genera in old reefs. He pointed out that a correspondency of physical conditions during the deposition of certain strata was indicated by their containing analogous forms—the presence of compound cœnenchymal species indicating neighbouring reefs, and their absence in places where simple or non-cœnenchymal Madreporaria are found being characteristic of deep-sea areas remote from the Coral-seas. By applying the principles thus elaborated to the evidence as to the condition of the seas of the European area from the Triassic period to the present time, the author then showed what must probably have been the physical condition of this part of the world at different periods.

December 8th, 1869.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "Notes on the Brachiopoda hitherto obtained from the Pebble-bed at Budleigh-Salterton, near Exmouth, in Devonshire." By Thomas Davidson, Esq., F.R.S., F.G.S., &c.

The author first described the general characters, and discussed
the opinions that have been put forward as to the origin, of the pebbles forming this bed. Nearly 40 species of Brachiopoda have been obtained from them. The fossils contained in the pebbles have been regarded as of Lower-Silurian age; the author considered the great majority of the Brachiopoda to be Devonian. The species identified with Silurian fossils are:—Lingula Lesueurii (Rouault), L. Rouaulti (Salter), and L. Hawkei (Rouault). The species regarded by the author as undoubtedly Devonian (i.e. either previously described from Devonian deposits or associated with such species in the same pebble) are 12 in number, namely:—Spirifer Verneuilii (Murch.), S. macroptera (Goldf.), Athyris budleighensis (sp. n.), Atrypa (recticularis?), Rhynchonella inauria (Sandb.), R. elliptica (Schnurr), R. Vicaryi (sp. n.), and 2 undetermined species of Rhynchonella, Streptorhynchus crenistria (Phil.), Productus Vicarii (Salt.), and an undetermined Chonetes. Eight species occurring in the same rock, three of which have been doubtfully identified, are considered by the author to be probably Devonian. The species supposed to be determined are:—Orthis redux (Barr.) and O. Berthosi (Rouault), Silurian; and Spirifer octoplicatus (Sow.), Devonian and Carboniferous, but possibly identical with the Silurian S. elevatus (Dalm.). The others are 2 new species of Orthis, and a new Rhynchonella (?), and an undetermined species of Terebratula (?) and Strophomena (?). Finally the author noticed 14 species (all new, except Orthis pulvinata, Salt.) only known from these pebbles, but which were stated to possess a Devonian facies.


The author described the Yorkshire glacial clays as of two kinds:—the lower, containing chalk débris, and belonging to the uppermost member of the glacial series in Eastern and East-central England; the upper, containing chalk sparingly in its lower part, and gradually losing this upwards. On the coast only the latter occurs north of Flambro'. He stated that, palaeontologically, the Lower and Middle glacial deposits closely agree with the Crag, and are quite distinct from the deposit at Bridlington, which he placed immediately above the "Great Chalky Clay."

The absence of chalk débris in the deposit north of Flambro' has been regarded as evidence of a drift from north to south; but the author stated that the purple clay without chalk extends over much of the north-eastern part of the Wolds, from the sea-level to an elevation of 450 feet, and that outliers of it occur at intervals along the Holderness coast-section as far as Dimlington, 42 miles south of the northern limit of the Wolds. In the direction of Flambro' and York the clay was said to be destitute of chalk, which would not be the case had the Wolds formed a sea-shore causing a drift from the north to pass either to south-east or south-west.

The author described the characters of the Great Chalky Boulder-clay in the eastern and central counties of England, and maintained
that the chalk found in it (equal, according to him, to a layer of at least 200 feet over the entire Wold) could only have been detached by the agency of moving ice, which he believed to have covered nearly the whole Wold for a long period.

The author stated that boulders of Shap Fell granite are confined to the deposit of clay without chalk, and discussed the means by which they could have been distributed. He ascribed their dispersion to the agency of floating ice during an adequate submergence of the district. He supposed them to have passed from Shap Fell by what is now the pass of Stainmoor.

Thus he ascribed the formation of the "Great Chalky Clay" to the extrusion from the sea-foot of a great sheet of ice, of materials abraded by the latter, the land being depressed 600–700 feet below its present level; and that of the clay without chalk and with boulders of Shap-Fell granite to deposition during a period of much greater depression (about 1500 feet), throughout which the sea bore much floating ice. He considered that the "Great Chalky Clay" indicated a long period during which the land, with its enveloping ice, remained stationary, and that during this period, when intense cold prevailed, the arctic fauna of Bridlington became established. He thought that the recommencement of subsidence was indicated by the reddish-brown or brownish-purple sediments of Holderness, in which some chalk occurs. He then indicated the species of Mollusca which have occurred in the purple clay without chalk about Scarbro' and Whitby, all of which were said to belong to existing forms, and thus to be in accordance with the date assigned by him to that deposit. The molluscan fauna of Moéil Tryfane was referred to by the author, who stated that he regarded it as belonging to the period of emergence from the deepest depression, during which the clay without chalk was assumed to have been deposited, i.e. to the earliest part of the post-glacial period, to which the stratified drifts of Scotland are referred by Mr. A. Geikie.

December 22, 1869.—Professor Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:


The authors introduced their account of the iron-ores of the Antrim basalts by stating that since 1790 an iron band had been known in the midst of the basalt of the Giant's Causeway, but that only within the last few years have further discoveries been made, which have developed a new branch of industry in the north-east of Ireland.

The iron-ore of the numerous exposures was considered to represent portions of one sheet extending uniformly throughout the basalt and over a very large area. Indeed everywhere the iron band and
its associated rock-masses present identical features, from which the authors deduced the following generalized section:

The underlying basalt gradually passes upwards into a variegated lithomarge of about 30 feet thick, graduating insensibly into a red or yellow ochre or bole of about 5–6 feet in thickness, which passes into a dense red ochreous mass of about 2 feet, charged with ferruginous spheroids consisting chiefly of a protoxide and peroxide. The spheroids are of the average size of peas; they increase in number and size towards the upper part of the band, and not unfrequently constitute that portion of it. The line of junction between the iron band and the overlying and usually more or less columnar basalt is in all cases well defined, and in a few instances exhibits decided unconformability.

The authors discussed the several theories that may be suggested to account for the origin of the present condition of the pisolitic ore, and proceeded to point out what appeared to have been the several stages of metamorphic action by which the pisolitic ore had been elaborated out of basalt. From field observations and chemical analyses, they have been led to consider the bole and lithomarge to be the resultants of aqueous action in combination with acidulated gases, which, dissolving out certain mineral substances, has effected the decomposition of the basalts,—and to assume that the bole underlying the iron band was a wet terrestrial surface, and that the subsequent outflow of basalt effected, by its heat, pressure, and evolved gases, a reduction of the contained oxides of iron into the more concentrated form in which they occur in the pisolite, the aggregation of the ferruginous particles being a result of the same actions.

The ferruginous series, with interstratified plant-beds, at Ballypalidy was next described, and demonstrated to be of sedimentary origin—the ferruginous conglomerate resulting from the degradation of the pisolitic ore, of which it is chiefly reconstructed, and of the underlying ochres.

Many additions were made to the list of plant-remains from these beds; and priority of discovery of plants in the Antrim basalts was accorded to Dr. Bryce, F.G.S.


In this paper the author criticised the statements of Mr. Carruthers on the structure of Sigillaria (see Q. J. G. S. xxv. p. 248). He remarked that Sigillaria, as evidenced by his specimens, is not coniferous; that the coniferous trunks found in the Coal-formation of Nova Scotia do not present discigerous tissue of the same type as that of Sigillaria; that no Conifer has a slender woody axis surrounded by an enormously thick bark; that Calamodendron was probably a Gymnosperm, and allied to Sigillaria; that although Stignaria may not always show medullary rays, the distinct separation of the wood into wedges is an evidence of their having ex-
On a Crocodilian Skull from Kimmeridge Bay, Dorset.

ister; that the difference in minute structure between Sigillaria and Stigmaria involves no serious difficulty if the former be regarded as allied to Cycadaceae; and, further, that we do not know how many of the Stigmaria belong to Sigillaria proper, or Favularia, or to such forms as Clathraria and Leioderma, which may have been more nearly allied to Lepidophloios; that the fruit figured by Goldenberg as that of Sigillaria is more probably that of Lepidophloios, or may be a male catkin with pollen; and that he has found Trigonocarpa scattered around the trunks of Sigillaria, and on the surface of the soil in which they grew. He agreed with Mr. Carruthers in regarding Mr. Binney’s Sigillaria vascularis as allied to Lepidodendron.

Prof. Morris thought that Clathraria and Lepidophloios ought to be discriminated from the Sigillaria, as being rather more nearly allied with cycadaceous plants, especially the former. He pointed out the manner in which certain vascular bundles communicating between the centre of the stem of Sigillaria and allied genera and their bark might be mistaken for medullary rays.


The author described the characters presented by the lower jaw of an Amphibian, of which a cast had occurred in the coarse sandstone of the Coal-formation between Ragged Reef and the Joggins Coalmine. It measured 6 inches in length; its surface was marked on the lower and posterior part with a network of ridges enclosing rounded depressions. The anterior part of the jaw had contained about 16 teeth, some of which remained in the matrix. These were stout, conical, and blunt, with large pulp-cavities, and about 32 longitudinal striæ, corresponding to the same number of folds of dentine. The author stated that this jaw resembled most closely those of Baphetes and Dendrerpeton, but more especially the former. He regarded it as distinct from Baphetes planiceps, and proposed for it the name of B. minor. If distinct, this raises the number of species of Amphibia from the Coal-measures of Nova Scotia to nine.

The author also noticed some insect-remains found by him in slabs containing Sphenophyllum. They were referred by Mr. Scudder to the Blattariae.

From the Devonian beds of Gaspé the author stated that he had obtained a small species of Ophalaspis, the first yet detected in America. With it were spines of Machairacanthus and remains of some other fishes. At Gaspé he had also obtained a new species of Psilophyton, several trunks of Prototaxites, and a species of Cyclostigma.

4. “Note on a Crocodilian Skull from Kimmeridge Bay, Dorset.”

By J. W. Hulke, F.R.S., F.G.S.

The author described a large Steneosaurian skull in the British
Museum, from Kimmeridge Bay, which had been previously regarded as Pliosaurian, and was recently identified with Dakosaurus by Mr. Davies, Sen. From the agreement of their dimensions, and their occurrence near together, the author thought it probable that this skull and the lower jaw described by him last session belonged to the same individual. It differs from the Steniosaurus rostro-minor in the greater stoutness of its snout, in the presence of an anterior pair of nasal bones prolonged into the nostril, and in the number of its teeth. The author proposed to name it Steniosaurus Manseli, after its discoverer.

5. "Note on some Teeth associated with two fragments of a Jaw, from Kimmeridge Bay." By J. W. Hulke, F.R.S., F.G.S.

The author described some small teeth associated with fragments of a long slender snout not unlike that of an Ichthyosaur, but too incomplete to be certainly identified. The teeth are peculiar in the great development of the cementum, which gives the base of the tooth the form of a small bulb. The exserted crowns are slightly curved, smooth, cylindrical, and pointed. The attachment to the dentary bone was probably by means of the soft tissues; and the teeth seem to have been seated in an open groove in the surface of the jaw-bone. Until additional material reveal the true nature of this fossil, the author proposes to place it alone, and to call it provisionally Euthekiodon.

X. Intelligence and Miscellaneous Articles.

EXPERIMENTS ON THE VELOCITY OF THE PROPAGATION OF SOUND IN WATER IN A CAST-IRON CONDUIT 8 DECIMETRES IN DIAMETER. By M. FR. ANDRÉ.

HAVING been commissioned by the École des Ponts et Chaussées to superintend the supply-works of the canal from the Aisne to the Marne, I had occasion to assist at the laying down of a tubular conduit intended to conduct the water from the pumping-works to the head of the supply reservoir. This conduit consisted of cast-iron tubes of 0.8 metre internal diameter and 0.02 metre thick, joined together by sockets and bands, and formed altogether a straight line of about 600 metres. The difference of level between the two ends of the conduit was 17.23 metres.

In order to test the joints of the tubes, the conduit had to be filled with water and the latter subjected to a pressure of 8 atmospheres. It occurred to me that this experiment afforded a good opportunity of making some fresh measurements of the velocity of the propagation of sound in water. The conditions under which I operated were as follows.

To record the motions of the liquid, instead of using electric registers, the fixing of which is always difficult and costly, I made use of a pneumatic register, which physiologists, and particularly M.
Marey, have of late frequently used. The disturbance communicated itself to the air confined in a small caoutchouc tube, and thence to a membrane of goldbeater's skin. A very delicate lever fastened to this membrane indicated by its oscillations the slightest movements of the liquid.

The time was calculated by help of a tuning-fork, which inscribed its vibrations on the blackened sheet of a registering cylinder. This tuning-fork, which was verified several times, gave, for a temperature of 20°, 256 vibrations in a second.

Before experimenting on the conduit filled with water, I performed a series of experiments on the velocity of the propagation of sound in air, in order to ascertain the degree of correctness this method admits of. The apparatus, except some slight modifications, remained the same. The sound was produced by a pistol charged with about one gramme of powder. The shock communicated to the air of the conduit was propagated through the whole length of the tubes, and then returned, after reflection. At each successive departure and return the small style of the membrane gave very distinct indications on the registering cylinder. As the initial shock and the reflected shocks were observed, the causes of error due to the inertia of the register were eliminated.

The greatest difficulty consisted in determining the temperature of the air enclosed in the conduit. The tubes were laid in an open trench; and whilst their upper part, heated by the sun's rays, had a temperature of 40°, the part in contact with the ground had one of 20° only.

Taking these two numbers as extreme limits of the temperature of the air confined in the conduit, I found for the velocity of sound, reduced to zero,

\[ V_o = 326.60 \text{ metres (supposing the temperature 40°)}, \]
\[ V_o = 337.50 \text{ metres (supposing the temperature 20°)}. \]

It is certain that the first number must be nearer the truth than the second; for the part of the tubes exposed to the sun was considerably greater than that in contact with the ground.

I now come to the experiments made on the velocity of sound in the conduit filled with water. After first assuring myself that the interior was absolutely void of air (which is easily done by examining the joints), I fitted an hydraulic-press pump to the upper part of the conduit. The shock in the water was caused by forcing in the piston suddenly. With whatever rapidity the lever of the pump was lowered, no shock, properly so called, was produced, but a gradual compression; thus the indication of the style upon the register, instead of being a well-defined zigzag, as in the case of air, traced an elongated curve, of which the point of coincidence with the spiral inscribed by the style at rest was difficult to determine. However, four successive experiments gave a mean of 345 vibrations of the tuning-fork between the initial and the return shocks. The length of the conduit between the two plates which closed its extremities was 603.25 metres; hence the distance travelled by the compression, between
departure and return, was 1206.5 metres. The temperature of the water at the top of the conduit was 20°, and 13° at the bottom. The temperature of the surrounding air was 18°. Under these conditions the velocity of the propagation of the compression was found to be 897.80 metres per second.

The second and the third return shocks were too feeble to enable us to deduce any accurate measure.

Wertheim deduced, from the sound given by brass organ-pipes dipping in water, 1173 metres per second as the velocity of sound in water. This number is much less than 1435 metres per second, found by MM. Colladon and Sturm in direct experiments made on the Lake of Geneva.

The value which I found is still further from the number observed in an indefinite mass of water. Notwithstanding this divergence, which I do not pretend to explain, I think it useful to give my results, which subsequent studies may confirm.

I confine myself to calling the attention of physicists and geometers to the influence which the elasticity and friction of the containing sides may have on the propagation of a shock in the midst of an almost incompressible fluid. Probably the difference between the propagation of a shock in an indefinite medium and that which we observed in a cast-iron cylinder is due to this circumstance.—Comptes Rendus, March 14, 1870.

Experimental Researches on the Duration of the Electric Spark. By MM. Lucas and Cazin.

The apparatus which we use to measure with accuracy the durations of electric sparks depends essentially on an application of the vernier.

A mica disk, 15 centims. in diameter, blackened on one face by a photographic process, and divided near its edge into 180 equal parts by means of transparent marks, is mounted on a horizontal axis, which may be made to rotate with a velocity varying from 100 to 300 revolutions in a second. A crank and gear govern this rapid motion. For one turn of the handle the mica disk makes $66\frac{1}{2}$ turns.

Another disk, of the same radius and centred on the same horizontal axis, is fixed vertically, as near as possible to the moveable disk. It consists of silvered glass, and has, towards the summit of its vertical diameter, six transparent marks, which form a vernier, in order to estimate the sixth of the interval between two consecutive marks on the mica disk.

The two disks are enclosed in a circular box of blackened copper. The vernier forms the bottom of it, on the side of the source of light. The mica disk turns in the interior. On the side of the observer there is a metal plate for a cover; a small aperture, provided with a plate of glass, is arranged opposite to the vernier in order to admit of observations. In this manner the moveable disk is preserved from dust, protected against shocks, and sheltered from currents of air.

The general appearance of this chronoscope, which has been very
Intelligence and Miscellaneous Articles.

cleverly constructed by M. Duboseq, recalls the apparatus devised and used by M. E. Becquerel for his important investigations on the phosphorescence of bodies.

Instead of the handle, a wooden pulley of several grooves is substituted, over which a catgut cord passes, which also passes round another pulley of a much larger diameter fixed on the fly-wheel of a gas-engine. This machine, which was very obligingly lent by its inventor, M. Hugon, is of half a horse-power. It works with great regularity; it may be started and stopped almost instantaneously; and unlike steam-engines, the pressure need not be maintained during the times of stoppage. Thus it is of excellent service.

To charge the Leyden battery in which the electricity is condensed, one of Holtz’s machines is used, the plate of which is put in motion by the gas-engine. The sparks pass between two metal knobs 11 millims. in diameter. At exactly half the distance between these two knobs is formed the principal focus of the lens of a collimator, so that the luminous rays fall perpendicularly on the vernier. The aperture of the chronoscope is viewed through a magnifying-glass.

Suppose that the electric spark occurs periodically under precisely the same conditions, whilst the mica disk turns almost uniformly. An observer looks through the eyeglass of the telescope and calls out the number of marks which he observes simultaneously with each spark. Another observer registers these numbers, and counts the number of turns which the handle of the chronoscope makes per minute.

Let \( N \) be the number of sparks observed, \( S \) the total number of marks read, \( n \) the number of turns of the handle.

The duration \( y \) of the spark, in millionths of a second, is given by the formula

\[
y = \frac{10000}{12n} \left( \frac{S}{N} - \mu \right), \quad \ldots \quad (1)
\]

in which \( \mu \) is a constant parameter, equal to 0·70 for our apparatus. If \( \epsilon \) denote the angular breadth of the marks on the mica disk, \( \omega \) the angle between the axes of two consecutive marks, and \( \epsilon' \) the angular breadth of the marks of the vernier, then

\[
\mu = \frac{6(\epsilon + \epsilon')}{\omega}, \quad \ldots \quad (2)
\]

Formula (1) assumes \( N \) to be a large number; thus, we usually observe series of a hundred sparks.

Other things being equal, the duration of the electric spark is a function of the surface of the Leyden battery, or, in other words, of the number of jars which compose it.

By varying this number \( x \) by units from 1 to 9, we found that the duration \( y \) may be expressed by the formula

\[
y = k(1 - a^x), \quad \ldots \quad (3)
\]
Intelligence and Miscellaneous Articles.

With two zinc knobs, 2.292 millims. apart, the following results were obtained:

\[
\log a = \frac{1}{4.5050453}, \quad \ldots \quad \log k = 1.5192181
\]

or

\[
a = 0.80361, \quad k = 33.05355
\]

\[\begin{array}{cccc}
x & S. & N. & 12a. \\
1 & 52.1 + 143.2 + 5.3 = 353 & 200 & 1402 & 7.45 & 6.49 & 0.96 \\
2 & 10.1 + 66.2 + 24.3 = 214 & 100 & 1215 & 11.85 & 11.71 & 0.14 \\
3 & 37.2 + 63.3 = 263 & 100 & 1215 & 15.85 & 15.90 & 0.08 \\
4 & 7.2 + 92.3 + 11.4 = 334 & 100 & 1212 & 19.30 & 19.27 & 0.03 \\
5 & 63.3 + 37.4 = 337 & 100 & 1216 & 21.69 & 21.98 & -0.38 \\
6 & 50.2 + 50.3 = 250 & 100 & 756 & 23.81 & 24.15 & -0.34 \\
7 & 22.3 + 67.4 + 11.5 = 389 & 100 & 1236 & 25.81 & 25.90 & -0.09 \\
8 & 20.2 + 71.3 + 9.4 = 290 & 100 & 792 & 27.52 & 27.31 & 0.21 \\
9 & 17.2 + 71.3 + 12.4 = 295 & 100 & 788 & 28.57 & 28.43 & 0.14 \\
\end{array}\]

It is seen that the difference between the duration observed and the duration calculated has not reached the millionth part of a second.

With the same zinc knobs 5 millims. apart, we found

\[y = k'(1 - a^x), \quad \ldots \quad \ldots \quad \ldots \quad \ldots (6)\]

\(a\) having the same value as above, whilst

\[
\log k' = 1.8226921, \quad \ldots \quad \ldots \quad \ldots (7)
\]

Hence the parameter \(a\) is independent of the striking-distance.

The jars which constitute our battery have an external coated surface of about 1243 square centimetres.

Other physical laws not less important are already brought out by our investigations; more remain to be discovered, and will form the object of subsequent studies. We shall have the honour to communicate to the Academy the results which may be obtained.

These researches were made at the Imperial Observatory at Paris, thanks to the kindness of the Administration, who were good enough to place a spare room at our disposal.—Comptes Rendus, April 25, 1870.
X1. Memoir on Internal Work in Gases.
By M. Achille Cazin*.

[With a Plate.]

* Translated from the Annales de Chimie et de Physique, January 1870, having been communicated to the Académie des Sciences, March 9, 1868.

internal work in a given change of state is evidently of very
great importance in the establishment of a law of molecular forces.
The mechanical theory of heat gives us the means of discovering
this sign in a great number of cases.

It is generally admitted that, in a material system, positive
work caused by the use of heat only is correlative to the ap-
pearance of a number of thermal units proportional to this work,
and that negative work is correlative to the disappearance of
a like number of thermal units. By saying that work is con-
verted into heat in the first case, and heat into work in the
second, we express this correlation in a convenient manner for
drawing conclusions. Hence when a body experiences a change
such that some internal work has been effected and a quantity
of heat has disappeared, without revealing any other pheno-
menon, mechanical or thermic, this work must be negative.
This is what happens in the melting of ice when it is occasioned
by the disappearance of a certain quantity of heat coming from
external bodies, and when no pressure is exerted on the surface
of the ice. In the most general case, where heat disappears ex-
ternally to a body whilst some of it appears internally, and ex-
ternal and internal work are effected at the same time, we shall
be able to deduce from the measure of the first three quantities
the value and sign of the last.

In gases we are accustomed to take into account only the ex-
ternal work, and can hardly conceive attractive molecular forces.
For a long time we have imagined these bodies as assemblages
of molecules mutually repelling each other. This was a way of
explaining their expansibility; and the repulsion was attributed
to caloric when this agent was invoked in order to interpret
the effects of heat. The mechanical theory of heat rejects this
explanation, and leads to other hypotheses on the constitution
of gases. By supposing that the molecules of gases are en-
dowed with certain motions, we easily explain the expansibility
and the principal properties of this kind of bodies. Although
these hypotheses cannot express the truth so long as they are
devised with the object of explaining approximate laws, such as
those of Mariotte and Gay-Lussac, they are nevertheless impor-
tant; for they disengage us from the old ideas, and certainly pre-
pare other, more complete hypotheses, which will be suggested
by a more profound knowledge of the phenomena.

Thus it is especially to experiment that we must have recourse,
if we wish to possess exact notions on the work in gases. As every
thing leads to the belief that the molecular forces in these bodies
are simply directed according to the lines of the centres, it may
be supposed that they are subject to a more simple law than
those of solids and liquids, and it may be hoped that this law will
be more easily discovered.
Many experimental researches have been made on this subject; but, whether because they have led us to regard the internal work of gases as very slight, or because they have not appeared sufficiently conclusive, several authors who have developed the mechanical theory of heat have only applied its principles to an ideal gaseous state, defined by the complete absence of all internal work, or else, under another form, by the laws of Mariotte and Gay-Lussac. Hence it follows that the formulæ given for gases are far from representing the real phenomena, and that it is frequently difficult to appreciate the degree of approximation which their use admits of. This is not of much importance in technical applications; but from a speculative point of view I think it is very important to abandon altogether formulæ the inexactness of which is certain.

I will endeavour briefly to recall the facts upon which the notion of internal work in gases is based.

When we apply the principles of the mechanical theory of heat to gases assuming that internal work is never produced, we demonstrate that the gas follows the laws of Mariotte and Gay-Lussac*. Now the two fundamental principles of this theory are:

1. The principle of the equivalence of heat and work, indicated for the first time, in France, by M. Marc Séguin†, developed by Dr. Mayer in Germany‡, and demonstrated experimentally first by Mr. Joule in England (1843), then by a great number of French and other physicists: this principle cannot now be doubted, and the divergencies of opinion relate only to the numerical value of the ratio of equivalence.

2. A principle conceived by Sadi Carnot (1824) at a time when the preceding principle was unknown, and which was afterwards rectified by Sir W. Thomson in England (1849), and Clausius in Germany (1850). This latter principle has generally been regarded as distinct from the first; not being capable of a direct experimental verification, it constituted a kind of postulate the correctness of which was demonstrated by the experimental verification of all its consequences. M. Clausius has in fact demonstrated it by combining the principle of equivalence with the following principle, which he has assumed as an axiom:—Heat cannot pass of itself from one body into another the temperature of which is higher—that is to say, without there being at the same time a conversion of work into heat, or vice versa.

† Marc Séguin, De l'influence des Chemins de fer, 1839.
Sir W. Thomson has substituted for this principle the following, which he has also taken as axiomatic:—It is impossible to obtain work from the heat of a body without having another body at a lower temperature to take from it a part of this heat. These two principles do not constitute true axioms; they are, rather, experimental facts. M. Hirn has recently given a new demonstration of the second fundamental principle, in which he seeks to deduce it from the first principle without making any hypothesis*. But even if this demonstration should rest, like M. Rankine's, on some hypothesis, the truth of the second fundamental principle cannot be doubted, since all its consequences have hitherto been verified by experiment.

The bases of the mechanical theory being perfectly verified, it remains to be known whether the laws of Mariotte and Gay-Lussac are sufficiently so. Now the celebrated experiments of M. Regnault demonstrate their incorrectness. They can only be accepted approximately for gases not liquefiable; and they must be completely rejected, even in technical applications, for the other gases and superheated vapours, the use of which in thermal motors cannot fail to spread now that the successes of M. Hirn in this direction have been verified†.

Hence the experiments of M. Regnault, and all those which have suggested the two fundamental principles of the mechanical theory, lead to the assumption of the existence of internal work in gases. It remains to us to examine the direct demonstrations of this property of gases.

Everybody at the present day knows Mr. Joule's celebrated experiment in which a reservoir containing compressed gas is put in communication with an empty reservoir‡. It is evident that the gas undergoes a change of volume and of pressure without there being any external work effected. Consequently a negative internal work will be accompanied by the disappearance of a certain quantity of heat; and if, the reservoirs being surrounded by water, we observe a decrease in temperature, we may from this conclude that the operation gives rise to a like amount of work. Theoretically this experimental process is not open to objection; yet it is not sufficiently delicate, and it does not give definite results. From this it has naturally been concluded that the internal work of gases may be neglected.

M. Hirn has tried to increase the delicacy of the apparatus by

† "Recherches expérimentales sur les machines à vapeur," par M. G. Lelouvre, Bulletin de la Société industrielle de Mulhouse, 1866.
‡ Phil. Mag. May 1845.
transforming it into a kind of gas-thermometer*. In order to do this he divided a cylinder into two compartments by means of a membrane, and adapted an oil-manometer to it. The gas being at first under the same pressure in the two compartments and in the manometer, the latter was closed, and by means of a pump the gas was made to pass from one of the compartments into the other. The membrane was then burst by allowing a weight to fall upon it, and the manometer was immediately opened. No displacement in the levels of the manometric liquid was observed; so that the apparatus revealed neither a creation nor a disappearance of heat. We shall see in the course of this memoir how complicated is the phenomenon; I will only at present remark that Messrs. Joule and Hirn only sought to observe the final thermic effect.

It is also in order to investigate the final effect that the formulae of the thermodynamic theory have been applied to this phenomenon. M. Bauschinger has published a long treatise on this subject†. But he has assumed Mariotte and Gay-Lussac’s laws, and consequently all the formulae which he has given are not verified by experiment.

Messrs. Joule and Thomson calculated the final thermal effect in question without assuming Mariotte and Gay-Lussac’s laws‡. They found that there must be a fall of temperature of 2°.8 for air under a pressure of twenty-one atmospheres when it was allowed to flow into an empty vessel of the same capacity as the first. As to the thermometric effect produced in the water which surrounded the apparatus, it was required to be only 0°.003, a quantity almost inappreciable.

There is another mode of observing the thermal effects which accompany the expansion of gas when the external work is none, or at least very little, and when the external calorific action may be neglected. It has been employed by Messrs. Joule and Thomson for various gases§, and by M. Hirn for aqueous vapour||. The compressed gas flowed into the atmosphere under a constant pressure through one or more small orifices. At a certain distance from the orifice it impinged upon a thermometer-bulb properly sheltered, and the current was kept up sufficiently long for the thermometer to become stationary. A fall of temperature was always observed. Messrs. Joule and Thom-

* G. A. Hirn, Exposition analytique et expérimentale de la Théorie mécanique de la Chaleur, p. 52 (1855).
† In Schliomich’s Zeitschrift für Mathematik und Physik, vol. viii.
|| G. A. Hirn, Exposition analytique et expérimentale de la Théorie mécanique de la Chaleur, p. 177 (1865).
son obtained results which accord with the thermodynamic formulae: for carbonic acid the fall was 8°-33 for an excess of pressure of 6·95 atmospheres at the ordinary temperature. For aqueous vapour at a temperature of 244° and with an excess of pressure of 6 atmospheres, M. Hirn obtained 233° after the expansion, and consequently a fall in temperature of 11°. He also found a satisfactory agreement between his results and the formula to which he had been led.

This method is not free from every objection; it will be completely discussed in the course of this memoir.

These two methods are certainly useful in revealing the existence of attractive molecular forces in gases; but in practice a host of circumstances may complicate the phenomena, and it appears to me that it will be useful to submit them to new tests. The experiments which I am about to describe will show how the expansion takes place in the case of two reservoirs. They form a sequel to the experiments which I have described in the Annales de Chimie et de Physique*. I observed at that time the passage of gas from one reservoir to the other when the excess of pressure was small, and I devised a method for observing the condition of the gas at each moment during the passage. By using at the present time an analogous method, I have succeeded in observing all the particulars of the flow of gas under a great pressure. Hence this method differs essentially from those methods usually employed. Instead of simply investigating the pressure which is established after the motion of the gas has completely ceased, I investigated it at several stated periods during the motion; and by the comparison of various observations I deduced the approximate time of the cessation of the motion, and the pressure of the gas at that time. This is evidently a suitable method whenever a temporary phenomenon has to be observed in which the quantities we wish to measure vary very rapidly with the time. By this contrivance phenomena as fugitive as the movements of a gaseous mass are rendered accessible to observation.

I shall devote the first part of this memoir to a description of the experiments, and to the conclusion which may be immediately deduced from them without having recourse to any mathematical theory. In the second part I shall apply the thermodynamic formulae to the explanation of the facts observed.

PART I.—EXPERIMENTAL RESEARCHES.

§ I. Principle of the method.

The gas is enclosed in two reservoirs at the same temperature, connected by a large stopcock; one of them can communicate with an open-air manometer. The stopcock being open, communication with the manometer is first established; then the stopcock is closed, communication with the manometer is cut off, and by means of a pump a portion of the gas is made to pass from one reservoir to the other. To make an experiment the stopcock is opened; the compressed gas begins to move, and communication is established with the manometer at a stated time from the moment when the opening of the stopcock commenced. The displacement of the level of the manometric liquid is followed, and its successive positions are noted at stated periods. A curve can thus be traced, of which the abscissæ measure the periods, and the ordinates the excesses, positive or negative, of the hydrostatic pressure of the liquid in motion over the final and stationary pressure of the liquid at rest. When certain conditions are satisfied we generally obtain a curve $a b c d e$ (Plate I. fig. 1).

The abscissa $o a$ indicates the time which elapsed from the opening of the stopcock to that of the manometer; the segment of the curve $a b c$ denotes that the variable pressure of the gas was at first less than the final pressure $p'$; the segment $c d e$ indicates that this pressure afterwards became greater and then gradually decreased to $p'$. A series of experiments are made by changing the time $o a$, all other circumstances remaining the same, which changes the position of the single points of the curve; and by comparing the various curves belonging to the same series, we deduce the succession of pressures which the gas possessed during the motion in the reservoir which communicated with the manometer.

§ II. Description of the apparatus.

The metal reservoirs A and B* (fig. 2), connected by a large brass stopcock C, are fixed horizontally on a solid support; they have a double envelope, and the circulation of water ensures to their sides a constant temperature; the gas flows through a short and thick tube (length 28 centims., minimum diameter 4 centims.). On each side of the stopcock are tubes (D, E) provided with stopcocks and connectors in order to establish various communications. The axis of the key of the large stopcock C

* The reservoir A was of copper; it had a capacity of 8'923 litres at 18°. The reservoir B was of very strong zinc; some iron hoops were soldered to the interior; the capacity was 33'805 litres at 15°; the connecting-pipe contained 0'140 litre.
is maintained in a horizontal position by an iron support F, fixed to a table on which all the apparatus stands. The stopcock is turned by means of a lever, G G.

In order to introduce the gas into the reservoirs, a vacuum is created by using the tubulure H, for instance; the gas is then allowed to enter, being dried in its passage; and this double operation is repeated a second time, as usual.

The small tube I connects the reservoir A with the open-air manometer K K. The height of the mercury in this manometer is read by the aid of a copper scale 4 metres in height, divided into millimetres, fixed vertically along the open branch of the manometer, and provided with two verniers.

The small tube L connects the reservoir B with the open-air manometer M M; the difference of the levels is measured by a cathetometer.

To the tubes I K, L M, are connected, by means of three-way tubes, tubes N O, P O, which terminate in a gas-pump. This pump serves to pass a portion of the gas from reservoir B to reservoir A.

The manometer which is intended to follow the change of pressure of the gas during the expansion, consists of a glass tube of two vertical branches, Q Q; one of these has a cylindrical enlargement, within which the interior level of the liquid always remains; the displacements of the exterior level are noted along a vertical scale. The liquid varies with the nature of the gas. The communication of this manometer with the reservoirs is by means of the small reservoir R, which is firmly fixed on the table; it consists of a small copper cylinder divided into two compartments, and contains a conical valve the rod of which passes through a stuffing-box (fig. 3). The upper compartment communicates with the tube S of the reservoir B (fig. 2), and the lower compartment with the manometer Q. The valve is closed by the pressure of a spring; and in order to open it considerable pressure must be exerted on the rod. This valve must be opened at a certain moment after the opening of the stopcock C. For this purpose the axis of the stopcock is provided with a kind of cam, which just presses on the rod of the valve R when the stopcock C is quite open. The manometer Q can thus be connected with the reservoir B at any time by turning more or less quickly the large stopcock.

We have now to describe the apparatus which measures the time.

A strip of paper (fig. 2) is unrolled from the roller a and rolled on the roller b by the descent of a weight. The motion is rendered nearly uniform by the small vanes d. The strip of paper is guided by two little rollers, e, f. A pencil, g, rests on
the paper and traces a right line when it is motionless. The pencil is carried by an elastic plate, \( h \), to which the armature, \( i \), of an electromagnet is adapted. The moment the circuit is closed the trace of the pencil on the paper changes its position, and it is easy to tell from this the time of the closing of the circuit.

We must first mark on the paper the instant the opening of the stopcock \( C \) commenced. To this end the back part of the stopcock carries a metal rod, \( l \), which moves with it and which communicates with one of the poles of the battery by the conducting-wire \( l m n o p \). When the stopcock begins to open, the rod \( l \) immediately plunges into the mercury \( g \), which communicates with the other pole of the pile by the wire \( q v k s \). The circuit is closed at this moment, and the pencil is displaced.

The moment at which the manometer \( Q \) communicates with the reservoir \( B \) must next be marked on the paper. For this purpose the head of the rod of the valve \( R \) rests on a little lever \( t m \), which supports an iron rod plunged in mercury, \( n \). So long as the valve \( R \) is not opened the circuit is closed; but the moment the stopcock \( C \) opens this valve the iron rod \( m \) is raised and no longer touches the mercury, the current ceases, and the pencil \( g \) returns to its initial position.

Lastly, the positions of the level in the manometer \( Q \) at some stated periods must be noted. For this purpose the observer reads the number on the scale corresponding to the level in the manometer, and at the same moment plunges the extremity \( u \) of the conducting-wire \( v u \) into the mercury \( o \). He thus closes the circuit \( u p s k v u \) for an instant only; and the displacement of the pencil leaves a trace on the paper. To measure the time which has elapsed between two displacements of the pencil, the distance of the corresponding traces on the paper must be measured and the movement of the paper known.

This movement not being quite uniform, it must be determined by some special experiments. To accomplish this the weights were caused to descend, and the circuit of the electromagnet closed, every five seconds, for example, after the departure of the weights. We thus knew to what periods the successive displacements of the pencil marked on the strip of paper corresponded; and this kind of graduation served afterwards to measure the times in each experiment with sufficient accuracy.

In several series the metal reservoir \( B \) was replaced by a glass vessel of twice the capacity (60-617 litres); the circulation of the water was then impossible, and the two reservoirs were either enveloped in a layer of thick sawdust or left uncovered. In these cases irregularities occurred, resulting from the inequality of temperature of the two reservoirs. But such irregularities are of little importance in the series in question.
§ III. Course of an experiment.

The stopcock C (fig. 2) is open, and the pressure of the gas contained in the reservoirs differs but little from the atmospheric pressure; for an instant the valve R is opened, and the levels in the manometer Q are allowed to settle themselves. The stopcock C and the valve are closed. By means of the pump some gas is caused to pass from the reservoir B to A; and when the temperature is stationary, the pressure $p_1$ is measured in the manometer K, and the pressure $p_2$ in the manometer M. The stopcocks $r, r''$ are closed, so that the gas contained in the manometers may be separated from that which the reservoirs contain, and not disturb the expansion; without this precaution the effects observed during the expansion would depend on the movement of the gas in the tubes L N O, I P O, and it would be impossible to obtain an accurate result.

After these preparations, the strip of paper is put in motion; the stopcock C is opened by an assistant, who writes down the various positions of the exterior level of the liquid contained in the manometer Q as they are read aloud on the scale of this manometer, closing at the same moment the circuit of the electromagnet. The highest and lowest points which the level has reached are particularly noted.

In general this level does not return quite to its initial position, because there remains some gas in the pump and the manometers; but the difference is not very great, and the manometer Q is sufficiently true, as has been already stated. When several consecutive experiments are made, the final level is always at the same point as in the first experiment, provided there has been neither leakage in the apparatus nor change in the exterior temperature and pressure. The essential condition of a successful experiment consists in the initial level being near the final level.

In order to complete the observations, the external temperature and the atmospheric pressure are measured. From the position of the final level, we know the pressure $p'$, which establishes itself in the reservoirs when every motion has ceased and the reservoirs have returned to the ordinary temperature. Thus we know that the gas has undergone an expansion under an excess of pressure $p_1 - p_2$ without external work, that it has finally attained the pressure $p'$, and that before attaining this pressure it has had a variable pressure $p$, which must be deduced from the observed variations of the level in the manometer Q.

It is in order to find this variable pressure that the curve defined in § I. was constructed, of which the general form is represented in fig. 1. The abscissae are calculated by the traces of the pencil on the strip of paper, and the ordinates by means
of the differences of level in the manometer Q corresponding to those traces. Let $H'$ be the difference of the final level which corresponds to the pressure $p'$, and $H$ the difference observed at a time $t$; the excess, positive or negative, $H - H' = \delta$ is the ordinate corresponding to the abscissa $t$.

I shall show how a succession of values of $p$ can be deduced from the comparison of the various curves which we obtain by only changing the time comprised between the opening of the stopcock $C$ and that of the valve $R$, which time shall be denoted by $\theta$.

§ IV. Discussion of a series of experiments. How the pressure varies during the expansion.

Experiments of the same series are those in which nearly the same values for $p_1$ and $p_2$ were preserved, and in which the value of the interval of time $\theta$, comprised between the opening of the large stopcock $C$ and that of the valve $R$, was alone changed.

In reality the curve constructed for each experiment varies with $p_1$ and $p_2$ for the same value of $\theta$. But as the ordinates only represent the excess $\delta$ of the difference of the variable level $H$ over the final difference $H'$, small changes in the values of $p_1$, $p_2$ produce no appreciable effect; which dispenses with a reduction to constant values of $p_1$, $p_2$.

The following is a series made with dry air. The liquid in the manometer Q was sulphuric acid. The values of $\delta$ are measured by the heights of the liquid, calculated in millimetres.

**Series I. (November 1867).**

**Dry air. Metal Reservoir B.**

$p_1 = 3.84$ atmospheres, $p_2 = 0.22$ atmosphere. Temperature between 7° and 11°.

<table>
<thead>
<tr>
<th>Curves.</th>
<th>$\theta = 0$</th>
<th>$\theta = 5$</th>
<th>$\theta = 10$</th>
<th>$\theta = 15$</th>
<th>$\theta = 20$</th>
<th>$\theta = 25$</th>
<th>$\theta = 30$</th>
<th>$\theta = 35$</th>
<th>$\theta = 40$</th>
<th>$\theta = 45$</th>
<th>$\theta = 50$</th>
<th>$\theta = 55$</th>
<th>$\theta = 60$</th>
<th>$\theta = 65$</th>
<th>$\theta = 70$</th>
<th>$\theta = 75$</th>
</tr>
</thead>
<tbody>
<tr>
<td>A.</td>
<td>$\delta = +13$</td>
<td>$\delta = +20$</td>
<td>$\delta = +27$</td>
<td>$\delta = +32$</td>
<td>$\delta = +29$</td>
<td>$\delta = +26$</td>
<td>$\delta = +23$</td>
<td>$\delta = +20$</td>
<td>$\delta = +17$</td>
<td>$\delta = +14$</td>
<td>$\delta = +11$</td>
<td>$\delta = +8$</td>
<td>$\delta = +5$</td>
<td>$\delta = +2$</td>
<td>$\delta = -1$</td>
<td>$\delta = -4$</td>
</tr>
<tr>
<td>B.</td>
<td>$\theta = 0$</td>
<td>$\theta = 7$</td>
<td>$\theta = 14$</td>
<td>$\theta = 21$</td>
<td>$\theta = 28$</td>
<td>$\theta = 35$</td>
<td>$\theta = 42$</td>
<td>$\theta = 49$</td>
<td>$\theta = 56$</td>
<td>$\theta = 63$</td>
<td>$\theta = 70$</td>
<td>$\theta = 77$</td>
<td>$\theta = 84$</td>
<td>$\theta = 91$</td>
<td>$\theta = 98$</td>
<td>$\theta = 105$</td>
</tr>
<tr>
<td>C.</td>
<td>$\theta = 1$</td>
<td>$\theta = 2$</td>
<td>$\theta = 3$</td>
<td>$\theta = 4$</td>
<td>$\theta = 5$</td>
<td>$\theta = 6$</td>
<td>$\theta = 7$</td>
<td>$\theta = 8$</td>
<td>$\theta = 9$</td>
<td>$\theta = 10$</td>
<td>$\theta = 11$</td>
<td>$\theta = 12$</td>
<td>$\theta = 13$</td>
<td>$\theta = 14$</td>
<td>$\theta = 15$</td>
<td>$\theta = 16$</td>
</tr>
<tr>
<td>D.</td>
<td>$\theta = 3$</td>
<td>$\theta = 6$</td>
<td>$\theta = 9$</td>
<td>$\theta = 12$</td>
<td>$\theta = 15$</td>
<td>$\theta = 18$</td>
<td>$\theta = 21$</td>
<td>$\theta = 24$</td>
<td>$\theta = 27$</td>
<td>$\theta = 30$</td>
<td>$\theta = 33$</td>
<td>$\theta = 36$</td>
<td>$\theta = 39$</td>
<td>$\theta = 42$</td>
<td>$\theta = 45$</td>
<td>$\theta = 48$</td>
</tr>
<tr>
<td>E.</td>
<td>$\theta = 6$</td>
<td>$\theta = 12$</td>
<td>$\theta = 18$</td>
<td>$\theta = 24$</td>
<td>$\theta = 30$</td>
<td>$\theta = 36$</td>
<td>$\theta = 42$</td>
<td>$\theta = 48$</td>
<td>$\theta = 54$</td>
<td>$\theta = 60$</td>
<td>$\theta = 66$</td>
<td>$\theta = 72$</td>
<td>$\theta = 78$</td>
<td>$\theta = 84$</td>
<td>$\theta = 90$</td>
<td>$\theta = 96$</td>
</tr>
<tr>
<td>F.</td>
<td>$\theta = 12$</td>
<td>$\theta = 24$</td>
<td>$\theta = 36$</td>
<td>$\theta = 48$</td>
<td>$\theta = 60$</td>
<td>$\theta = 72$</td>
<td>$\theta = 84$</td>
<td>$\theta = 96$</td>
<td>$\theta = 108$</td>
<td>$\theta = 120$</td>
<td>$\theta = 132$</td>
<td>$\theta = 144$</td>
<td>$\theta = 156$</td>
<td>$\theta = 168$</td>
<td>$\theta = 180$</td>
<td>$\theta = 192$</td>
</tr>
<tr>
<td>G.</td>
<td>$\theta = 24$</td>
<td>$\theta = 48$</td>
<td>$\theta = 72$</td>
<td>$\theta = 96$</td>
<td>$\theta = 120$</td>
<td>$\theta = 144$</td>
<td>$\theta = 168$</td>
<td>$\theta = 192$</td>
<td>$\theta = 216$</td>
<td>$\theta = 240$</td>
<td>$\theta = 264$</td>
<td>$\theta = 288$</td>
<td>$\theta = 312$</td>
<td>$\theta = 336$</td>
<td>$\theta = 360$</td>
<td>$\theta = 384$</td>
</tr>
<tr>
<td>H.</td>
<td>$\theta = 48$</td>
<td>$\theta = 96$</td>
<td>$\theta = 144$</td>
<td>$\theta = 192$</td>
<td>$\theta = 240$</td>
<td>$\theta = 288$</td>
<td>$\theta = 336$</td>
<td>$\theta = 384$</td>
<td>$\theta = 432$</td>
<td>$\theta = 480$</td>
<td>$\theta = 528$</td>
<td>$\theta = 576$</td>
<td>$\theta = 624$</td>
<td>$\theta = 672$</td>
<td>$\theta = 720$</td>
<td>$\theta = 768$</td>
</tr>
<tr>
<td>I.</td>
<td>$\theta = 96$</td>
<td>$\theta = 192$</td>
<td>$\theta = 288$</td>
<td>$\theta = 384$</td>
<td>$\theta = 480$</td>
<td>$\theta = 576$</td>
<td>$\theta = 672$</td>
<td>$\theta = 768$</td>
<td>$\theta = 864$</td>
<td>$\theta = 960$</td>
<td>$\theta = 1056$</td>
<td>$\theta = 1152$</td>
<td>$\theta = 1248$</td>
<td>$\theta = 1344$</td>
<td>$\theta = 1440$</td>
<td>$\theta = 1536$</td>
</tr>
</tbody>
</table>
In fig. 4 we have only shown six curves, to avoid confusion. In figs. 4, 6, 7, and 8 a division represents a second in the direction of the abscissæ, and 1 centim. in the direction of the ordinates.

It is plainly seen from the figure how the curves run as the time $\theta$ increases. For a small value of $\theta$ (curve A) the line falls rapidly for three seconds until $h = -136$ millims.; then it rises more slowly, passes the abscissæ-axis until $h = +16$ millims., and again descends very slowly until $h = 0$.

For a greater value of $\theta$ (curve B) the line falls for two seconds, until $h = -41$ millims.; then it rises at first parallel to the preceding, attains $h = +30$ millims., and further on coincides with the curve A.

The separation of these curves evidently depends on the liquid in the manometer not instantly indicating the pressure of the gas, and on the level being more or less behind according as it must run a greater or less length before reaching the position of equilibrium.

Let us suppose the variation of the real pressure of the gas to be represented by the line $g h i$ (fig. 5). When the valve of the manometer is opened the liquid would indicate $h = -f g$ if it could move instantly, and if the pressure of the gas remained invariable. But whilst the liquid moves, the pressure of the gas increases; and when the level indicates $h = -k b$, the pressure corresponds to the point $l$. The point $b$ is evidently higher than the point $g$, and it is also lower than the point $l$ on account of the impulse the liquid receives. The level afterwards rises until the manometric column and the pressure of the gas are in equilibrium, which can only take place after a certain time.

Let the valve be opened after a time $o f' > o f$. Instead of instantly indicating $h = -f' g'$, it only indicates $h = -k' b'$; and the distance of the point $b'$ from $g'$ and $l'$ is less than before, because the impulse and the initial rarefaction $f' g'$ have been less; moreover the duration $f' k'$ of the depression is also less than $f k$, because $k' b' < k b$. Hence the curve $b' c'$ approaches closer to $g h i$ than the curve $b c$ and will join it sooner.

In the following paragraph several observations will confirm this interpretation. In fig. 4 the line X X is traced, with which the experiments of Series I. agree as well as possible. It represents approximately the law of the variable pressure of the gas during the expansion from 0·5 second. Several other experiments, in which the value of $\theta$ was less, have shown that the curves differ very little from the curve A until $\theta = 0·1$ second. I have not made observations of shorter duration.

If follows from this, that if $o p_1$ (fig. 5) denote the positive excess of the pressure of the compressed gas in reservoir A above
the final pressure $p'$, and $o p_2$ the negative excess of the pressure of the rarefied gas in reservoir B above the same final pressure, the succession of the pressures in each of the reservoirs is represented by curves of the form $p, g h i e, p_2 g h i e$.

The abscissa of $f$ is less than 0·1 second. During this very short period the compressed gas rushes into the reservoir B, compresses the gas in the latter, and establishes equality in the whole of the apparatus. From this moment the pressure common to the two reservoirs increases at first rather rapidly, reaches a maximum in about fourteen seconds, and afterwards decreases very slowly and becomes stationary after two minutes. The final pressure is greater by 14 centimetres at least (sulphuric acid) than the pressure which existed at the moment when the expansion ceased, and less by 3 centims. than the maximum of the variable pressure.

It is during the period $g h i$ that the mechanical and thermal phenomena which interest us manifest themselves. They are due, at least in part, to the properties of the gas and the action of the sides, which can be disregarded at no part of the whole duration of the phenomenon. But, before seeking to explain the latter, we must increase the number of experiments, and examine attentively the various circumstances which appear likely to exercise an influence either on the magnitude or on the direction of the effects.

§ V. Influence of the nature of the liquid contained in the manometer.

Most of the experiments relative to the accessory circumstances have been made since the publication of the earliest researches of this series. They have gradually led me to the discovery of facts described in the preceding paragraph. If in my exposition of these I have not observed their chronological order, it was that I might first establish the experimental method as a whole. The details which follow will, I hope, prove that I have carefully avoided allowing myself to be directed in these researches by any preconceived ideas; and I shall only draw conclusions after having solved several questions which have successively presented themselves to my mind.

In order to examine whether the nature of the liquid had any influence, two series of experiments were made under the same conditions, with the same value of $\theta_2$—the manometer $Q$, for example, first containing sulphuric acid and then water. In the latter case the interior level was covered with a thin layer of turpentine to avoid the influence of the vapour of the water. For the reservoir B, in these two series, the glass vessel was used.
Dry hydrogen. Glass reservoir B. Manometer of sulphuric acid communicating with the tube H' (fig. 2).

\[ p_1 = 3.89 \text{ atmospheres, } p_2 = 0.55 \text{ atmosphere. Temperature between } 24^\circ 6 \text{ and } 25^\circ 1. \]

<table>
<thead>
<tr>
<th>( \theta = 0.10 )</th>
<th>( t = 1.9 )</th>
<th>( 10 )</th>
<th>( 19 )</th>
<th>( 25 )</th>
<th>( 60 )</th>
</tr>
</thead>
<tbody>
<tr>
<td>( h = -1 )</td>
<td>( -61 )</td>
<td>( -11 )</td>
<td>( +4 )</td>
<td>( +5 )</td>
<td>( +2 )</td>
</tr>
<tr>
<td>( \theta = 0.13 )</td>
<td>( 2.4 )</td>
<td>( 5.4 )</td>
<td>( 8.9 )</td>
<td>( 13.5 )</td>
<td>( 19 )</td>
</tr>
<tr>
<td>( h = 0 )</td>
<td>( -67 )</td>
<td>( -25 )</td>
<td>( -4 )</td>
<td>( +4 )</td>
<td>( +3 )</td>
</tr>
<tr>
<td>( \theta = 0.15 )</td>
<td>( 2 )</td>
<td>( 4.8 )</td>
<td>( 7.3 )</td>
<td>( 11 )</td>
<td>( 14 )</td>
</tr>
<tr>
<td>( h = -14 )</td>
<td>( -67 )</td>
<td>( -25 )</td>
<td>( -9 )</td>
<td>( +1 )</td>
<td>( +3 )</td>
</tr>
<tr>
<td>( \theta = 0.18 )</td>
<td>( 2.1 )</td>
<td>( 5 )</td>
<td>( 8 )</td>
<td>( 11.9 )</td>
<td>( 17 )</td>
</tr>
<tr>
<td>( h = -1 )</td>
<td>( -61 )</td>
<td>( -22 )</td>
<td>( -1 )</td>
<td>( +6 )</td>
<td>( +4 )</td>
</tr>
<tr>
<td>( \theta = 0.19 )</td>
<td>( 2 )</td>
<td>( 5.3 )</td>
<td>( 8.6 )</td>
<td>( 11.9 )</td>
<td>( 19 )</td>
</tr>
<tr>
<td>( h = +9 )</td>
<td>( -59 )</td>
<td>( -15 )</td>
<td>( 0 )</td>
<td>( +4 )</td>
<td>( +2 )</td>
</tr>
<tr>
<td>( \theta = 0.2 )</td>
<td>( 2.1 )</td>
<td>( 5.6 )</td>
<td>( 8.4 )</td>
<td>( 13.3 )</td>
<td>( 17.9 )</td>
</tr>
<tr>
<td>( h = +1 )</td>
<td>( -64 )</td>
<td>( -16 )</td>
<td>( -1 )</td>
<td>( +5 )</td>
<td>( +4 )</td>
</tr>
<tr>
<td>( \theta = 0.21 )</td>
<td>( 2.1 )</td>
<td>( 4.8 )</td>
<td>( 6.8 )</td>
<td>( 9.3 )</td>
<td>( 12.6 )</td>
</tr>
<tr>
<td>( h = -3 )</td>
<td>( -63 )</td>
<td>( -24 )</td>
<td>( -8 )</td>
<td>( +2 )</td>
<td>( +5 )</td>
</tr>
<tr>
<td>( \theta = 0.52 )</td>
<td>( 2.2 )</td>
<td>( 5.4 )</td>
<td>( 9.9 )</td>
<td>( 13.2 )</td>
<td>( 17.3 )</td>
</tr>
<tr>
<td>( h = 0 )</td>
<td>( -64 )</td>
<td>( -17 )</td>
<td>( +4 )</td>
<td>( +5 )</td>
<td>( +3 )</td>
</tr>
<tr>
<td>( \theta = 0.59 )</td>
<td>( 2.1 )</td>
<td>( 5.2 )</td>
<td>( 9 )</td>
<td>( 12 )</td>
<td>( 17 )</td>
</tr>
<tr>
<td>( h = 0 )</td>
<td>( -63 )</td>
<td>( -17 )</td>
<td>( +4 )</td>
<td>( +6 )</td>
<td>( +4 )</td>
</tr>
<tr>
<td>( \theta = 0.74 )</td>
<td>( 2.2 )</td>
<td>( 5.3 )</td>
<td>( 8.3 )</td>
<td>( 10.5 )</td>
<td>( 20 )</td>
</tr>
<tr>
<td>( h = -16 )</td>
<td>( -61 )</td>
<td>( -13 )</td>
<td>( +2 )</td>
<td>( +7 )</td>
<td>( +2 )</td>
</tr>
<tr>
<td>( \theta = 0.96 )</td>
<td>( 2.6 )</td>
<td>( 9 )</td>
<td>( 12.9 )</td>
<td>( 19.2 )</td>
<td>( \ldots )</td>
</tr>
<tr>
<td>( h = -1 )</td>
<td>( -56 )</td>
<td>( +3 )</td>
<td>( +6 )</td>
<td>( +3 )</td>
<td>( \ldots )</td>
</tr>
<tr>
<td>( \theta = 1.2 )</td>
<td>( 2.6 )</td>
<td>( 7.4 )</td>
<td>( 11 )</td>
<td>( 18.6 )</td>
<td>( \ldots )</td>
</tr>
<tr>
<td>( h = -1 )</td>
<td>( -41 )</td>
<td>( 0 )</td>
<td>( +6 )</td>
<td>( +4 )</td>
<td>( \ldots )</td>
</tr>
<tr>
<td>( \theta = 2.5 )</td>
<td>( 3.6 )</td>
<td>( \ldots )</td>
<td>( 12.1 )</td>
<td>( \ldots )</td>
<td>( \ldots )</td>
</tr>
<tr>
<td>( h = -4 )</td>
<td>( -18 )</td>
<td>( \ldots )</td>
<td>( +6 )</td>
<td>( \ldots )</td>
<td>( \ldots )</td>
</tr>
<tr>
<td>( \theta = 2.7 )</td>
<td>( 3.9 )</td>
<td>( \ldots )</td>
<td>( 12.1 )</td>
<td>( 16.4 )</td>
<td>( \ldots )</td>
</tr>
<tr>
<td>( h = -4 )</td>
<td>( -23 )</td>
<td>( +4 )</td>
<td>( +6 )</td>
<td>( +4 )</td>
<td>( \ldots )</td>
</tr>
</tbody>
</table>

We must only compare the curves which correspond to the same values of \( \theta \). In Series II. the curve falls about 60 millims. in two seconds; it afterwards rises rapidly, reaches the abscisse-axis six seconds later; it then rises 4 millims. above this axis.
before definitively joining it. In Series III, the minimum of \( h = \) about \(-107\) millims, for an abscissa of \(2.2\) seconds; then the curve rises rapidly until the abscissa is \(7\) seconds, and slowly approaches the abscissa-axis, but still remains below it. This latter fact is owing to the inequality of the temperatures of the two reservoirs; but it will suffice, in order to solve the question proposed, to observe the agreement of the individual points and the ratio of the ordinates which have the same abscissæ. This ratio is exactly the inverse ratio of the densities of the liquids used. Thus for the minimum the densities give \(112\) millims. of water as equivalent to \(60\) millims. of sulphuric acid. Now the Table indicates a value of \(107\); and even in two experiments which I have not given in this Table, since they indicate another kind of influence which will be inquired into presently, I have observed \(112\) millims. and \(113\) millims. with values of \(\theta\) equal to \(0.09\) second and \(0.08\) second. Hence the nature of the liquid has no influence on the phenomenon as a whole, if we regard only the magnitude of the pressures; it manifests itself only by a slight change of the abscissæ. Thus the minimum is reached a little later with water than with sulphuric acid; this is due to the fact that water, although more mobile than the acid, undergoes a greater displacement at the instant the valve is opened.

§ VI. Influence of the form and dimensions of the manometer.

In Series IV. (air) the manometer was made of a tube \(1\) centim. in diameter, and the enlargement was a globe of \(6\) centims. diameter. In Series V. (air) the tube had a diameter of \(0.5\) centim., and the enlargement was a cylinder of \(1.5\) centim. diameter. The manometer contained water, and communicated with the tube H (fig. 2). The apparatus was not identical in the two series; a metal tube, \(a a'\), was adjusted in Series V. to the lower orifice of the tube \(D\), with an object which will be afterwards discussed. But the influence sought being sufficiently characterized by the results of these two series, I have not thought it necessary to give other proofs.

Series IV. (May 1867).

Dry air. Glass reservoir B. Manometer with spherical enlargement.

\[ p_1 = 3.87 \text{ atmospheres, } p_2 = 0.54 \text{ atmosphere.} \]

Temperature between \(14^\circ\) and \(16^\circ\).

\[
\begin{array}{|c|c|c|c|c|c|}
\hline
\theta & 0.11 & 0.13 & 0.14 & 0 & & \\
\hline
h & +3 \text{ mm} & +5 & +7 & +10 & & \\
\hline
l & -4 & -6 & -9 & -14 & -17 & ... \\
\hline
\end{array}
\]
Dry air. Glass reservoir B. Manometer with cylindrical enlargement.

\[ p_1 = 3.89 \text{ atmospheres}, \quad p_3 = 0.54 \text{ atmosphere}. \]

Temperature between 19° and 20°.

\[
\begin{array}{|c|c|c|c|c|c|c|}
\hline
\theta = 0^\circ 15' & t = \ldots & h = -22 \text{ mm} & 2.9 & 7.7 & 12.9 & 23 & 53 & 90 \\
\theta = 0^\circ 17' & & & 2.7 & 7 & 14.4 & 18.6 & 54 & 90 \\
\theta = 0^\circ 18' & & & 2.9 & 6.3 & 9.8 & 15.1 & 54 & 90 \\
\theta = 0^\circ 20' & & & & & & & & \\
\theta = 0^\circ 23' & & & & & & & & \\
\theta = 0^\circ 26' & & & & & & & & \\
\hline
\end{array}
\]

The coordinates of the minimum of the curves are

\[ h = -65 \text{ millims.}, \quad t = 4.5 \text{ seconds}, \]

and

\[ h = -62 \text{ millims.}, \quad t = 2.9 \text{ seconds}, \]

Hence a result of the increase of the dimensions of the manometer, as might be foreseen, is to lessen the variation of the levels, without changing materially the values of \( h \). We also see that the curves of Series IV. rise more slowly from the minimum than those of Series V.

The differences which the curves present at their point of departure are due to the presence of the tube \( a'd' \) in Series V., as we shall see in the following paragraph. It may be observed that, the effect of this tube being to raise for an instant the curve at its point of departure, it attains its minimum a little later; so that our conclusion cannot be doubted.

§ VII. Influence of the position of the tube which connects the reservoirs with the manometer.

We shall see in what this influence consists in the following series, which may be compared with Series I. These two series only differ in that the manometer \( Q \) is put in communication with the tube \( S \) (fig. 2) in Series I., and with the tube \( H' \) in Series VI. Thus the tube of communication passes into the middle of the side of the reservoir \( B \) in Series I., and very near to the large stopcock, on the side of the reservoir \( A \), in Series VI.
Series VI. (September 1867).

Dry air. Manometer communicating with the tube H' (fig. 2).

\[ p_1 = 3.86 \text{ atmospheres}, \quad p_2 = 0.22 \text{ atmosphere.} \]

\[
\begin{array}{|c|c|c|c|c|c|c|}
\hline
\theta &= &0.10 & t &= &37 & 89 & 13.6 & 20.3 & 24 & 54 & 120 \\
\hline
h &= &-3^\text{mm} & +92 &= &-40 & +10 & +18 & +16 & +14 & +3 & 0 \\
\hline
\theta &= &0.21 & ... &= &3.3 & 8.1 & 13.8 & 23.1 & ... & 53 & 120 \\
\hline
h &= &-3 & +22 &= &-73 & -17 & +8 & +11 & ... & +4 & 0 \\
\hline
\theta &= &0.30 & ... &= &2.9 & 6.6 & 9.7 & 14.2 & 24 & 54 & 120 \\
\hline
h &= &-4 & ... &= &-88 & -25 & +6 & +19 & +15 & +6 & 0 \\
\hline
\theta &= &0.38 & ... &= &2.9 & 6 & 8.2 & 11.3 & 15.3 & 24 & 120 \\
\hline
h &= &0 & ... &= &-99 & -42 & -10 & +10 & +18 & +14 & 0 \\
\hline
\theta &= &0.92 & ... &= &3.2 & 7.1 & 10.5 & 16.1 & 25 & 55 & 120 \\
\hline
h &= &-2 & ... &= &-88 & -21 & +9 & +17 & +12 & +1 & 0 \\
\hline
\end{array}
\]

It will be seen, in tracing the curves of this series, that when the valve of the manometer is opened after a time \( \theta < 0.30 \) second, the exterior level of the manometric liquid rises at first very rapidly higher the lower \( \theta \) is, and afterwards redescends and comports itself as before. The form of the first two curves of Series VI. is that of the line \( a'b''c' \) of fig. 1.

It is easy to explain this peculiarity. Before the expansion commences, the tube D H'R and the reservoir A are filled with compressed gas. When the expansion commences, and before the valve R is opened, the gas in the tube D H'R escapes into the reservoir A; and it may happen that the valve opens before the tube is emptied. Then the manometer indicates a momentary increase of pressure, until equilibrium is again established throughout the tube. An analogous effect may be produced in Series I. on account of rarefaction in the tube S R: the depression of the level will be increased by this effect for small values of \( \theta \). But the difference of pressure between the reservoir B and the tube S R is much less than between the reservoir A and the tube D H'R. Moreover I used for S R a wide and short lead tube. This kind of influence is therefore altogether insignificant in experiments arranged like those of Series I.

It must be observed that if \( \theta > 0.30 \) second, the results are the same in the two series; so that it is a matter of indifference whether the manometer communicates with the tubulures S, H', or even H (as was the case with the glass reservoir B), on condition that we do not take \( \theta \) too small.

Comparisons analogous to those I have just mentioned have been frequently repeated; and they all lead to the conclusion that equality of pressure establishes itself very quickly in the reservoirs, and that it afterwards maintains itself, as is represented by the line \( g'h'e \), fig. 5, whilst the various portions of the gas exchange among themselves motion and heat.
It will be remembered that I have mentioned in § V. two experiments on hydrogen not given in Series III. They show that the influence only manifests itself for $\theta < 0.1$ second.

Continuation of Series III.

<table>
<thead>
<tr>
<th>$\theta = 0^\circ 08$</th>
<th>2-2</th>
<th>6-2</th>
<th>11</th>
<th>16</th>
<th>24</th>
<th>60</th>
</tr>
</thead>
<tbody>
<tr>
<td>$h = 0$</td>
<td>+19</td>
<td>-13</td>
<td>-34</td>
<td>-13</td>
<td>-6</td>
<td>-1</td>
</tr>
<tr>
<td>$\theta = 0.09$</td>
<td>2-3</td>
<td>6-9</td>
<td>10-4</td>
<td>18-4</td>
<td>...</td>
<td>60</td>
</tr>
<tr>
<td>$h = -6$</td>
<td>+13</td>
<td>-12</td>
<td>-24</td>
<td>-14</td>
<td>-3</td>
<td>...</td>
</tr>
</tbody>
</table>

§ VIII. Influence of the arrangement of the channel which joins the two reservoirs.

Wishing to investigate at the outset of these researches the influence of the mode of communication of the manometer with the copper reservoir A and the glass one B, I had fitted interiorly to the tubes D, E (fig. 2) some copper tubes, $a \, a'$, $a' \, a''$, open at $a', \, a''$, so that the interior pressure might be transmitted to the manometer Q, either by the pipe $a' \, a \, D \, H \, R$, or by the similar one $a'' \, a' \, E \, H \, R$. These tubes grazed the sides of the tubulures of the reservoirs, and did not seem likely to obstruct the jet of gas when it rushed from A towards B. I was very much astonished, in making some experiments like the preceding, to see the curve take the form of the line $m \, n \, e \, (\text{fig. 1})$; $h$ attained the maximum value +250 millims. (water), the difference of pressure $p_1 - p_2$ being about four atmospheres. This effect was always produced, whatever the value of $\theta$. It underwent no change when an opening was made at the point $a_1$ of the tube, when the extremity $a'_1$ was closed, and, finally, when both $a_1$ and $a'_1$ were closed. In this latter case the internal pressure was transmitted to the manometer only by the passage $a' \, a \, D \, H \, R$, whilst in the other cases we used either this passage or the other.

The tube $a_1 \, a'_1$ was suppressed, and then the effects of series V. observed, which are described in § VI. and represented by the line $a' \, b' \, b'' \, c$, fig. 1.

It is evident that the anomaly observed was due to the action of the tube $a_1 \, a'_1$ on the gaseous jet, and that the tube $a \, a'$ exercised an analogous action, but not so great. Hence this is the explanation of the anomaly:

Let us distinguish three parts in the gas of the reservoirs:—

(1) that which remains in the reservoir A after the expansion;
(2) that which remains in the reservoir B before the expansion;
(3) that which passes from the reservoir A into the reservoir B.

When the expansion has taken place, there is equality of pressure between these three parts; the magnitude $p$ of this pressure depends on the state in which each is found. It afterwards varies, in proportion as these parts mix together and exchange heat, either with one another or with the sides; and when these exchanges have ended, the pressure attains the final value $p'$. 
At the moment equality of pressure commences, the first part is cold, the second is warm, and the third is in a condition which depends on the friction which it has produced during the expansion. Let us suppose that this friction is greater in one experiment than in another; then there is a greater heating in the third part of the gas; consequently the pressure $p$ is greater. If we notice a value $p < p'$ with slight friction, we may observe, with friction sufficiently great, $p > p'$ ($p'$ having, moreover, the same value in both cases).

This is what occurred in my experiments: the portion of gas which passed from reservoir $A$ glided along the tubes $a' a, a_1 a', a_2 a'$, and produced great friction, especially along the part $a_1 a'$, because the molecules in this place had the greatest velocity. As to the first two parts of the gas, their velocity was inconsiderable, and they behaved almost as if the tubes $a a', a_1 a'$ had not existed.

By suppressing the tube $a, a'$, the friction was considerably diminished, and the increase of pressure was much less than before.

These observations clearly demonstrate that the sides of the canal $X Y$ which join the two reservoirs exert an influence on the curve of excesses of pressure $h$; this influence consists in raising the minimum $b$ of this curve (fig. 1). It ought to be appreciable with the glass vessel (the neck of which was 12 centims. long), but is lessened as much as possible with the zinc reservoir which was used in the actual experiments. The neck of this reservoir was, in fact, very wide and very short (diameter 5 centims., length 4 centims.).

This influence was considerable in an experiment I made with a stone vessel: the neck consisted of a wide glass tube; it formed an ajutage 15 centims. long and 4 centims. thick, and there was a little cement on the edges of the tube. The curves obtained by means of this reservoir were generally of the form $a' b' c' e'$ (fig. 1). I was compelled to give up the use of it.

I ought to remark that it must not be concluded, from the curve $m n p$, that friction occasions the creation of a quantity of heat different from that which results from the destruction of the velocities of the gaseous molecules under ordinary circumstances. The final thermal effect is always the same in every case—such, for example, as would be observed in a body of water surrounding the reservoirs. That which changes with the intensity of the friction is the law of the pressures during the expansion, because the succession of thermal and partial mechanical effects itself changes. The conversion of work into heat is effected the more quickly the more intense the friction.

[To be continued.]

In a paper published in the Philosophical Magazine for October 1869, I showed that carbon, like hydrogen and nitrogen, is capable of giving two or more distinct spectra, and I endeavoured to explain these differences as dependent solely on differences in the temperature to which the carbon-vapour was heated. Leaving the spectrum of the Bessemer flame out of consideration, the following estimations of temperature were given for the carbon spectra I., II., and IV.:

<table>
<thead>
<tr>
<th>Spectrum</th>
<th>Temperature Range</th>
</tr>
</thead>
<tbody>
<tr>
<td>C II.</td>
<td>Below 1500° C.</td>
</tr>
<tr>
<td>C I.</td>
<td>1500° C. to 10,000° C.</td>
</tr>
<tr>
<td>C IV.</td>
<td>Above 10,000° C.</td>
</tr>
</tbody>
</table>

The spectrum No. II. is that obtained by the electric discharge in carbonic oxide or olefiant gas at a few millimetres pressure. The only evidence for the temperature assigned to it was the fact that the electric spark in either carbonic oxide or olefiant gas at the ordinary pressure gives the spectrum No. I., that on increasing the density of the gas the temperature rises, as is shown by the addition of new lines (groups ξ and θ), but on diminishing the pressure the spectrum No. I. gives place to spectrum No. II. The lowest temperature at which the spectrum No. I. is produced having been shown to be about 1500° C., it was concluded that spectrum No. II. belonged to temperatures below 1500° C.

It appeared probable that more decisive evidence as to the temperature of the discharge in a Geissler’s tube might be obtained by means of certain sodium- and lithium-lines, which become visible only at a high temperature. If a bead of sodium chlorate be brought into a Bunsen-flame, the spectroscope shows during the final deflagration of the salt, besides the D lines, four other groups of lines, whose wave-lengths are given in Ångström’s map as follows:

<table>
<thead>
<tr>
<th>Sodium Line</th>
<th>Wavelengths (in Angstrom)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Naβ</td>
<td>${5681, 5687}$ ten-millionths of a millimetre.</td>
</tr>
<tr>
<td>Naγ</td>
<td>${5152\frac{1}{2}, 5150}$</td>
</tr>
<tr>
<td>Naδ</td>
<td>${4982, 4983\frac{1}{2}}$</td>
</tr>
<tr>
<td>Naε</td>
<td>${6160, 6154}$</td>
</tr>
</tbody>
</table>

* Communicated by the Author.
The readings of these lines on the scale of my spectroscope are

\[ \text{Na} \beta, 56; \quad \text{Na} \gamma, 75.5; \quad \text{Na} \delta, 83.2; \quad \text{Na} \epsilon, 43; \]

the Fraunhofer lines C, D, and F reading 34.5, 50, and 90 respectively.

The same lines were observed by Huggins* in the spark taken between sodium poles, and were shown to be due to sodium itself, since they were obtained by the use of pure sodium-amalgam. Diacon† obtained them from the flame of hydrogen which had been passed through an iron tube in which sodium was volatilized.

The sodium-spectrum obtained by means of a Bunsen burner gives only the D lines; but if the temperature of the flame be increased, these other lines become visible and in the order given above.

I find that \( \text{Na} \beta \) becomes visible almost precisely at the temperature at which platinum melts. This, according to the experiments of Deville, is \( 2000^\circ \) C.

The flame of hydrogen in air (of which the temperature, according to the experiments of Bunsen, is \( 2024^\circ \) C,) gives the D lines only. The flame is incapable of fusing platinum except at one point.

The Bunsen flame of coal-gas and air gives only D. The flame is incapable of melting platinum.

The flame of coal-gas fed by a jet of air gives a spectrum in which, besides D, the line \( \text{Na} \beta \) is faintly visible. This flame is just capable of fusing a fine platinum wire.

The flame of coal-gas fed by a mixture of oxygen and air (containing about 30 per cent. oxygen and 70 per cent. nitrogen) gives \( \text{Na} \beta \) distinctly and \( \text{Na} \delta \) faintly. \( \text{Na} \gamma \) and \( \epsilon \) are not seen. The flame fused platinum tolerably easily.

The flame of coal-gas fed by pure oxygen gives a spectrum containing \( \text{Na} \beta, \gamma, \delta, \) and \( \epsilon \), but \( \epsilon \) is only faint. The flame fuses platinum easily.

The flame of carbonic oxide in air (temperature \( 1997^\circ \), Bunsen) gives only the D line. It is incapable of melting platinum.

Carbonic oxide fed by a jet of air still gives only D. The flame just melts platinum.

Carbonic oxide fed by oxygen (temperature of flame \( 3033^\circ \) C., Bunsen) gives \( \text{Na} \beta \) and \( \gamma \) brilliantly. \( \text{Na} \delta \) and \( \epsilon \) were not seen. The flame melts platinum easily.

The flame of sulphur in air (calculated temperature \( 1900^\circ \) C.) gives the D lines only. It melts gold, but not platinum.

The flame of sulphuretted hydrogen in air (calculated tempe-

rature 2250° C.) gives the D lines only. It melts gold but not platinum.

We may therefore employ the line Na β as a test of temperature indicating a temperature at least 2000° C.

Certain lines of the lithium-spectrum may be employed for the same purpose. In the Bunsen burner, a bead of lithium-chloride gives a spectrum of one red line whose wave-length is about 6684. The flame of coal-gas fed by a jet of air shows, besides the red line, an orange line whose wave-length is about 6107. In the flame of coal-gas and oxygen a blue line (4605) is added; and in the electric arc a fourth line (4921) becomes visible. All these lines can be obtained by means of lithium-chlorate in the Bunsen-flame.

A vacuum-tube containing coal-gas gives the same spectrum as if carbonic oxide or olefiant gas were employed, viz. the spectrum C II. This experiment was repeated with a coal-gas tube containing pieces of metallic sodium. At first the spectrum was that of carbon, as previously described; but as the tube became heated by the continued discharge, the line Na β came out followed by the lines γ, δ, and e, and the carbon-lines faded away till ultimately the sodium-spectrum of five lines alone remained. During this change the carbon-lines and the sodium-lines were seen together; and as the temperature to which the sodium-vapour was heated cannot be supposed to be different from that to which the carbon-vapour was heated, it follows that the spectrum C II. may be produced by carbon heated above 2000° C.

It is to be observed also that this spectrum may be produced by carbon heated not much above 2000° C., since it was obtained together with C β and without C γ, which comes out under 3000° C.

Carbon-spectrum No. I. is given by the blue cone of the Bunsen flame, the temperature of which cannot be much above 1500° C., and is certainly less than 2000° C.; and the same spectrum (with the addition of two new groups of lines, ξ and θ) is obtained at all temperatures up to that of the cyanogen-flame in oxygen, or probably 10,000° C. We have thus two quite different spectra, each of which has been shown to be due to carbon itself, and not to any compound of carbon, which are proved to be obtainable at the same temperature. In the case of the six different spectra of hydrogen described by Wüllner*, which are all obtained by the electric discharge in gas at different pressures, we may suppose the differences to be due to difference of temperature; but in the case of the carbon-spectra we are forced to some other explanation. It is worthy of remark that, while the spectrum C II. is obtained only by the use of electricity, the

spectrum C I. can be obtained both from flame and by the use of the electric spark.

The sodium-lines $\beta$, $\gamma$, $\delta$, and $\varepsilon$ are seen in the spectrum of the electric light, of the spark of an induction-coil between sodium poles in air, both with and without the Leyden jar, and are obtained simultaneously with the hydrogen-lines $\alpha$, $\beta$, and $\gamma$ in a hydrogen vacuum-tube. The simplest mode of obtaining them is to heat the narrow part of the vacuum-tube with a Bunsen flame; the discharge inside the hot part of the tube becomes yellow and exhibits the sodium-lines brilliantly.

I have confirmed the results obtained by Wüllner in his experiments on hydrogen under pressure, and have pushed the pressure to nine atmospheres. The spectrum of the spark of an induction-coil (without condenser) in the gas at nine atmospheres' pressure is still far from being continuous. H $\alpha$ is still a very bright and distinct line; H $\beta$ and $\gamma$ are merely maxima of light.

---


To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

If I rightly understand the paper of the Rev. J. M. Heath, published in the Philosophical Magazine for July 1870, p. 51, he lays down a principle which may be virtually expressed by saying that the work done by a force in overcoming attractions or repulsions cannot take effect in producing heat,—that is, in accelerating molecular motions. That principle is perfectly correct, and is an obvious consequence of the laws of motion; and every one who knows those laws must agree with Mr. Heath when he states it. But from the remarks with which his statement is accompanied, he seems not to be aware that this very principle has been most carefully kept in view by every author of original researches in thermodynamics, and by every writer on the subject who has understood those researches. In fact the problem which is solved by the general equation of thermodynamics may be stated as follows:—A certain quantity of work being done by the action of external forces on a body in a certain way, to distinguish that quantity of work into two parts, one of which is expended in overcoming molecular attractions and repulsions, and the other in accelerating molecular motions.

A system of particles contained within a vessel and in a state of rest, being kept in equilibrio by their mutual attractions and repulsions, exerts a pressure or a tension against the internal surface of that vessel according as repulsions or attractions predo-
minate; and work done in altering the capacity or the figure of the vessel does not produce heat, but only stored up energy, like that possessed by a bent spring.

A system of particles confined within a vessel, and not sensibly attracting or repelling each other, but in a state of motion, exerts outward pressure against the internal surface of that vessel through the reactions of the particles that tend to escape, but are prevented by the vessel from doing so; and work done in diminishing the capacity of this vessel wholly takes effect in accelerating the motions of the confined particles,—that is, in the language of thermodynamics, producing heat.

The condition of actual bodies is compounded of those two; and it is by means of an equation deduced from what has been called the "Second Law" of thermodynamics, that the force exerted by a substance against the internal surface of a vessel containing it (in other words, the elastic force of the substance) is distinguished into two components, due respectively to molecular attractions and repulsions, and to the reactions of moving particles (of the nature of centrifugal force). It is the latter component of the force only that is taken into account in calculating how much heat is produced by a given alteration of the dimensions or figure of the containing vessel.

It has been proved by experiment that very nearly the whole of the work done in compressing a gas takes effect in producing heat; and hence it has been concluded that the elasticity of gases is almost wholly due to the motion of their particles, the component due to attractions and repulsions being small in comparison.

The detailed exposition of the principles to which I have briefly referred, and the comparison of their results with those of experiment, have been made so often, by so many authors, and in so many ways, that it would be a waste of time and space for me to explain them further here; and I shall therefore, in conclusion, merely refer to Professor Tait's work on Thermodynamics as the best source of information regarding the history and present condition of that science; for he gives a summary, in very moderate compass, of the different methods of demonstration followed by the various original authors. In most of the popular writings on the subject, the second law of thermodynamics, together with its proofs and consequences, is omitted, as requiring too much mental exertion for its comprehension.

I am, Gentlemen,

Your most obedient Servant,

W. J. MACQUORN RANKINE.

Glasgow, July 5, 1870.
XIV. On the Refractive Indices and the Dispersion of Opaque Bodies. By W. Wernicke*.

VARIOUS indirect methods have been employed for the determination of the refractive indices of bodies which appear opaque when they are of such thickness as is requisite for direct determination by means of prismatic deflection. The one mostly used is the determination of the angle of the greatest polarization (or of the nearly identical principal angle of incidence†) of the substance, the tangent of which is assumed to be equal to the refractive index. But, apart from the contradictions to which we are led by a comparison of the refractive indices of metals determined by this method with those obtained by another method of determination, the observation of the angle of polarization is very uncertain: even with transparent substances of great refractive power, results are at times obtained which differ even in the first decimal ‡ from those correctly determined by means of prismatic deflection. The same uncertainty occurs in a still higher degree with Wollaston's method for the determination of refraction by means of the total reflection: most opaque bodies of great refractive power, when placed in contact with an hypotenuse-surface of the prism, afford no definite limiting angle of total reflection. Still less applicable is Arago's method, who, by means of Poisson's formulae, determined the refractive index of mercury to be 5·829, from the ratio of the quantity of perpendicularly incident light reflected to that which is transmitted. If this method is applied to silver, which reflects 95 per cent. of light, the refractive index is found to be 71·8, whilst the method by the angle of polarization only gives 4·8. A fourth method, more recently applied by Quincke§, according to which the velocity of light in the metal is determined by means of the displacement of fringes which is produced by two interfering pencils of light, one direct and the other passing through a thin lamina of the metal, has given the refractive indices of a few metals less than 1, which is in agreement with Cauchy's formula, but in contradiction to the results of the method by the angle of polarization. All these methods can at most give, in the most favourable case, an approximate idea of the magnitude of the mean refraction; not one is suitable even for an approximate determination of the dispersion.

In the present research I describe a method which admits the determination of the refractive indices and of the dispersion of a

* Translated from Poggendorff's Annalen, 1870, No. 1.
† Haughton, Phil. Trans. vol. clii. pp. 81–125.
great number of that group of bodies which stand next to the metals in degree of opacity, as the protoxides, oxides, peroxides and chlorides, bromides, iodides, and sulphides of the heavy metals. The method depends on the fact that these substances admit of being prepared as uniformly thin layers, which show interference-colours varying with the thickness of the layer. When these colours are examined by the spectroscope, we see spectra of alternate bright and dark bands, from the number and position of which the wave-length of light in the substance, and therefore the refractive index, not only \textit{en bloc}, but also for the different colours or Fraunhofer's lines, may be deduced. From the comparative perfection of the methods of preparing the thin layers, the accuracy of the results which this method furnishes depends almost entirely on the delicacy of the balance, which is preferable to any indirect method of determining the thickness of the layers.

I.

To determine the wave-length from the position of the maxima or minima in the spectrum, it is first of all necessary to deduce the equations subsisting between these magnitudes with regard to elliptical polarization and absorption. As the interference-bands in the spectrum are best observed in reflected light with perpendicular incidence, I give the complete formulae for this case only.

If \( \epsilon \) denotes the thickness of a thin layer of the substance to be investigated, attached to a metal, then, according to the theory of the colours of thin plates, the intensity of reflected light with perpendicular incidence is

\[
I_r = \frac{(r + \rho)^2 - 4r \rho \sin^2 \frac{\theta}{2}}{(1 + r \rho)^2 - 4r \rho \sin^2 \frac{\theta}{2}}
\]  

(1)

In this expression \( r \) and \( \rho \) denote the amplitudes of the light reflected from the layer into the air and from the metal into the layer, when the amount of incident light is put equal to 1. \( r \) is always positive, \( \rho \) only when the refractive index of the layer lies between the indices of the two bounding media, usually air and metal; \( \rho \) is negative when it is greater or less than each of the other two. The magnitude \( \theta \) is equal to \( \left(\epsilon + \frac{\delta_1 - \delta_2}{2}\right) \frac{2\pi}{\lambda} \), in which \( \lambda \) is the wave-length of light in the substance, and \( \delta_1, \delta_2 \) denote the retardations which the light undergoes by reflection at the metal surface—that is, the reflection and refraction at the limit of the substance and the air.

Formula (1) presupposes that the material of the thin layer is perfectly transparent; in order to apply it to bodies which, although thin, exert an appreciable absorption on light, the
coefficient of absorption must be introduced. If \( k \) denotes the quantity of light passing through a layer whose thickness is 1, then, according to the law of absorption given by Herschel and Brewster, and afterwards confirmed by Bunsen, Roscoe, and others, the quantity of light which emerges from a layer of the thickness \( e \) is \( = k^e \). If in deducing the formula (1) we remember that the amplitude of light in each passage through a layer is diminished to \( k^e \), then we obtain for the intensity of the reflected light the expression

\[
I_r = \frac{(r + \rho k^2e)^2 - 4r \rho k^e \sin^2 D}{(1 + r \rho k^e)^2 - 4r \rho k^e \sin^2 D}.
\]

(2)

The first differential of this magnitude for \( e \) gives as common condition for the maxima and minima of the intensity of light the equation

\[
\sin^2 D = \frac{1 + \alpha \cdot \lambda^2 \cdot \log^2 k + \sqrt{1 + \beta \cdot \lambda^2 \cdot \log^2 k}}{1 + \gamma \cdot \lambda^2 \cdot \log^2 k},
\]

(3)

out of which, for the minima,

\[
\sin^2 D = \frac{1}{2} \left( 1 + \alpha \cdot \lambda^2 \cdot \log^2 k + \sqrt{1 + \beta \cdot \lambda^2 \cdot \log^2 k} \right) \frac{1}{1 + \gamma \cdot \lambda^2 \cdot \log^2 k}.
\]

(4)

results, in which the sign of the square root is positive, and the coefficients \( \alpha, \beta, \gamma \) are as follows:

\[
\alpha = \frac{(1 + \rho^2 k^2) \left( 1 + \frac{\rho}{r} + \rho r + \rho^2 k^2 \right)}{16 \pi^2 (1 - \rho^2 k^2)^2}.
\]

(5)

Equations (4) and (5) show that the influence of the absorption is only perceptible when the natural logarithm of the coefficient of absorption \( k \) has a value equivalent to the reciprocal value of the wave-length \( \lambda \). For all bodies which, with the thickness of \( 4\lambda \), are still perceptibly transparent to light of the wave-length \( \lambda \), from the smallness of the coefficients \( \alpha, \beta, \gamma \) the terms multiplied by \( \lambda^2 \) almost entirely disappear in equation (4), and
\( \sin D \) acquires the values +1 and -1 for the minima of intensity of the light; that is to say, the argument \( D \) or \( \left( \epsilon + \frac{\delta_1 - \delta_2}{2} \right) \frac{2\pi}{\lambda} \)

becomes equal to an indefinitely large multiple of \( \pi \); consequently

\[ \epsilon + \frac{\delta_1 - \delta_2}{2} = m \cdot \frac{\lambda}{2}, \ldots \ldots \ldots \ldots \ldots \ldots (6) \]

in which \( m \) may be any positive whole number. The magnitudes \( \delta_1 \) and \( \delta_2 \) cannot be experimentally determined with sufficient accuracy by means of the formula which the theory of elliptical polarization gives; but even if they could, in order to obtain the most correct value of \( \lambda \) possible we should nevertheless have to proceed according to the following reasoning. Let \( \epsilon_1 \) and \( \epsilon_2 \) be two values of \( \epsilon \) and \( m_1 \) and \( m_2 \) the values of \( m \), which satisfy equation (6); then by subtraction, since \( \delta_1 \) and \( \delta_2 \) in both cases remain the same,

\[ \lambda = 2 \cdot \frac{\epsilon_2 - \epsilon_1}{m_2 - m_1}, \ldots \ldots \ldots \ldots \ldots \ldots (7) \]

According to this formula, the differences of the thicknesses \( \epsilon_2 \) and \( \epsilon_1 \) of the layer for which the minima of the intensity of light occur are proportional to the wave-length, which may be verified by observation.

The observations are now conducted in the following manner. If by any of the following methods a thin layer of the body to be examined is prepared and then observed in reflected light through the spectroscope, the solar spectrum is at first seen unchanged in the field of view; when the layer has reached a certain thickness, a dark band appears at the more refrangible end, which, with increasing thickness of the layer, moves through the spectrum, and after a definite period again appears in the original place. If the thickness of the layer is still further increased, then two, three, and more dark bands gradually appear in different places of the spectrum. The bands increase in sharpness, and, when more than three are together at the same time, are sometimes so dark that direct sunlight is needed in order to recognize Fraunhofer's lines: generally cloud-light is sufficient to fix their position in the spectrum. If we wish to determine the wave-length of the light in the substance for any particular Fraunhofer's line, then the layer must be made so thick that a dark least band appears in this place; the layer with its support is placed in a balance which is very delicate for small weights, and is then strengthened until the bands appear for the second, third, or fourth time in the same place of the spectrum. The increases in weight give the magnitudes \( \epsilon_2 - \epsilon_1, \epsilon_3 - \epsilon_1, \epsilon_4 - \epsilon_1 \) and
so forth; and the differences \( m_2 - m_1 \) have for those cases the values 1, 2, 3. With a little practice it is not difficult to determine by time the increase of the layer, and in this way to obtain beforehand the moment in which the minima reappear in the same place of the spectrum.

If \( p \) denotes the increase in weight of the layer determined by the balance after a minimum has passed \( m \) times a given Fraunhofer's line, \( s \) the specific weight, \( o \) the surface of the layer, then the refractive index of the substance for the line in question is

\[
\frac{n_a}{l_a} = \frac{m \cdot s \cdot o \cdot l_a}{2p}, \ldots \ldots \ldots \ldots (8)
\]

when \( l_a \) denotes the wave-length of the light in air for that line.

II.

For the production of thin layers, with various substances several methods often present themselves; they cannot, however, always be used for the production of good interference-layers. By heating iron in air, for instance, very thin layers of protosesquioxide of iron are obtained, a body which, prepared electrolytically, gives distinct interference-spectra and beautiful colours. If it be attempted, however, to strengthen by further heating the thin layers which only show the first dull colours, we get neither the beautiful colours of the second series, nor generally any interference-bands in the spectroscope. The reason of this can be easily shown: the layers obtained by heating are only pure protosesquioxide on the surface; within they get less oxygen; and from a body which from the outside to the inside is continually changing its refractive index the interference-phenomena cannot be expected.

The best interference-layers are obtained by suitable action of chemical agents on thin metal layers, or by electrolysis. I have prepared numerous interference-layers by both methods; but at present I confine myself to the latter, which is especially fitted for oxygen compounds of the heavy metals.

Nobili (Pogg. Ann. vol. x.) described a great number of liquids the electrolysis of which gives coloured rings on plates of gold, platinum, and other metals; but they are of no value for the production of interference-layers. It is easy to prove, for instance, that the rings which he obtained on the positive electrodes of silver, copper, zinc, and bismuth consisted of the oxygen compounds of these metals, and were produced by oxidation of the plates by means of the electrolyzed oxygen, but contained nothing whatever of the substance of the electrolytes used. If Nobili had used dilute sulphuric acid or an alkaline solution instead of those different solutions, he would with a corresponding intensity
of the current have obtained the same rings. The only exceptions to this are the few precipitates which he obtained on gold or platinum. In the following paragraphs I explain the methods by which serviceable interference-layers are best obtained for optical examination, and how their optical constants and specific weight are determined.

1. Suboxide of Copper.

In the preparation of the interference-layers of this body two different ways are available. In both cases a solution consisting of 30 grs. hydrate of soda, 60 grs. seignette salts, and 25 grs. blue vitriol in 500 cubic centims. of water, serves as a decomposing liquid. Using as cathode a thin platinum plate about 1 decimetre square, and, as anode, two* copper points at a distance of 2 centims. on each side of the middle point of the plate (each presenting a surface of a few square millimetres), by means of the current of a small† Bunsen’s element only so much hydrogen is condensed on the platinum plate as will reduce the oxide of copper to suboxide. The surface of the anode may at first be taken a little larger, for the first precipitate is formed slowly; the anode may afterwards be lessened to the prescribed size, to be certain that no metallic copper is reduced along with the protoxide. If the anode be too large, then at first pure protoxide is obtained; but afterwards the hydrogen, which is developed more abundantly, reduces the oxide either wholly or partially to metal. The limit between the two processes, however, is tolerably wide; and the smallest traces of metallic copper can be recognized by placing iodine on it, which leaves the suboxide perfectly unchanged, whilst it immediately changes the metal into subiodide.

If the surface of the platinum was sufficiently clean (the cleansing is best done by rubbing the plate with linen cloth soaked with a solution of soda, and by placing the plate for some time as a cathode in an alkaline solution so that it becomes electrolytically covered with hydrogen), the colours soon appear in the following order—gold, brown, purple, blue, &c.; and repeat themselves within a few hours three or four times almost unchanged; as the thickness increases, only pale green and pale red

---

* I must here observe that the laws of divided currents in fixed conductors for this and similar fluids are by no means applicable; the influence of polarization so preponderates that, even by using one point, both sides of the cathode almost equally colour themselves. This is so much more the case the weaker the current and the slower the decomposition.

† The resistance of alkaline copper solution is so considerable that a small piece of copper or zinc wire accomplishes the same effect as a large Daniell’s element, if in both cases equal exciting fluids are applied.

‡ All the substances investigated show at first Newton’s rings in the transmitted light when the reflection takes place from a metal.
alternate, which after about five or six hours blend into one another and form a pale brownish red closely resembling metallic copper. If the layer be then observed with the spectroscope, four dark bands will show themselves between Fraunhofer’s lines F and B. The layer can be made more than double as thick, and then in the interval named eight or nine minimum-bands obtained. Such a spectrum is, in consequence of the bounding of the dark bands by the high intensities of light of the maxima, far more brilliant than an ordinary absorption-spectrum. The spectrum and also the colours are most splendid when the reflection takes place from silver; for this purpose a thin silvered glass forms the best support for the layer, which moreover conducts sufficiently well and makes possible the examination of the layers in transmitted light.

Cleansing the surface of the platinum from impurities, which are particularly obstructive to the formation of a uniform layer, is most easily accomplished if it is first covered with copper by means of an alkaline solution of copper. Instead of platinum, ordinary tinfoil may be applied with advantage, which, before the plating, is cleaned by breathing on it and polishing it with a linen pad sprinkled with prepared chalk. The tinfoil is coppered by the current of a weak Bunsen’s element in an alkaline copper solution; the tinfoil covers itself uniformly with a bright copper coating and with condensed hydrogen, which, precipitated simultaneously with the copper, adheres far better to the plate than it would without this—that is, better than if the tinfoil in a soda solution were to be covered with hydrogen. After from ten to fifteen minutes the current is broken, and the copper-covered tinfoil, without being washed, is instantly suspended in the above-mentioned solution of copper. The reduction by the hydrogen adhering to the plate soon begins; the copper is gradually coloured golden-yellow, red, blue, whitish, greenish yellow, and again golden yellow. It takes about fifty minutes for these colours to develop themselves; the development afterwards proceeds slowly; but it is best to interrupt it at the second golden-yellow, where a minimum near the line F appears in the spectroscope, and to cover the plate afresh with hydrogen if stronger layers are wanted immediately. For this purpose a soda solution of 1.035 specific gravity is used, which is first diluted with an equal quantity of water; if the lye or the current be too strong, the suboxide of copper may be reduced to metallic copper. With the prescribed data, in ten minutes the layer of suboxide of copper will be covered with a layer of hydrogen, which within an hour reduces from the solution of copper a layer of suboxide of the thickness of half a wave-length. This method may also be used as a convenient lecture experiment, to make visible the hydrogen
condensed on the surface of a metal, and to determine its amount quantitatively.

To ascertain the specific gravity of the suboxide of copper, the absolute weight and also the loss of weight of a platinum plate in water were determined by several experiments, and it was then coated with a thicker layer of suboxide (being exposed twelve, and from that to thirty-six hours to the action of the current). When the layer becomes opaque, the copper colour changes to a dark violet; the least traces of metallic copper, which in the event of a too strong current might be mixed, would change the beautiful violet into an ugly dark brown. When the layer has reached the desired thickness, the absolute weight and the loss of weight of the whole plate are determined.

From four experiments with layers of different thicknesses and almost perfectly agreeing in their results, the density of the suboxide of copper (at 15° C.) was found to be

\[ s = 5.975. \]

To determine the refractive indices, a rectangular sheet of tin-foil, 10.72 by 7.76 centims., was copper-plated, and then a uniform layer of suboxide of copper deposited on it.

The increase of weight after the minimum-bands had passed eleven times through the line \( F \) amounted to 0.08975 gr., and after seven passages of the band through the line \( C \) to 0.08970 gr. From these numbers and equations (8) and (9), together with Fraunhofer's numbers for the wave-lengths of these lines in air, we calculate the refractive exponents for \( F \) and \( C \) to be 2.963 and 2.558.

A second rectangular sheet of tinfoil, whose surface was 18230 square millims., gave for the refractive indices of the lines \( E \) and \( D \), after nine and six passages of the minimum-band, the numbers 2.816 and 2.705 respectively.

A third plate, 21946 square millims. in surface, gave for \( B \) the value 2.534; so that the refractive indices of suboxide of copper (\( \text{Cu}_2\text{O} \)) are as follows:

<table>
<thead>
<tr>
<th>Fraunhofer's line</th>
<th>Refractive index</th>
</tr>
</thead>
<tbody>
<tr>
<td>( B )</td>
<td>=2.534</td>
</tr>
<tr>
<td>( C )</td>
<td>=2.558</td>
</tr>
<tr>
<td>( D )</td>
<td>=2.705</td>
</tr>
<tr>
<td>( E )</td>
<td>=2.816</td>
</tr>
<tr>
<td>( F )</td>
<td>=2.963</td>
</tr>
</tbody>
</table>

The refraction, like the dispersion, of suboxide of copper is therefore, apart from the doubtful determinations, the greatest which has hitherto been observed for a solid body.
2. Hydrated Peroxide of Lead.

By the electrolytic decomposition of a solution of oxide of lead in potash at the positive pole, a series of colours is obtained which is commonly used to represent Nobili's rings. The solution for this purpose is prepared by boiling litharge with a strong alkaline lye. As, however, comparatively little lead goes into solution, and operations with strong alkaline fluids are attended with inconvenience, it is more judicious to prepare the solution in the following manner:—50 grms. of acetate of lead dissolved in water are poured, whilst being stirred, into a solution of 50 grms. of tartar and 35 grms. of hydrate of soda, and after the precipitate has disappeared is diluted with water to 500 cubic centims. This liquid still gives good results when three-fourths of the lead have been precipitated by the electrolytic process.

As a positive electrode I have always used a platinum plate about 1 decimetre square, which is preferable for the measurements, as other metals might become oxidized by the active oxygen; the negative pole consists of two small plates of lead or platinum, arranged on both sides of the platinum plate at equal distances from the edges and bent sharply downwards. This system, after insulation of the conducting-wires, it is easy to regulate so that the positive platinum plate shall be uniformly coloured. With these dimensions of the apparatus a small weak Bunsen's element is sufficient to produce the layers.

It appears to be generally considered that the body which gives the interference-colours is peroxide of lead; but the far lower specific gravity (the statements of the density of PbO₂ vary between 8·903 and 8·933) which I found as the result of numerous concordant experiments induced me to investigate it more closely. It followed that the coloured layers on the positive-pole plate, whatever solutions were applied, are never PbO₂, but a hydrated oxide of definite composition, which only with difficulty entirely loses its water at a higher temperature, without at the same time any oxygen disappearing. In a thicker layer it forms a compact, bright, blue-black body, which adheres firmly to the platinum, and is by no means hygroscopic, so that its specific gravity is easily determined. Three determinations, which differed very little in their results, gave, as the mean, for the density of hydrated peroxide of lead

\[ s = 6.169, \]

a number which is very different from the specific gravity of the dry hydrated peroxide of lead.

For the investigation of the refraction and dispersion, it is prudent to place the spectroscope in front of the decomposition-apparatus with glass sides; in this way, since the liquid is colourless, *Phil. Mag. S. 4. Vol. 40. No. 265. Aug. 1870.*
the passage of the interference-bands in the spectrum can be observed and the current broken at the proper moment. The colours form far more quickly than with suboxide of copper; the strength of the current is so regulated that the layers increase half a wave-length within from seven to ten minutes or a little longer. If the increase takes place too quickly, the layers as they thicken become brittle, and the colours are not perfectly pure.

Whilst with suboxide of copper, for light having the refraction of Fraunhofer's line F, the minimum-bands can be plainly observed, yet with hydrated peroxide of lead this is no longer the case for line E; in the whole of the more refrangible portion of the spectrum no trace of interference-bands is observable, whilst in the yellow, and still more in the red part, they occur with great sharpness. From this it must be inferred that the body, even in layers of the thickness of one or a few wave-lengths, is only transparent for yellow and red rays.

To determine the refractive index for the line D, a thin platinum plate 13570 square millims. in surface was introduced into the decomposition-apparatus, and after it had been tared the passage of a minimum-band was observed fifteen times through that line. The increase in weight of this layer, fifteen half wave-lengths thick, amounted to 0.166 milligrm.; therefore the refractive index, according to formula (8), is

\[ n_a = \frac{15 \times 13570 \times 6.169 \times 0.0005888}{2 \times 0.1660} = 2.229. \]

For the line C a similar inspection gave the value \( n(C) = 2.010 \). When the layer reaches a thickness of twenty-four half wave-lengths (D), then from D downwards in the red part of the spectrum six minimum-bands appear, of which the last is almost coincident with line B. Whilst a minimum travels from D to B, the thickness of the layer increases about \( \frac{1}{2} \lambda(D) \); so that nineteen wave-lengths \( \lambda(B) = 23.5 \lambda(D) \). From this it follows that \( n(B) = 1.802 \). The latter consideration may therefore serve as control for the direct determination by weighing. The numbers

\[ n(D) = 2.229 \]
\[ n(C) = 2.010 \]
\[ n(B) = 1.802 \]

show that the refraction by hydrated peroxide of lead is less, but the dispersion somewhat greater than that by suboxide of copper.

3. Hydrated Peroxide of Manganese.

The interference-layers of this body are best obtained by electrolysis of very dilute neutral solutions of salts of protoxide of manganese; 12 grms. chlorate of manganese and 8 grms. ace-
and the Dispersion of Opaque Bodies. 115
tate, dissolved in 500 cubic centims. of water, form a liquid which
gives good results even when a great portion of the manganese
is separated by the electrolytic process. Concentrated solutions
are by no means applicable; even the liquids which Nobili
(Pogg. Ann. vol. x.) and Böttger (vol. i.) used for the preparation
of the coloured rings are too concentrated. A weakly-charged
element is used along with the decomposition-apparatus described
for hydrated peroxide of lead; as the liquid is colourless, the
progress of the interference-bands during the operation can be
observed through the spectroscope. The strength of the cur-
rent is best directed so that the layer grows half a wave-length
within from fifteen to thirty minutes; if the growth goes on too
quickly, the thicker bright blue-black layer becomes brittle,
and with a change of temperature, especially by a wash of cold
water, easily cracks.

To determine the specific gravity, layers of more than 100
wave-lengths in thickness and of the absolute weight of about
0:5 grm. were prepared; from two concordant experiments the
density at 13° C. was

\[ s = 2.542. \]

The body is not MnO₂, but, like all bodies of this group, a
hydrate which does not lose its water under the air-pump. It
is transparent to green and blue rays in quite thin layers only,
of from 1 to 2 wave-lengths, so that minimum-bands can be ob-
served in E and F; with greater thicknesses the same only appear
in the yellow and red. The minimum in F, however, is so broad
and faint that the wave-length cannot be determined from it by
the simple spectroscope without the application of photometrical
means. For the lines E, D, C, I have obtained according to the
foregoing methods the values

\[ n(E) = 1.944, \]
\[ n(D) = 1.862, \]
\[ n(C) = 1.801. \]

III. General Conclusions.

Besides the bodies already described, I also prepared a num-
ber of interference-layers in electrolytic and chemical ways,
which, like those described, are distinguished by an unusually
strong dispersion. The examination of these bodies has led to the
conclusion that all bodies of strong dispersion have optical pro-
properties in common which appear of interest for the theory of light.

It is known from experience that dispersion and absorption
are related to one another; and Cauchy, in his "Mémoire sur la
dispersion de la Lumière," has given an equation in which this
On the Refractive Indices and the Dispersion of Opaque Bodies.

relation is implicitly contained. The discussion of this equation, of which one side is an infinite series, presents some difficulties; it has been thought* sufficient to retain the first two terms of this series and neglect the rest. This would be permissible, as M. Christoffel has shown, if in every case the sphere of action were infinitely small in comparison with the wave-length. That the last assumption is not admissible, however, the discussion of the incomplete equation shows; for it yields the result that every spectrum is bounded at the violet end by a visible beam of definite refraction. This inference is a physical absurdity, since it presupposes the existence of bodies which, under any arbitrary angle of incidence, totally reflect a visible beam, or completely absorb it on the surface. The dispersion-formula derived from the imperfect series, even if correct for large wave-lengths, can offer no explanation as to what really occurs at that limit at the more refrangible end of the spectrum.

Whilst this limit with substances of weak dispersion would lie very far in the ultra-violet, it sometimes appeared in the green in the bodies which I investigated. With no single body of this group were even traces of interference observed in the violet. The reason of this phenomenon might be sought in a strong reflection of these rays at the surface, or in a strong absorption in the interior; it has been shown that the latter is the preponderating cause of the absence of interference-bands. Then they always vanish gradually with increasing thickness from the violet to the red end of the spectrum, and are very soon only present in the yellow and red. Hence the absorption increases with decreasing wave-lengths, and indeed continuously so for a certain position in the spectrum which is special to each substance, and so quickly that on the other side of it no ray can pass through a layer of the thickness of half a wave-length.

Hence in transmitted light sufficiently thick layers of bodies of preeminent dispersion always appear yellow-red or red. I have sought in vain for a substance of this kind which would be transparent with green, blue, or violet light.

To meet any objections to these matters of fact arising from the mention of apparent exceptions, I must make the following remarks.

Thin layers can be prepared in different ways which strongly absorb the light and are transparent to other than yellow or red light; such layers, however, like glass coated with soot, are not to be regarded as bodies, but as loosely connected apparatus of individual particles, and can only be quoted as exceptions if it be proved generally that they possess refractive and dispersive

properties. For example, let chlorine, bromine, iodine, sulphur-vapour, or sulphuretted hydrogen act on thin layers of silver; then layers of chloride, iodide, and sulphide of silver are obtained, which in comparison with the metals and the metallic oxides described are very transparent, and show in the spectro-scope beautiful interference-bands. If, however, the intensity or duration of the action of those agents exceeds a certain limit, the structure of the layers is destroyed; the same are then to be regarded as aggregates of many particles (in several cases microscopic crystals), although they appear to the eye as coherent masses; they are more opaque than the metal itself, and show no trace of interference-bands in the spectro-scope.

In reference to the chemical combination of the oxygen-com-pounds of the heavy metals prepared by electrolysis, the follow-ing result has been found:—Those compounds separated by the current at the positive pole are not, as has hitherto been commonly assumed*, peroxides, but definite hydrates of the same, which do not lose their moisture under the air-pump. I believe I am able to lay this down as a general proposition, as I have proved it for the most different metals, namely lead, manganese, cobalt, bismuth, and antimony. The oxides and suboxides separated at the negative pole are, on the contrary, always free from water, as must be inferred from the examination of the electrolytic subs-oxides of copper, bismuth, antimony, and oxide of iron.

Berlin, October 1869.

---

XV. On the Determination whether the Corona is a Solar or Ter-restrial Phenomenon. By George M. Seabroke, Esq.†

It is my intention in this paper to attempt to show that, with the existing state of our knowledge of the corona, the theory set forth by Mr. Lockyer, that the corona is a terrestrial phenomenon, is quite possible, rather than to show that other theories are wrong; and further to demonstrate how the question may be set at rest by observations on future eclipses. The points which present themselves are as follows:—

1. What are the facts with respect to the spectra of the corona seen in past eclipses?

2. What spectra ought we to obtain from the corona on the terrestrial theory during totality?

3. Are the spectra obtained from the corona in past eclipses reconcilable with those we ought to get on the above hypothesis?

† From the Monthly Notices of the Royal Astronomical Society, June 10, 1870.
4. What spectrum ought we to get from the corona after totality?
5. What spectrum ought we to get before totality on the following side of the moon?
6. What difference will there be between the spectrum of the central portions of the corona and that of the distant parts during totality?

With regard to 1. During the Indian eclipse, Major Tennant writes:—"Directly I saw the whole moon in the finder I set the cross-wires immediately outside its upper limb. By the time I got to the spectroscope the cloudy range seen in the photographs had vanished from the slit, and I saw a faint continuous spectrum. Thinking that want of light prevented my seeing the bright lines which I had fully expected to see in the lower strata of the corona, I opened the jaws of the slit and repeatedly adjusted by the finder, but without effect. What I saw was undoubtedly a continuous spectrum, and I saw no bright lines. There may have been dark lines, of course; but with so faint a spectrum and the jaws of the slit wide apart they might escape notice." With respect to the American eclipse, Professor Pickering, with an ordinary chemical spectroscope directed to the sun's place during totality, saw a continuous spectrum with two or three bright lines, one "near E" and a second "near C." Professor Young, while examining a part of the prominence at +146°, saw C, near D, a line at 1250 ± 20, and another at 1350 ± 20, and the 1474 K line very bright, but not equal to C and D₃; but he observed that the 1474 K line, unlike C and D₃, extended across the spectrum; and on moving the slit away from the prominence it persisted, while D₃ disappeared. He also believes that the two faint lines between it and D₃ behaved in like manner. On examining a prominence on the other side of the sun, he observed nine lines and a faint continuous spectrum without any traces of dark lines in it.

As to the second point, let us find what spectrum we ought to obtain from a corona at a point on the earth where the limbs of the sun and moon are in line,—that is, where the eclipse is total exactly.
Let A be a point on the earth where the sun is eclipsed;  
B C, limit of earth’s atmosphere;  
D, the moon;  
H E, photosphere of sun;  
E F, the apparent corona.

Now, if the corona be terrestrial, the light producing it must be reflected or separated from the atmosphere within the triangle A B C.

Join B D and produce to G.

Then G is the most distant point from the limb on the sun’s disk from which light is reflected to A by the atmosphere; and if the triangle E A F or angular extent of the corona from the sun is given, we can find \( \angle E A G \).

The angles being small,  
\[
\frac{\angle E A G}{\angle E A F} = \frac{G E}{E F}
\]

approximately.

\[G E : C B :: E D : D C, \text{ therefore } G E = C B \frac{E D}{D C}, \quad (1)\]

and  
\[E F : C B :: E A : C A, \text{ therefore } E F = C B \frac{E A}{C A}; \quad (2)\]

and \( E D = E A - A D \); and \( A D \) being small in proportion to \( E A \), \( E D \) may without great error be taken as equal to \( E A \).

Dividing (1) by (2),

\[
\frac{G E}{E F} = \frac{E D}{E A} = \frac{C A}{D C} = \frac{\text{height of atmosphere}}{\text{dist. of moon—height of atm.}},
\]

\[
\angle E A G \frac{\text{height of atmosphere}}{\text{dist. of moon—height of atmosphere}}
\]

\[
E A G = E A F \frac{\text{height of atmosphere}}{\text{dist. of moon—height of atmosphere}}.
\]

If, for example, we now take  
\[E A F = 30',\]

and  
Height of atmosphere = 100 miles,

and  
Dist. of moon—height of atmosphere = 240,000 miles,

then  
\[
\angle E A G = 30' \frac{100}{240,000} = 0'75.
\]
Therefore the only part of the photosphere available in this case for illuminating the atmosphere is a ring of photosphere 0".75 in width; and from the figure it will be seen that only that part of the corona most distant from the centre (as at B) will receive even the whole of this light; and it is manifest from the figure that the nearer any part of the corona is to the centre (nearer C) the less light will it receive from the photosphere, so that the mean illumination of the corona by the photosphere is only equal to that which would be given by a ring \( \frac{1}{2} \times 0".75 = 0".375 \) wide.

Now, since the chromosphere extends from E towards F, the whole of the atmosphere producing the corona is illuminated equally by the chromosphere; and since the mean height of the chromosphere is much more than 0".375, or other height deduced from the foregoing formula, it is quite possible that the dark lines of the spectrum coming from so small an area of photosphere may be blotted out, as Mr. Lockyer observes, by the light from a greater area of chromosphere wherever the chromosphere contains the proper substances; and it is probable that the vapours of a number of substances from the photosphere are carried up into the chromosphere in small quantities sufficient to obliterate the dark lines, since we find the vapours of magnesium, sodium, barium, and iron sometimes in the chromosphere.

Although the total amount of light of all kinds given by an equal area of chromosphere is small compared with that given by an equal area of photosphere, still each particular kind of light from the chromosphere is as intense, or nearly so, as that particular kind of light from the photosphere; so that if equal areas of chromosphere and photosphere be illuminating a part of our atmosphere, that part would give a spectrum having its dark lines erased by the chromosphere, or a continuous spectrum. When the area of the photosphere is much less than that of the chromosphere, the bright lines given by the chromosphere would be much more visible than the remaining dark lines of the photospheric spectrum.

From this it appears that during totality we ought to get from the corona a nearly continuous spectrum, with bright lines given by the substances in the chromosphere. Some of the dark lines of the photospheric spectrum ought to remain, where the chromosphere does not contain substances giving bright lines in their place. Where the illuminating areas of the photosphere and chromosphere are equal, which is possible where the chromosphere is unusually low, we ought to obtain a spectrum as above, but without bright lines, the chromospheric lines being then only just able to obliterate the dark ones.

3. In the Indian eclipse Major Tennant saw a continuous
the Corona is a Solar or Terrestrial Phenomenon. 121

spectrum without bright lines, which is that we should obtain on the above hypothesis when the areas of chromosphere and photosphere illuminating our atmosphere are equal; but it is shown above that during totality with the ordinary height of chromosphere the illuminating area of chromosphere is much greater than that of the photosphere, so that the part of the chromosphere illuminating that part of the corona under examination must have been unusually low, or, as was probably the case, there were bright lines; for, as he says the spectrum was very faint, they may have been missed. There ought on this hypothesis to have been dark lines; but Major Tennant says that with so wide a slit he might have missed them. Professor Pickering saw a continuous spectrum with bright lines, which is what we ought to obtain when the atmosphere is illuminated by a greater area of chromosphere than photosphere, as has been shown to be the case when the chromosphere is at its normal height. The dark lines which ought to have been visible on Mr. Lockyer’s theory might possibly have been too faint to be noticed, since, as stated above, the area of photosphere in this case would be small in proportion to that of the chromosphere, so that the bright lines would appear very plainly when the photospheric spectrum was too faint to render the dark lines visible. As to the 1474 K line observed by Professor Young to extend across the spectrum beyond the other lines of the chromosphere, Mr. Lockyer observes that he often sees this line and often does not, which appears fatal to this being a real corona line, as, if so, it ought always to be visible. Professor Young also seems, in the note to his observations, to be doubtful how far this line extended from the prominence; and it is very probable that this line is either iron or hydrogen. There seems to be no evidence that the other lines seen in the corona spectrum are not chromospheric lines.

4. With regard to this point, an inspection of the figure will show that, as the moon passes over the sun, more photospheric light becomes available for illuminating the corona; but so long as the available area of the photosphere is less than that of the chromosphere, the dark lines of the spectrum, due to the photosphere, will be erased by the chromospheric lines (wherever the chromosphere contains the proper substances); and as the moon moves forward the spectrum should on this hypothesis change; and when the illuminating area of the photosphere becomes greater than the area of the chromosphere, the dark lines of the photospheric spectrum should appear. It will also be seen that the larger the illuminating area of the photosphere becomes, the smaller will be the difference between the spectrum of the interior part of the corona and that of the exterior part, since, whatever
be the extent of the illuminating surface of the photosphere, the exterior parts of the corona will only receive an excess of light over that received by the interior part equal to the amount of photospheric light received by those parts during totality, or, as in the case above taken, the excess will be equal to that given by a ring of light from the photosphere 0′.75 wide (or GE in the figure), so that, when a few seconds of photosphere are visible to the observer, the difference between the spectra of the exterior and interior parts of the corona would be inappreciable.

5. What spectrum ought the corona to give before totality on the following side of the moon? In this case, when the angular distance of the limits of the sun and moon is some seconds, the difference between the spectra of the exterior and interior parts of the corona is small, since no part of the atmosphere in this case will be illuminated by the photosphere; so we ought to obtain a chromospheric spectrum, together with a faint photospheric one caused by a small amount of photospheric light reflected from the photosphere by the chromosphere.

6. On the foregoing hypothesis, during totality the parts of the corona nearest the centre should give a different spectrum from the more distant portions, since the portions nearer the centre receive less photospheric light than the more distant parts, and the same amount of light from the chromosphere.

In order to test the correctness of this theory, advantage may be taken of the following facts:—1st. At that period of the eclipse when the limb of the sun and moon are in line with the observer, there will be a difference between the central and distant parts of the corona; and this difference will decrease as the moon passes on, whereas, by the other theory, there should be the same difference as long as the corona is visible. 2nd. If the corona be terrestrial, the spectrum of any portion of it ought to be continually changing during the passage of the moon; but if solar, the spectrum should remain unchanged.

XVI. On a Mechanical Theorem applicable to Heat.

By R. Clausius*.

In a treatise which appeared in 1862, on the mechanical theory of heat†, I advanced a theorem which, in its simplest form, may be thus expressed:—The effective force of heat is

* Translated from a separate impression communicated by the Author, having been read before the Niederrheinischen Gesellschaft für Natur- und Heilkunde, on June 13, 1870.
proportional to the absolute temperature. From this theorem, in conjunction with that of the equivalence of heat and work, I have, in the subsequent portion of that treatise, deduced various conclusions concerning the deportment of bodies towards heat. As the theorem of the equivalence of heat and work may be reduced to a simple mechanical one, namely that of the equivalence of vis viva and mechanical work, I was convinced à priori that there must be a mechanical theorem which would explain that of the increase of the effective force of heat with the temperature. This theorem I think I shall be able to communicate in what follows.

Let there be any system whatever of material points in stationary motion. By stationary motion I mean one in which the points do not continually remove further and further from their original position, and the velocities do not alter continuously in the same direction, but the points move within a limited space, and the velocities only fluctuate within certain limits. Of this nature are all periodic motions—such as those of the planets about the sun, and the vibrations of elastic bodies,—further, such irregular motions as are attributed to the atoms and molecules of a body in order to explain its heat.

Now let \( m, m', m'', \&c. \) be the given material points, \( x, y, z, x', y', z', x'', y'', z'', \&c. \) their rectangular coordinates at the time \( t \), and \( X, Y, Z, X', Y', Z', X'', Y'', Z'', \&c. \) the components, taken in the directions of the coordinates, of the forces acting upon them. Then we form first the sum

\[
\sum \frac{m}{2} \left[ \left( \frac{dx}{dt} \right)^2 + \left( \frac{dy}{dt} \right)^2 + \left( \frac{dz}{dt} \right)^2 \right],
\]

for which, \( v, v', v'', \&c. \) being the velocities of the points, we may write, more briefly,

\[
\sum \frac{m}{2} v^2,
\]

which sum is known under the name of the vis viva of the system. Further, we will form the following expression:

\[
-\frac{1}{2} \sum (Xx + Yy + Zz).
\]

The magnitude represented by this expression depends, as is evident, essentially upon the forces acting in the system, and, if with given coordinates all the forces varied in equal ratio, would be proportional to the forces. We will therefore give to the mean value which this magnitude has during the stationary motion of the system the name of Virial of the system, from the Latin word vis (force).
In relation to these two magnitudes the following theorem may now be advanced:—

The mean vis viva of the system is equal to its virial.

Distinguishing the mean value of a magnitude from its variable value by drawing a horizontal line over the formula which represents the latter, we can express our theorem by the following equation:—

\[ \sum \frac{m}{2} v^2 = - \frac{1}{2} \sum (Xx + Yy + Zz). \]

As regards the value of the virial, in the most important of the cases occurring in nature it takes a very simple form. For example, the forces which act upon the points of the mass may be attractions or repulsions which those points exert upon one another, and which are governed by some law of the distance. Let us denote, then, the reciprocal force between two points of the mass, \( m \) and \( m' \), at the distance \( r \) from each other, by \( \phi(r) \), in which an attraction will reckon as a positive, and a repulsion as a negative force; we thus have, for the reciprocal action:—

\[ Xx + X'x' = \phi(r) \frac{x'}{r} x + \phi(r) \frac{x - x'}{r} x' = - \phi(r) \frac{(x' - x)^2}{r}. \]

And since for the two other coordinates corresponding equations may be formed, there results

\[ - \frac{1}{2} (Xx + Yy + Zz + X'x' + Y'y' + Z'z') = \frac{1}{2} \phi(r). \]

Extending this result to the whole system of points, we obtain

\[ - \frac{1}{2} \sum (Xx + Yy + Zz) = \frac{1}{2} \sum \phi(r), \]

in which the sign of summation on the right-hand side of the equation relates to all combinations of the points of the mass in pairs. Thence comes for the virial the expression

\[ \frac{1}{2} \Sigma r \phi(r) ; \]

and we immediately recognize the analogy between this expression and that which serves to determine the work accomplished in the motion. Introducing the function \( \Phi(r) \) with the signification

\[ \Phi(r) = \int \phi(r) dr, \]

we obtain the familiar equation

\[ - \Sigma (Xdx + Ydy + Zdz) = d \Sigma \Phi(r). \]

The sum \( \Sigma \Phi(r) \) is that which, in the case of attractions and re-
pulsions, which act inversely as the square of the distance, is named, irrespective of the sign, the reciprocal potential of the system of points. As it is advisable to have a convenient name* for the case in which the attractions and repulsions are governed by any law whatever, or, more generally still, for every case in which the work accomplished in an infinitely small motion of the system may be represented by the differential of any magnitude dependent only on the space-coordinates of the points, I propose to name the magnitude whose differential represents the negative value of the work, from the Greek word ἐργαλ (work), the ergal of the system. The theorem of the equivalence of vis viva and work can then be expressed very simply; and in order to exhibit distinctly the analogy between this theorem and that respecting the virial, I will place the two in juxtaposition:—

(1) The sum of the vis viva and the ergal is constant.

(2) The mean vis viva is equal to the virial.

In order to apply our theorem to heat, let us consider a body as a system of material points in motion. With respect to the forces which act upon these points we have a distinction to make: in the first place, the elements of the body exert upon one another attractive or repulsive forces; and, secondly, forces may act upon the body from without. Accordingly we can divide the virial into two parts, which refer respectively to the internal and the external forces, and which we will call the internal and the external virial.

Provided that the whole of the internal forces can be reduced to central forces, the internal virial is represented by the formula above given for a system of points acting by way of attraction or repulsion upon one another. It is further to be remarked that, with a body in which innumerable atoms move irregularly but in essentially like circumstances, so that all possible phases of motion occur simultaneously, it is not necessary to take the mean value of \( r\phi(r) \) for each pair of atoms, but the values of \( r\phi(r) \) may be taken for the precise position of the atoms at a certain moment, as the sum formed therefrom does not importantly differ from their total value throughout the course of the individual motions. Consequently we have for the internal virial the expression

\[
\frac{1}{2} \Sigma r\phi(r).
\]

As to the external forces, the case most frequently to be considered is where the body is acted upon by a uniform pressure normal to the surface. The virial relative to this can be expressed

* The term force-function, besides some inconvenience, has the disadvantage of having been already used for another magnitude, which stands to the one in question in a relation similar to that in which the potential-function stands to the potential.
very simply; for, \( p \) signifying the pressure, and \( v \) the volume of the body, it is represented by

\[
\frac{3}{2}pv.
\]

Denoting, further, by \( h \) the vis viva of the internal motions (which we call heat), we can form the following equation:

\[
h = \frac{1}{2} \sum \phi(r) + \frac{3}{2}pv.
\]

We have still to adduce the proof of our theorem of the relation between the vis viva and the virial, which can be done very easily.

The equations of the motion of a material point are:

\[
m \frac{d^2 x}{dt^2} = X; \quad m \frac{d^2 y}{dt^2} = Y; \quad m \frac{d^2 z}{dt^2} = Z.
\]

But we have

\[
d \frac{d^2 (x^2)}{dt^2} = 2 \frac{d}{dt} \left( x \frac{dx}{dt} \right) = 2 \left( \frac{dx}{dt} \right)^2 + 2x \frac{d^2 x}{dt^2},
\]

or, differently arranged,

\[
2 \left( \frac{dx}{dt} \right)^2 = -2x \frac{d^2 x}{dt^2} + \frac{d^2 (x^2)}{dt^2}.
\]

Multiplying this equation by \( \frac{m}{4} \), and putting the magnitude \( X \) for \( m \frac{d^2 x}{dt^2} \), we obtain

\[
\frac{m}{2} \left( \frac{dx}{dt} \right)^2 = -\frac{1}{2} X + \frac{m}{4} \frac{d^2 (x^2)}{dt^2}.
\]

The terms of this equation may now be integrated for the time from 0 to \( t \), and the integral divided by \( t \); we thereby obtain

\[
\frac{m}{2t} \int_0^t \left( \frac{dx}{dt} \right)^2 dt = -\frac{1}{2t} \int_0^t X x dt + \frac{m}{4t} \left[ \frac{d(x^2)}{dt} - \left( \frac{d(x^2)}{dt} \right)_0 \right],
\]

in which \( \left( \frac{d(x^2)}{dt} \right)_0 \) denotes the initial value of \( \frac{d(x^2)}{dt} \).

The formulæ

\[
\frac{1}{t} \int_0^t \left( \frac{dx}{dt} \right)^2 dt \quad \text{and} \quad \frac{1}{t} \int_0^t X x dt,
\]

occurring in the above equation, represent, if the duration of time \( t \) is properly chosen, the mean values of \( \left( \frac{dx}{dt} \right)^2 \) and \( X x \), which were denoted above by \( \left( \frac{dx}{dt} \right)^2 \) and \( \overline{X x} \). For a periodic motion the
duration of a period may be taken as the time \( t \); but for irregular motions (and, if we please, also for periodic ones) we have only to consider that the time \( t \), in proportion to the times during which the point moves in the same direction in respect of any one of the directions of coordinates is very great, so that in the course of the time \( t \) many changes of motion have taken place, and the above expressions of the mean values have become sufficiently constant.

The last term of the equation, which has its factor included in the square brackets, becomes, when the motion is periodic, \( =0 \) at the end of each period, as at the end of the period \( \frac{d(x^2)}{dt} \) resumes the initial value \( \left(\frac{d(x^2)}{dt}\right)_0 \). When the motion is not periodic, but irregularly varying, the factor in brackets does not so regularly become \( =0 \); yet its value cannot continually increase with the time, but can only fluctuate within certain limits; and the divisor \( t \), by which the term is affected, must accordingly cause the term to become vanishingly small with very great values of \( t \). Hence, omitting it, we may write

\[
\frac{m}{2} \frac{d(x^2)}{dt} = -\frac{1}{2} Xx.
\]

As the same equation is valid also for the remaining coordinates, we have

\[
\frac{m}{2} \left[ \left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2 \right] = -\frac{1}{2}(Xx + Yy + Zz),
\]

or, more briefly,

\[
\frac{m}{2} v^2 = -\frac{1}{2}(Xx + Yy + Zz),
\]

and for a system of any number of points we have the perfectly corresponding one

\[
\Sigma \frac{m}{2} v^2 = -\frac{1}{2} \Sigma (Xx + Yy + Zz).
\]

Hence our theorem is demonstrated; and at the same time it is evident that it is not merely valid for the whole system of material points, and for the three directions of coordinates together, but also for each material point and for each direction separately.
XVII. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 68.]

April 28.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"On a Cause of Error in Electroscopic Experiments." By Sir Charles Wheatstone, F.R.S.

To arrive at accurate conclusions from the indications of an electroscope or electrometer, it is necessary to be aware of all the sources of error which may occasion these indications to be misinterpreted.

In the course of some experiments on electrical conduction and induction which I have recently resumed, I was frequently delayed by what at first appeared to be very puzzling results. Occasionally I found that I could not discharge the electrometer with my finger, or only to a certain degree, and that it was necessary, before commencing another experiment, to place myself in communication with a gas-pipe which entered the room. How I became charged I could not at that time explain; the following chain of observations and experiments, however, soon led me to the true solution.

I was sitting at a table not far from the fireplace, with the electrometer (one of Peltier's construction) before me, and was engaged in experimenting with disks of various substances. To ensure that the one I had in hand, which was of tortoiseshell, should be perfectly dry, I rose and held it for a minute before the fire; returning and placing it on the plate of the electrometer, I was surprised to find that it had apparently acquired a strong charge, deflecting the index of the electrometer beyond 90°. I found that the same thing took place with every disk I thus presented to the fire, whether of metal or any other substance. My first impression was that the disk had been rendered electrical by heat, though it would have been extraordinary that, if so, such a result had not been observed before; but on placing it in contact with a vessel of boiling water, or heating it by a gas-lamp, no such effect was produced. I next conjectured that the phenomenon might arise from a difference in the electrical state of the air in the room and that at the top of the chimney; and to put this to the proof, I adjourned to the adjacent room, where there was no fire, and bringing my disk to the fireplace I obtained precisely the same result. That this conjecture, however, was not tenable was soon evident, because I was able to produce the same deviation of the needle of the electrometer by bringing my disk near any part of the wall of the room. This seemed to indicate that different parts of the room were in different electrical states; but this again was disproved by finding that when the position of the electrometer and the place where the disk was supposed to be charged were interchanged, the charge of the electrometer was still always negative. The last resource was to assume that my body had become charged by walking across the carpeted room, though the effect was produced even by the most
careful treading. This ultimately proved to be the case; for resuming my seat at the table and scraping my foot on the rug, I was able at will to move the index to its greatest extent.

Before I proceed further I may state that a gold-leaf electrometer shows the phenomena as readily.

When I first observed these effects the weather was frosty; but they present themselves, as I have subsequently found, almost equally well in all states of the weather, provided the room be perfectly dry.

I will now proceed to state the conditions which are necessary for the complete success of the experiments, and the absence of which has prevented them from being hitherto observed in the striking manner in which they have appeared to me.

The most essential condition appears to be that the boot or shoe of the experimenter must have a thin sole and be perfectly dry; a surface polished by wear seems to augment the effect. By rubbing the sole of the boot against the carpet or rug, the electricities are separated, the carpet assumes the positive state and the sole the negative state; the former being a tolerable insulator prevents the positive electricity from running away to the earth, while the sole of the foot, being a much better conductor, readily allows the charge of negative electricity to pass into the body.

So effective is the excitation, that if three persons hold each other by the hands, and the first rubs the carpet with his foot while the third touches the plate of the electrometer with his finger, a strong charge is communicated to the instrument.

Even approaching the electrometer by the hand or body, it becomes charged by induction at some distance.

A stronger effect is produced on the index of the instrument if, after rubbing the foot against the carpet, it be immediately raised from it. When the two are in contact, the electricities are in some degree coerced or dissimulated; but when they are separated, the whole of the negative electricity becomes free and expands itself in the body. A single stamp on the carpet followed by an immediate removal of the foot causes the index of the electrometer to advance several degrees; and by a reiteration of such stamps the index advances 30° or 40°.

The opposite electrical states of the carpet and the sole of the boot were thus shown: after rubbing, I removed the boot from the carpet, and placed on the latter a proof-plate (i.e. a small disk of metal with an insulating handle), and then transferred it to the plate of the electrometer; strong positive electricity was manifested. Performing the same operation with the sole of the boot, a very small charge was carried, by reason of its ready escape into the body.

The negative charge assumed by sole-leather when rubbed with animal hair was thus rendered evident. I placed on the plate of the electrometer a disk of sole-leather and brushed it lightly with a thick camel's-hair pencil; a negative charge was communicated to the electrometer, which charge was principally one of conduction, on account of the very imperfect insulating-power of the leather.

Various materials, as India-rubber, gutta percha, &c., were sub-

stituted for the sole of the boot; metal plates were also tried: all communicated negative electricity to the body. Woollen stockings are a great impediment to the transmission of electricity from the boot; when these experiments were made I wore cotton ones.

When I substituted for the electrometer a long wire galvanometer, such as is usually employed in physiological experiments, the needle was made to advance several degrees.

At the Meeting of the British Association at Dublin in 1857, Professor Loomis, of New York, attracted great attention by his account of some remarkable electrical phenomena observed in certain houses in that city. It appears that in unusually cold and dry winters, in rooms provided with thick carpets and heated by stoves or hot-air apparatus to 70°, electrical phenomena of great intensity are sometimes produced. A lady walking along a carpeted floor drew a spark one quarter of an inch in length between two metal balls, one attached to a gas-pipe, the other touched by her hand; she also fired ether, ignited a gaslight, charged a Leyden jar, and repelled and attracted pith-balls similarly or dissimilarly electrified. Some of these statements were received with great incredulity at the time both here and abroad; but they have since been abundantly confirmed by the Professor himself and by others. (See Silliman's American Journal of Science, July 1858.)

My experiments show that these phenomena are exceptional only in degree. The striking effects observed by Professor Loomis were feeble unless the thermometer was below the freezing-point, and most energetic when near zero, the thermometer in the room standing at 70°. Those observed by myself succeed in almost any weather when all the necessary conditions are fulfilled. Some of these conditions must frequently be present; and experimentalists cannot be too much on their guard against the occurrence of these abnormal effects. I think I have done a service to them, especially to those engaged in the delicate investigations of animal electricity, by drawing their attention to the subject.

May 19.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communications were read:—

"On the Cause and Theoretic Value of the Resistance of Flexure in Beams." By W. H. Barlow, F.R.S.

The author refers to his previous papers, read in 1855 and 1857, wherein he described experiments showing the existence of an element of strength in beams, which varied with the degree of flexure, and acts in addition to the resistance of tension and compression of the longitudinal fibres. It was pointed out that the ratio of the actual strength of solid rectangular beams to the strength as computed by the theory of Liebniitz is,

In cast iron, as about $2 \frac{1}{4}$ to 1.
In wrought iron as $1 \frac{2}{3}$ and $1 \frac{4}{3}$ to 1.
And in steel, as $1 \frac{2}{3}$ and $1 \frac{4}{3}$ to 1.
The theory of Liebnitz assumes a beam to be composed of longitudinal fibres only, contiguous, but unconnected, and exercising no mutual lateral action. But it is remarked that a beam so constituted would possess no power to resist transverse stress, and would only have the properties of a rope.

Cast iron and steel contain no actual fibre; and wrought iron (although some qualities are fibrous) is able to resist strain nearly equally in any direction.

The idea of fibre is convenient as facilitating investigation; but the word fibre, as applied to a homogeneous elastic solid, must not be understood as meaning filaments of the material. In effect it represents lines of direction, in which the action of forces can be ascertained and measured; for in torsion-shearing and "angular deformation" the fibres are treated by former writers as being at the angle of 45°, because it has been shown that the diagonal resistances have their greatest manifestation at that angle.

Elastic solids being admitted to possess powers of resistance in the direction of the diagonals, attention is called to omission of the effect of resistance in the theory of beams.

The author then states, as the result of his investigation, that compression and extension of the diagonal fibres constitute an element of strength equal to that of the longitudinal fibres, and that flexure is the consequence of the relative extensions and compressions in the direct and diagonal fibres, arising out of the amount, position, and direction of applied forces.

Pursuing the subject, it is shown that certain normal relations subsist between the strains of direct fibres and their relative diagonals, evenly distributed strain being that in which the strain in the direct fibres is accompanied by half the amount of strain in the relative diagonal fibres.

Any disturbance of this relation indicates the presence of another force.

Thus tensile forces applied at right angles to compressive forces of equal amount, produce no strain in the diagonals. But if forces applied at right angles to each other are both tensile, or both compressive, the strain in the diagonal is as great as that in the direct fibres.

It is also pointed out that in a given fibre a b c, the point b may be moved with regard to a and c, thus producing plus and minus strains in the same fibre.

Treating a solid as being made up of a series of laminae, and showing that every change of figure can be represented by the variation in length of the diagonals, taken in connexion with those of the direct fibres, the author proceeds to trace the effects of the application of tensile and compressive forces acting longitudinally on either side of the neutral plane, and shows that curvature is the result of the relation between the strains in direct fibres and those in the diagonals.

The operation of a single tensile force applied along one side of the plate and a transverse stress are likewise traced out, and the conditions of "elastic equilibrium" referred to.

K 2
The amount of resistance offered by the diagonal fibres is shown as follows:

\[ a \ b \ c \ d \] represents a portion of a beam strained by transverse forces into the circular curve \( a \ e \).

Two resistances arise.

1. That due to the extension and compression of the longitudinal fibres produced by the rotation of \( b \ d \) about the neutral axis, which is the resistance considered in the theory of Leibnitz.

2. That due to the extension and compression of the diagonal fibres, caused by the deformation of the square \( a \ b \ c \ d \) into the figure \( a \ h \ o \ c \), which is the resistance of flexure.

It is then shown that, in a solid rectangular beam, the second resistance is equal to the first, and that both resistances act independently, and consequently that the true theoretic resistance of a solid rectangular beam is exactly twice that arrived at by the theory of Leibnitz.

The strength so computed is in general accordance with the results of experiments in cast iron, wrought iron, steel, and other materials, the maximum strength being found in cast iron, which is one-eighth above, and the minimum in glass, which is one-fourth below the calculated strength.

The author considers this treatment of the subject as arising necessarily out of Dr. Hook's law "ut tensio sic vis," and that it is in effect completing the application of those principles which were only partially applied by Leibnitz.

The paper concludes with some practical illustrations (accompanied by photographs) of the effect of diagonal action.

The appendix contains the results of experiments on the tensile, compressive, and transverse resistances of steel.

"On Deep-sea Thermometers." By Staff-Commander John E. Davis, R.N.

The results of thermometric observations at great depths in the ocean not being of a satisfactory nature, the attention of the Hydrographer of the Navy was directed to the defects in the construction of the Six's self-registering thermometers then in use, and also to the want of knowledge of the effects of compression on the bulb;
and as it was known that a delicate thermometer was affected in *vacuo*, it was natural to suppose that an opposite effect would be had by placing them under pressure, and particularly such as they would be subjected to at great depths.

Several thermometers, of a superior construction, were made by different makers, and permission was granted to make experiments by pressure in an hydraulic press; but much delay was caused by not being able to obtain a press suitable to the requirements, until Mr. Casella, the optician, had a testing-apparatus constructed at his own expense, and the experiments were commenced.

Previously to the experiments being made, Dr. W. A. Miller, V.P.R.S., proposed, or rather revived, a mode of protecting the bulb from compression by encasing the full bulb in glass, the space between the case and the bulb being nearly filled with alcohol*.

A wrought-iron bottle had been made to contain a thermometer, for the purpose of comparison with those subjected to compression; but it failed, and finally burst under great compression; it proved, however, of but little consequence, as those designed by Dr. Miller showed so little difference under pressure that they were at once accepted as standards.

Two series of experiments were then most carefully made, at pressures equal to depths of 250, 500, 750, &c. to 2500 fathoms, the results of which satisfactorily proved that the strongest-made unprotected thermometers were liable to considerable error, and therefore that all previous observations made with such instruments were incorrect.

Experiments were also made in the testing-apparatus with Sir Wm. Thomson’s enclosed thermometers, to ascertain the calorific effect produced by the sudden compression of water, in order to find what error, if any, was due to compression in the Miller pattern: an error was proved to exist, but small, amounting to no more than 1°·4 under a pressure of 3 tons to the square inch.

The dredging-cruise of the ‘Porcupine’ afforded an opportunity of comparing the results of the experiments made in the hydraulic testing-apparatus with actual observation in the ocean, and a most careful series of observations were obtained by Staff-Commander E. K. Calver at depths corresponding to the pressure applied in the testing-apparatus; the result was, that although there was a difference in the curves drawn from the two modes of observation, still the general effect was the same, and the means of the two were identical.

From these experiments and observations a scale has been made by which observations made by thermometers of similar construction to those with unprotected bulbs can be corrected and utilized, while it is proposed that by means of observations made with the Miller pattern in the positions and at the same depths at which observations have been made with instruments not now procurable for actual

* Phil. Mag. November 1869.
experiment, to form a scale for correcting all observations made with that particular type.

In conclusion, it is suggested that to avoid error from the unsatisfactory working of the steel indices, which, from mechanical difficulties in their construction, cannot always be depended on, two instruments should be sent down for every observation; and although their occasional disagreement of record may raise a doubt, a little experience will enable the observer to detect the faulty indicator, while their agreement will create confidence.

A description of such deep-sea metallic thermometers as have been invented is appended.


In the course of his researches upon nitro-compounds, the author found it extremely desirable to submit the genetic relations of those bodies to a detailed examination; in other words, to trace the modifications undergone by nitryl as it is transmitted (from the chloride, hydrate, or free radical) through an adequate succession of combinations. One of the first steps in this direction is the preparation of nitryl chloride, which can be most easily effected, according to a statement in Watts's 'Dictionary of Chemistry'*, by the action of phosphoric oxychloride on plumbic nitrate—

\[ 3 \text{Pb}(\text{NO}_3)_2 + 2 \text{POCl}_3 = \text{Pb}_3(\text{PO}_4)_2 + 6 \text{NO}_2\text{Cl}. \]

Among other modes of verifying this equation, the examination of the residue left behind when excess of the oxychloride is heated with plumbic nitrate, and then distilled off in a current of dry air, appeared the most simple and obvious. The results were found not to agree with the equation; and after three nitrates had been tried, a law of chemical activity became evident, rendering the reaction worthy of pursuit for its own sake, although, as an available source of nitryl chloride, it had failed entirely. The nature and mode of establishment of this law constitute the subjects of the author's memoir.

When a nitrate is treated with phosphoric oxychloride, as has just been mentioned, the residue contains phosphoric oxide and a metallic chloride. Within the limits of experimental error, or subject to other satisfactory explanation, the ratio between these two products is constant for each nitrate; and from that ratio a quotient \( \alpha \) can be found as follows:

\[
\frac{\text{weight of chlorine}}{\text{weight of phosphoric oxide}} = \frac{\text{weight of chlorine}}{\text{weight of phosphoric oxide}} \times 4.06.
\]

This quotient, which is different for each nitrate, is termed the "coefficient of chemical activity" of nitrates, and the method of obtaining it is designated the "method of ratios." The data from which

Dr. E. J. Mills on the Chemical Activity of Nitrates. 135

α is deduced, namely, certain weights of argentic chloride and magnesic pyrophosphate, are, if singly considered, new with each experiment; they depend on time, rate of heating, the state of division of the nitrate, and other conditions. But, assuming the results to have been brought about under a law of chemical action, the values of α must be independent of those circumstances, by which the primitive numerator and denominator could have been only pari passu affected; they are related only to the actual occurrence of the reaction. This property, in a chemical ratio, has not, it is believed, been previously observed.

After describing the means employed for obtaining a current of dry air, the apparatus required for the reaction, and the individual experiments which were severally made, the following Table of results is given, Σ being the symbolic value of a nitrate, and $Q = \frac{\Sigma}{\alpha}$.

<table>
<thead>
<tr>
<th>Nitrate</th>
<th>α</th>
<th>Σ</th>
<th>Q</th>
</tr>
</thead>
<tbody>
<tr>
<td>Thallous nitrate</td>
<td>8.76</td>
<td>265.30</td>
<td>30.29</td>
</tr>
<tr>
<td>Argentic nitrate</td>
<td>5.48</td>
<td>169.94</td>
<td>31.01</td>
</tr>
<tr>
<td>Plumbic nitrate</td>
<td>5.17</td>
<td>165.56</td>
<td>32.02</td>
</tr>
<tr>
<td>Rubidic nitrate</td>
<td>2.38</td>
<td>147.40</td>
<td>61.93</td>
</tr>
<tr>
<td>Cæsic nitrate</td>
<td>2.21</td>
<td>195.01</td>
<td>88.24</td>
</tr>
<tr>
<td>Potassic nitrate</td>
<td>1.99</td>
<td>101.14</td>
<td>50.82</td>
</tr>
<tr>
<td>Sodic nitrate</td>
<td>1.70</td>
<td>85.05</td>
<td>50.03</td>
</tr>
<tr>
<td>Lithic nitrate</td>
<td>1.61</td>
<td>69.00</td>
<td>42.86</td>
</tr>
</tbody>
</table>

The above list probably contains all the metallic nitrates that can be completely dried, excepting nitrates derived from amines and amides, which, in the present state of our knowledge of the phosphamides, it was evidently advisable to exclude.

In the silver group, the mean value of Q is 31.11; and the following equation may be accepted therefor:

$$\alpha = \frac{\Sigma}{31.11}.$$  

In the potassium group we have likewise

$$\alpha = \frac{\Sigma}{50.42}.$$  

Hence, within each set of nitrates, chemical activity is in direct proportion to symbolic value. It is further sufficiently apparent that (excepting rubidic nitrate) α and Σ increase and diminish in the same general order. Within the limits of error, the Q column is an incomplete arithmetical series, the most probable value of whose first term is 6.258, so that

$$Q = m \cdot 6.258,$$

m being integral. Reasons are then adduced for identifying the number 6.25 with Dulong and Petit's constant of specific heat. Moreover, since the product of specific heat and symbolic value is,
generally, \( n = 6.25 \), and \( m \) is greater than \( n \), taking \( m = xn \) and \( s = \) the specific heat of a nitrate, we have

\[ Q = xn \cdot 6.25; \]

but

\[ \Sigma s = n \cdot 6.25; \]

\[ \therefore Q = x \Sigma s, \]

and

\[ a = \frac{\Sigma}{Q} = \frac{\Sigma}{x \Sigma s} = \frac{1}{xs}, \]

the expression for chemical activity in terms of specific heat. Comparing the coefficients \((a, a')\) for any two nitrates, the following relations are obtained:

\[ \frac{a}{a'} = \frac{m'}{m} \cdot \frac{\Sigma}{\Sigma'} = \frac{x's'}{xs}; \]

and it is shown that these formulae agree sufficiently well with experiment. Where \( m = m' \) and \( x = x' \), we have the simple expression

\[ \frac{a}{a'} = \frac{\Sigma}{\Sigma'} = \frac{s'}{s}. \]

The values of \( Q \) are strictly equivalent to each other in point of activity. The author believes that \( a \) is commensurate with the elective function of chemical attraction, first discovered by Bergman. He terminates the memoir with a reference to some well-known instances of chemical action (such as that of argentie nitrate on a mixture of aqueous potassic chloride, bromide, and iodide) as serving to bestow a presumptive generality on his principal conclusions.

---

GEOLOGICAL SOCIETY.

[Continued from p. 76.]

January 12th, 1870.—Prof. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—


The author noticed the history of our knowledge of the Dolomitic Conglomerates of the Bristol area from which the remains of Dinosaurian Reptiles have been obtained, and then described their mode of occurrence and distribution over the district near Bristol. He regarded these deposits as due to the action of the sea-waves of the later or Middle Triassic periods upon the rocks of older Triassic (Bunter) or Permian age during the gradual elevation of the land, and as the probable representatives in point of time of the Muschelkalk, otherwise deficient in Britain. The author then noticed the influence of the conglomerate upon the production of certain minerals, such as calamine and hematitic iron-ores, and discussed at some length the probable course of the phenomena of
Mr. J. Prestwich on the Crag of Norfolk and associated Beds. 137

denudation which furnished the materials for the formation of the conglomerate at different levels, in which he recognized two great periods of oscillation, the first witnessing a downward movement of the Palæozoic lands and lasting throughout the deposition of the New Red marl and sands, and the second, during which the accumulations of the former were again, at least partly, denuded. With regard to the time at which the remains of Thecodont Reptiles were imbedded in the conglomerate, the author inferred from the evidence that this took place late in the period of the Keuper.


The author, after describing the general characters of what he termed the Drifts of the Upper and Lower series, and the freshwater gravels of the Lower Avon, comprised within the district of the Avon valley between Tewkesbury and Rugby, and of the Severn valley above and below the town of Worcester, endeavoured to show that there was a balance of evidence in favour of the existence of an upper and lower platform of drift in the main valley of the Lower Avon, the upper one being of marine origin, and probably belonging to the same epoch as the stratified beds of gravel in the neighbourhood of Worcester, which contain marine shells and mammalian remains, whilst the lower one, of freshwater origin, had been derived from the former by fluvial action, as supposed by the late Prof. Strickland. Further, that there was no evidence to warrant the supposition of the existence of high- and low-level river-gravels in those portions of the Severn and Avon valleys under review, and that the apparent absence of any freshwater shells in the gravels of the Severn valley between Bridgnorth and Tewkesbury led to the inference that the freshwater gravels of the Avon were not represented in the adjoining portions of the Severn valley, although remains of some of the same species of Mammalia occurred in both localities. After stating his opinion that the time had not yet arrived for indulging in theoretical speculations concerning the phenomena of the Drifts of the Upper and Lower series exhibited in so small an area as the one under consideration, the author concluded by expressing hopes that the facts which he had brought forward would contribute their share of help to the further elucidation of the question.

By J. M. Wilson, Esq., F.G.S.

January 26th, 1870.—Prof. T. H. Huxley, LL.D., F.R.S.,
President, in the Chair.

The following communication was read:

“On the Crag of Norfolk and associated Beds.” By Joseph
Prestwich, Esq., F.R.S., F.G.S.

The author commenced by referring to his last paper, in which
he divided the Red Crag into two divisions—a lower one, of variable oblique-bedded strata, and an upper one, of sands passing up into the clay known as the Chillesford clay. In 1849 he had alluded to the possibility of this clay being synchronous with the Norwich Crag. He has since traced this upper or Chillesford division of the Red Crag northwards, with a view to determine its relation to the Norwich Crag. He has found it at various places inland; but the best exhibition of it occurs in the Easton-Bavant Cliff. He there found in it a group of shells similar to those at Chillesford, and under it the well-known bed of mammaliferous or Norwich Crag, with the usual shells. The author also showed that in this cliff and the one nearer Lowestoft traces of the Forest-bed clearly set in upon the Chillesford clay. He traced these beds at the base of Horton Cliff, and then passed on to the well-known cliffs of Happisburgh and Mundesley. He considered the Chillesford clay to pass beneath the Elephant bed, and to represent some part of the Forest-bed. The same clay may be traced to near Weybourne. The Crag under these beds he referred to the Chillesford sands. Mention was then made of the sands and shingle above the Chillesford, for which the author proposed the names of "Southwold Sands and Shingle." These, usually, are very unfossiliferous; but at two or three places near Southwold the author found indications of an abundance of shells (Mytilus &c.) and Foraminifera in some iron-sandstones intercalated in this series. In the Norfolk cliffs these beds contain alternating seams of marine and freshwater shells. The inland range of the beds to Aldeby, Norwich, and Coltishall was next traced, and the Chillesford clay shown to be present in each section, and the sands beneath to be referable to the Chillesford sands, as already shown by other geologists, on the evidence of the organic remains. Mr. Gwyn Jeffreys, who had carefully examined the shells of the Norwich Crag for the author, stated that a considerable number of arctic species were found in the Norfolk Crag which did not occur in Suffolk. While, therefore, the Norwich Crag seems to be synchronous with a portion of the Suffolk Crag, that portion is the upper division; and therefore the triple arrangement proposed by Mr. Charlesworth and advocated by Sir C. Lyell, together with the fact of the setting in of a gradually more severe climate, pointed out by the late Dr. Woodward and by Sir C. Lyell, are confirmed.

Mr. Prestwich then referred to the origin of the materials of the Southwold shingle, and showed that, with few exceptions, they came from the south. In it he had found a considerable number of worn fragments of chert and ragstone from the Lower Greensand of Kent. He considered this a convenient base-line for the Quaternary period, as then commenced the spread of the marine gravels over the south of England, and soon after commenced the great denudations which have given the great features to the country.
On the Fossil Corals of the Australian Tertiary Deposits.

February 9th, 1870.—Prof. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—


The author noticed the history of our knowledge of the South-Australian Tertiary Deposits, and indicated the general distribution of the fossiliferous beds from which the corals forming the subject of his communication were derived. These were said to be confined to the region west of Cape Howe, prevailing especially in the province of Victoria, where they had been admirably surveyed by Mr. Selwyn and the officers under him, and to consist chiefly of limestones covered, and in some cases underlain, by great outflows of basalt. The author then gave a list and descriptions of the species (31 in number) of fossil Madreporaria obtained from these South-Australian Tertiary beds, followed by remarks on the characters and relations of the more remarkable forms, and on the localities where they have occurred. From his examination of these fossils, he objected to the application of the divisions adopted in European geology to the deposits in which they are found. He then compared the assemblage of corals obtained from the South-Australian Tertiaries with those found in various deposits elsewhere, or living in the existing seas. The species were stated not to belong to reef-building forms, but to such as now occupy the sea-bottom from low spring-tide mark to the depth where Polyzoa abound. Of these, 20 genera were said to be now represented in the Australian seas; but only three of them to have species in the Tertiaries, viz. the cosmopolite Trochoeyathus, Flabellum, and Amphihelia. The fossil species of these were stated to be quite distinct from those now living in the neighbouring sea. Two species, viz. Flabellum Condeanum and F. distinctum, are living in the Chinese, Japanese, and Red Seas; the author's Plecotrochus elongatus is very nearly allied to the Chinese P. Condeanus; and a Deltoeyathus is regarded by the author as only a varietal form of a living West-Indian and European Miocene species (D. italicus). Three species are common to the Australian and European Cainozoic deposits. Several of the species were said to present curious anomalies of structure, such as so frequently appear in Australian forms, and those of the different beds to exhibit so close a general resemblance, that they offer no evidence of great changes having taken place during the deposition of the whole series of sediments. The evidence afforded by the fossil corals led the author to conclude:—that, at the time of the formation of these deposits, the central area of Australia was occupied by sea, having open water to the north with reefs in the region of Java, and with openings into the Mediterranean and Sahara to the north-west; that Continental India did not form part of a great continent; that the greater part of America was submerged, and the Caribbean sea a coral-area; that the bulk of the land was situated in the north and south; and that
the upheaval of Australia and New Zealand was approximately synchronous with that of the great mountain-chains of the Old World, with the closure of the Panama area, and the depression of the areas on either side of the American continent.

2. "Note on a new and undescribed Wealden Vertebra." By J.W. Hulke, F.R.S., F.G.S.

The author in this note describes a very large Wealden vertebra which he obtained last autumn at Brook, Isle of Wight, remarkable for its great size, its extremely light structure, and the extraordinary development of the processes connected with the neural arch. It consists of a thin outer shell, enclosing a very open cancellated tissue, having extremely large spaces, comparable with those of Pterosauria, and surpassing those of the cancellous tissue in any of the known larger Dinosaurs. A wedge and notch, similar in principle to the ophidian zygosphene and zygantrum, but differently placed, are superadded to the ordinary articular processes. A broad horizontal platform stretches along the side of the arch from the transverse process to the postzygapophysis. The neural spine is composite; all the outstanding parts are supported and strengthened by thin bony plates. Only a small part of the centrum is preserved, so that the form of this, and in particular of its articular faces, is not determinable. The author notices, in conclusion, certain textural resemblances between the vertebra and a peculiar Strepspondylus vertebra in the British Museum, from the Weald of the south-east of England.

3. "Note on the Middle Lias in the North-east of Ireland." By Ralph Tate, Esq., A.L.S., F.G.S.

The author remarked that hitherto no higher member of the Jurassic series than the Lower Lias has been detected in Ireland. He stated that he had received from near Ballintoy some blocks of a grey, marly, micaceous sandstone, containing an assemblage of fossil forms, indicating that the rock from which they were derived belonged to the lowest part of the Middle Lias. The origin of these specimens, which were obtained "from cultivated fields and patches of drift," was said to be still unknown; and the occurrence of *Hipposaurus ponderosum*, associated with Middle-Lias species, as in the Island of Skye, coupled with the agreement in lithological composition between the Irish blocks and the Pabba shales, led him to suggest the possibility that the former may have been transported from the Hebrides by glacial action.

February 23rd, 1870.—Joseph Prestwich, Esq., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "Additional observations on the Neocomian Strata in Yorkshire and Lincolnshire, with notes on their Relations to the Beds of the same age throughout Northern Europe." By J. W. Judd, Esq., F.G.S.

This paper embodied the results of the author's further study of
the Neocomian beds of the north of England, in connexion with those of North-western Germany.

The inland development of these strata in the Vale of Pickering was described as being greatly obscured by superficial deposits. The beds, however, exposed at Reighton, West Heslerton, and Knapton were shown to agree, both in physical and palæontological characters, with several of those before described in the cliff section at Speeton.

The Neocomian ironstones of Lincolnshire have, since the date of the former paper on the subject (1867), been extensively opened out by mining-operations; and the valuable and instructive sections thus afforded were described in detail. A general sketch was then given of the range and characteristics of the Neocomian strata in Yorkshire and Lincolnshire.

Evidence was next adduced to show that strata of the same age, and remarkably similar in character, had been deposited over a very wide area in Northern Europe. Throughout the whole of these districts, however, the Neocomian strata were very inadequately exposed, and afforded no good general sections; and the Speeton Cliff thus acquired an additional interest from the fact that it forms a valuable, and almost the only, key whereby we can correlate the beds over this vast area.

Studying the continental deposits in this manner, by the aid of the Speeton section the fossiliferous clays of the island of Heligoland were shown to belong to the "zone of Ammonites Speetonensis," i.e., the upper part of the Lower Neocomian. The boulders found in the drift deposits of Holland were referred to as evidence of the former wide extension of limestones similar to those of Tealby in Lincolnshire. In Westphalia the sandstones, limestones, ironstones, and clays, so extensively developed in the hills of Bentheim and the Teutoburger Wald, were shown to be of Middle Neocomian age, while certain beds of clay in the same district were referable to the Upper Neocomian. In Hanover the "Hilshton" of M. Fr. Ad. Römer was shown to be in its upper part Upper Neocomian, and in its lower part Middle Neocomian, the latter passing locally into beds of oolitic ironstone, sandstones, and limestones precisely similar to those of the same age in Lincolnshire. The narrow strip of highly inclined Neocomian strata along the northern foot of the Hartz was shown to belong to the same two subdivisions. In Brunswick, however, the Neocomian series was more complete; for underneath some 400 feet of clays, which on palæontological evidence clearly belong to the Upper and Middle divisions, there were certain marly limestones, in places becoming ferruginous, containing an abundant and interesting fauna which was most unmistakably that of the Lower Neocomian.

It was then pointed out that in northern Germany there was evidence, as in this country, of an unconformity existing between the Upper Cretaceous and the Neocomian, as well as between this last and the Jurassic. Attention was also drawn to the fact that while the Neocomian series was complete in Yorkshire and Brunswick, its lowest member was absent in the intermediate districts, being
apparently replaced by the freshwater deposits of the German Wealden.


XVIII. Intelligence and Miscellaneous Articles.

ON THE USE OF THE ELECTRIC CURRENT IN CALORIMETRY.

BY M. J. JAMIN.

Joule's law gives the heat which is developed in conductors when traversed by currents. A metal wire may be regarded as a focus. It may have any possible form and be placed where we please, in the midst of liquids or gases; a quantity of heat will be given off proportional to the time, to its resistance, and to the square of the intensity of the current; it will heat those bodies by a quantity which can be measured, and which is inversely proportional to their mass and to their specific heat. Hence results a new process to determine this specific heat. After numerous trials I fixed upon the following arrangements.

I. Case of Solids and of Liquids.—In dealing with a solid or a liquid, I use as a calorimeter an elongated cylindrical vessel of thin copper, on which is coiled 8 metres of German-silver wire 0.2 millim. in diameter, and covered with silk. This spiral commences at the bottom of the vessel, and ascends to one-third of its height; it is connected with the circuit by thick copper wires; its resistance is measured for all the temperatures of the experiment. I envelope it with a thin silk ribbon to keep it in its place, some swan's down to insulate it, and I enclose the whole in an envelope of thin copper polished. When the calorimeter contains a liquid and a current is caused to pass through the spiral, nearly all the heat will be transmitted to the sides, then to the liquid; a scarcely appreciable portion will be transmitted to the swan's down.

With this view, fresh liquid must be continually brought into contact with the sides by uniform agitation. For this purpose a basket of metal gauze, formed of two concentric tubes, is immersed in the calorimeter. A small machine raises and lowers it at equal intervals; a thermometer marking the hundredth of a degree is immersed in the central tube; it is fixed, and is read with a telescope. When the specific heat of solids is to be measured, they are placed in the basket in the water.

This constitutes the entire apparatus ; the operation is one of extreme simplicity. After pouring into the calorimeter the weight of liquid which is to be investigated and agitating it some time, the variation (if any) of the thermometer is observed for five minutes. Generally it does not vary. A current of a measured intensity is then made to pass during one, two, &c. minutes, until an elevation
of 3 or 4 degrees is produced; this is noted, after which the cooling of the thermometer is observed during five minutes. The quantity of heat given off is known, the effect it has produced is calculated, and from the known formulae of calorimetry the desired capacity is deduced.

The old method required two operations, which were:—the first, to heat in a stove for a long time the body to be studied, and to pour it with minute precautions into the calorimeter; the second, to observe the thermometer immersed in the calorimeter. In the method which I propose the first operation is omitted, and the second suffices such as it was before. The corrections remain the same, but are simplified.

They are simplified because a lower temperature is sufficient, and because, the heat given off being proportional to the time, the method known as Rumford's is applicable. We may even dispense with all correction, as I shall show.

I provided the external envelope of the apparatus with a spiral twenty times as long as the first, and immersed the whole into a vessel containing twenty times as much liquid as the calorimeter, and forming a medium in which the latter is immersed. The current passes simultaneously into the two spirals; it produces there heats proportional to the quantities of liquid, and consequently equal heatings. At each moment the temperatures of the calorimeter and its surroundings are in equilibrium, and the first, neither gaining nor losing anything by radiation, is subject only to the action of the current. It is impossible to maintain this equilibrium strictly during the whole time of the experiments if they are prolonged; but it is very easy to establish it within a few tenths; and that is sufficient to obviate all necessity for correction. Thus we can measure for each degree the specific heat of a liquid (water or alcohol for example) from the lowest temperatures to its boiling-point.

I have verified this method by determining the capacities of iron and of copper, which are the most difficult to obtain exactly, because they are very small. I found 0'098, 0'093. M. Regnault obtained the numbers 0'113, 0'095, which are a little larger; but he operated with a higher temperature.

II. Of Gases and Vapours.—The advantages of this method are especially apparent when treating of aeriform fluids. A gaseous current passes through a glass tube to the middle of a cork of badly conducting material; a thermometer there measures its temperature. It immediately enters a second tube through the folds of a metal spiral or a bundle of twisted wires traversed by electricity—that is to say, through a focus; it becomes heated and meets a second thermometer, which measures its increase of temperature. Before

* Suppose two experiments to be made with the same current during the same time, with the weights P and P' of water and of the liquid to be studied. The quantities of heat are the same; they have heated the liquids \( \theta \) and \( \theta' \) degrees. Denoting the weight of the calorimeter reduced to water by \( \pi \), and the capacity sought by \( x \), we have

\[
(P + \pi)\theta = (P'x + \pi)\theta'.
\]
emerging, the gas is led round the first tube to prevent any loss by radiation and conductibility; and when the temperature has become stationary, we may say that all the heat of the focus, which is known, is taken by the gas, the temperature of which is increased by a measured quantity; hence the specific heat can be deduced.

There are two advantages in this method. The first is, that the greatest cause of error which Delarocche and Béard, and afterwards M. Regnault, met with is suppressed. In their experiments the gas reached 100° in a calorimeter at 10°; and the greatest difficulty was felt in appreciating the heat which passes by conductibility from the hot tube to the cold calorimeter. In my method the gas reaches at the ordinary temperature (say, 10°), it passes from the spiral at about 20°: the difference is 10°; it was 90° before; the present error is at most one-ninth of the former.

Here is the second improvement. The whole of my apparatus is the size of a finger, it is of thin glass; it might be of mica, even of goldbeater's skin; it weighs no more than a litre of gas, and expends no more heat in reaching the final temperature. Ten litres of gas are sufficient to make one measurement; thus the difficulties which for a long time had to be overcome in order to obtain a uniform current disappear, ordinary gasometers suffice, and the method is applicable even to vapours. A first determination gave the number 0.242 for air, instead of 0.237, which M. Regnault found.

Thermometers, even, may be dispensed with, and the temperature measured by the increase of resistance in the wires. It is known that a resistance r at zero becomes \( r(1 + at) \) at \( t \) degrees. That being the case, let two equal bundles of wires be placed one after the other in a tube; then, having decomposed the total circuit into two equal derived circuits, let us make each of them pass, first through one of the two bundles of wires, then into a differential galvanometer; the latter remains at zero. But if a current of gas at \( t \) degrees be sent through this tube, it will pass at \( t + \theta \) in the first spiral, at \( t + 2\theta \) in the second; they take a difference of temperature \( \theta \), a different resistance, and the galvanometer is deflected. It is reduced to zero on introducing, by means of a special rheostat, a platinum wire into one of the circuits. The length of this wire is proportional to the increase of temperature \( \theta \); it admits of measurement.

The same apparatus is applicable to vapours. The liquid to be examined is distilled as regularly as possible; the current of vapour is at first superheated by the first bundle of wires, it afterwards traverses the second, becomes heated by a quantity \( \theta \), which is measured as before; the vapour is condensed, and afterwards weighed. In order to take into account the irregularities of the distillation, it is necessary to observe the apparatus from minute to minute.

III. Latent Heat.—In order to measure latent heats, a double alembic is employed, of which one part is exterior; the liquid in it is caused to boil, and the vapour is brought there after having been condensed by a refrigerator: the effect of this is simply to raise to the boiling temperature the interior alembic, which contains the same liquid, and in which is immersed the spiral, the resistance
of which is known for every temperature. The vapour which forms in the second apparatus is collected during ten minutes, before the passing of the current; there is scarcely any; the circuit is then closed, which determines a rapid boiling. The heat supplied is known; the vapour which it has formed without change of temperature is weighed, and the latent heat is deduced.

IV. The two Specific Heats.—A third application of the same principle can be made. In a large bell-glass filled with air a metal wire is stretched; an intense current is passed for a short time through it, which develops a determined quantity of heat; a fraction of this disappears by radiation; the remainder, which is constant, gives heat to the gas, which can be measured in two ways—either by increase of the volume at constant pressure, or by increase of the pressure at constant volume. From these two effects the ratio of the two specific heats can easily be deduced; and the number found is about 1·42, a number indicated by the velocity of sound.

These experiments are now in full operation. I wished by this communication to make my own the general method which will be applicable to all questions of calorimetry. I have associated with me in this work four distinguished pupils of the Laboratoire de Recherches de la Sorbonne, MM. Richard, Amaury, Champagneur, and A. Thenard. We shall presently publish the results of our work.

—Comptes Rendus, March 28, 1870.

---

ON THE FIXED NOTES CHARACTERISTIC OF THE VARIOUS VOWELS.

BY M. R. KENIG.

According to the researches of MM. Donders and Helmholtz, the mouth, arranged for the emission of a vowel, has a note of stronger resonance, which is fixed for each vowel, whatever may be the fundamental note on which it is given. A slight change in the pronunciation modifies the vocal notes so sensibly that M. Helmholtz has been able to propose to linguists to define by these notes the vowels belonging to the different idioms and dialects. Hence it is of great interest to know exactly the pitch of these notes for the different vowels. M. Donders sought to arrive at this by observing the rustling or whistling which the current of air produces in the mouth when the different vowels are whispered; the notes which he has found differ considerably from those given by M. Helmholtz. The latter used a set of tuning-forks, which he made to vibrate in front of the mouth when it was arranged to articulate a vowel. Every time the sound was strengthened by the air enclosed in the cavity of the mouth, this mass of air was evidently in unison with the tuning-fork. By this method, which is more correct than the first, M. Helmholtz found that the vowel A was characterized by the fixed note (si♭), O by (si♭), E by (si♭); and these results really appear incontestable. As none of the tuning-forks arranged was sufficiently acute for the vowel I, M. Helmholtz tried to determine the characteristic note by the means already employed.

Intelligence and Miscellaneous Articles.

by M. Donders, and he found it to be re₉. If a tuning-fork be tuned for this note, we ascertain, in fact, that it is increased whilst the mouth passes from E to I; at least I have been able to assure myself that the increase occurs before the mouth is exactly arranged for the I. Hence the true characteristic of I must be higher. By constructing tuning-forks more and more acute, I ascertained that this note was approached; it was finally found to be sib₉; with tuning-forks still higher, it is immediately felt that the limit has been passed.

For OU M. Donders had given fa₃. This note can undoubtedly be strengthened by the mouth, but it is only in departing very little from the position O; and one feels that the note of OU must be much more grave. M. Helmholtz assigns fa₃ to OU. However, a tuning-fork fa₃ does not resound before the mouth arranged for OU, which M. Helmholtz accounts for by the smallness of the opening of the mouth; but it seemed to me that this smallness of the opening, while rendering a very energetic increase impossible, must still admit an appreciable increase in the intensity of the sound. Having moreover ascertained the simple ratios which exist between the notes of the vowels O, A, E, I, ascending by octaves, I thought that this law would extend to the vowel OU. I verified this hypothesis circumstantially by means of a tuning-fork, the pitch of which could be raised by means of slides; I was thus able to assure myself that the characteristic note of OU (such as I ordinarily pronounce it) was really (sib)₃; for the maximum of resonance always occurred between 440 and 460 simple vibrations.

For the pronunciation of the Germans of the North (to which the experiments of M. Helmholtz also refer), the vowels are then characterized as follows:—

OU O A E I
(sib)₃ (sib)₄ (sib)₅ (sib)₆

or, in round numbers of simple vibrations, 450, 900, 1800, 3600, 7200.

It seems to me more than probable that we must seek, in the simplicity of these ratios, the physiological cause which makes us find nearly always the same five vowels in the different languages, although the human voice can produce an indefinite number, as the simple ratios between the numbers of vibrations explain the existence of the same musical intervals among most nations.

It is some time since I obtained these results; but I wished to have them verified by several eminent physiologists, whose approbation has encouraged me to publish them.—Comptes Rendus, April 25, 1870.

COMPRESSIONIBILITY OF GASES UNDER HIGH PRESSURES.

BY M. L. CAILLETTET.

In order to obtain very high pressures applicable to the experiments in which I am engaged, after numerous trials I fixed upon an apparatus which consisted of a hollow steel cylinder firmly
Intelligence and Miscellaneous Articles.

fastened on a cast-iron stand. A piston, also of steel, is moved in this cylinder by a square-threaded screw, which works in a female screw of bronze, wedged in the axis of a fly, also of cast iron. When this fly is turned by means of the pegs on its circumference (as the screw cannot follow it in its rotation, owing to a catch secured by two slide bars), the piston traverses the vacuum of the cylinder in a direction determined by the direction of the motion of the fly. The water which the cylinder contains cannot escape; a leather is fitted so perfectly that, even under pressures of more than 800 atmospheres, scarcely a drop of liquid escapes. To the cylinder in which the compression is effected a steel laboratory-tube can be united by a capillary tube of copper—which, leaving that part of the apparatus quite free, allows the majority of the experiments to be made here. The pressure is estimated by two mutually controlling processes: (1) by a lever which rests on a very moveable valve; (2) by a Degove's modified manometer, which I will briefly describe.

This instrument consists of a cylindrical cast-iron vessel, filled with mercury, upon which rests a metallic disk. A thin membrane of caoutchouc separates the disk from the mercury, which consequently cannot escape. A metal rod penetrates to the centre of the disk, passing through a leather fixed in a bronze cylinder connected with the pressure-machine. When the compressed water acts on the small piston, the pressure is transmitted to the mercury, which rises in a vertical glass tube, communicating with the reservoir.

If the ratio of the surface of the small piston to that of the disk is \(1:100\), then for a pressure of 100 atmospheres the mercury will only rise in the manometric tube 1 atmosphere, or 0.76 metre.

A grave à priori objection might be made to this apparatus; in fact, it is not known what resistance the leather exercises on the piston. In the apparatus employed by me, the ratio of the surfaces is \(1:212\), and it is sufficient to lower the piston \(\frac{1}{3}\) of a millimetre in order to raise the mercury 4.30 metres, the height of my manometric tube. The path traversed being very small, the resistance will be nearly none. To overcome the inertia, the mercury is caused to oscillate about its position of equilibrium in the glass tube by means of a small lever, which acts on the compressing disk. The manometer thus constructed has been verified up to 80 atmospheres by the help of a very large manometer, in which the compressed air was replaced by hydrogen. The graduation was based on the numbers published by M. Regnault. The apparatus for pressure, such as I have just described it, easily gives pressures from 8 to 900 atmospheres, which can be maintained for a considerable time. Danger from the bursting of any part of the machine, there is almost none: steel tubes filled with liquid have frequently split without any of their parts being projected.

In an experiment, in which I subjected to about 850 atmospheres pressure 60 cubic centimetres of hydrogen, the laboratory-tube was broken, the compressed gas suddenly expanded and exploded with L 2
the sound of a pistol-shot; but the splinters of broken glass were not thrown about, owing to the metal cover.

In order to investigate Mariotte's law under high pressures, I employ a cylindrical glass tube capable of containing 40 to 50 cubic centims. of gas; a capillary glass tube is welded to this reservoir, in which the compressed gases will be measured. The other extremity of the reservoir is open and tapered. This apparatus is filled with the gas to be examined pure and dry, the extremity of the capillary tube is welded, and to the lower point a kind of small inverted gauge filled with mercury is fitted, which admits of the apparatus being placed in the laboratory-tube filled with mercury. At the moment when the pressure is exerted by the machine, the mercury, pressed by the water, will penetrate into the reservoir through the tapered part, will drive back the gases in the capillary tube, and will just stop at a point of its height. In order to determine this point exactly (which cannot be done during the experiment, because the apparatus is enclosed in the steel tube), I had recourse to an artifice which gives extremely correct results.

With this object I slightly gilded the interior of the capillary tube by M. Böttger's process. The mercury, rising in contact with the sides, dissolves the gold; and the height of the bright metal corresponds exactly with the height attained by the mercury. This is noted on a coat of varnish applied to the surface of the glass. It can be understood how a great number of heights, corresponding to the volumes occupied by the gas at pressures determined by the manometer, may thus be found.

The correctness of the determinations which I have obtained depend especially (1) on the marking of the heights attained by the mercury in the capillary tube, (2) on the weights of this mercury, (3) on the correctness of the manometer. I have assured myself by numerous experiments that the volume of the mercury could be obtained very correctly; the weight taken has always been the mean of four operations. I have already discussed the correctness of the manometer; I have moreover compressed at the same time, in the same tube, two different gases. I thus proved that the volumes occupied by the two gases under identical pressures corresponded well to the numbers found in my experiments. The numbers obtained have not undergone the correction due to the compressibility of the glass apparatus; I did not know this contraction; I made all my determinations for the different gases under the same pressures, in such a manner that, if a cause of error not recognized should vitiate my results by the same quantity, the experiments, made under identical conditions, will still remain comparable.

As M. Regnault has done in his memorable researches on the compressibility of gases, I have calculated the departures from Mariotte's law by employing the formula $\frac{VP}{VT}$; the numbers thus obtained were taken as lengths of the ordinates for the construction of the curves, which cannot be given here:
60 .................. 0·9810             1·0131
80 .................. ...             1·0118
90 .................. ...             1·0106
100 .................. 0·9552           1·0098
125 .................. 0·9442           1·0062
150 .................. 0·9372           1·0047
175 .................. ...             1·0027
200 .................. 0·9158           0·9990
225 .................. 0·9078           0·9862
250 .................. 0·9001           0·9792
275 .................. ...             0·9599
300 .................. 0·8761           0·9465
325 .................. 0·8670           0·9320
350 .................. 0·8537           0·9047
375 .................. ...             0·8929
400 .................. 0·8347           0·8672
450 .................. 0·8136           0·8265
500 .................. 0·7893           0·7927
550 .................. 0·7701           0·7502
600 .................. 0·7580           0·7215
660 .................. ...             0·6895
705 .................. ...             0·6660

The above results were obtained by operating on 43·638 cubic centims. at +15°.

It appears, according to these numbers, that Mariotte’s law is not verified for high pressures; each gas in contracting seems to follow a special course. Hydrogen decreases regularly; air, on the contrary, very curiously, reaches a maximum at 80 atmospheres, and afterwards decreases more rapidly than hydrogen.

In presenting these still very incomplete experiments to the Academy, I simply wished to record them, reserving to myself the time necessary for their execution.

I am at present occupied in pursuing my determinations with much higher pressures, and extending them to other gases.—Comptes Rendus, May 23, 1870.

NOTE TO MR. MOON’S PAPER ON THE SOLUTION OF LINEAR PARTIAL DIFFERENTIAL EQUATIONS OF THE SECOND ORDER, IN THE PHILOSOPHICAL MAGAZINE FOR JULY.

I desire to point out that when the coefficient U in the equation

\[ 0 = Rr + Ss + Tt + Pp + Qq + Uz \]

is finite, the assumptions

\[ c=1, \quad c_1 = c_2 = \&c. = 0 \]

materially cripple the generality of the result; as a glance at the mode in which each of the coefficients \( A_1, A_2, \&c. \) is formed from its immediate predecessor will readily show.

July 23, 1870.
ON THE RAPIDITY OF THE ABSORPTION OF CARBONIC OXIDE BY THE LUNGS. BY M. N. GRÉHANT.

In the researches which I made in 1864 on the renewal of air in the lungs of man, I proved that in a man the volume of whose lungs is equal to 2.93 litres after an inspiration, and to half a litre of air after an expiration, 100 cub. centims. of gaseous mixture, taken at any point of the air-vessels, have received 11 cub. centims. of pure air.

From this measurement, obtained by experiment, I drew this conclusion—that if a man be placed in an atmosphere containing poisonous gas, from the first inspiration this gas is distributed throughout all the air-vessels, to be given up to absorption by the blood.

To establish more completely this consequence, and to study the successive phases of poisoning by the medium of the lungs, I made several experiments in the physiological laboratory at the Museum of Natural History, under the direction of my illustrious teacher, M. Claude Bernard. As a poisonous gas I used carbonic oxide; and I chose this gas for several reasons. M. Claude Bernard was the first to make out that carbonic oxide kills animals because it fixes itself on the red blood-globules and displaces the oxygen combined with these globules, that in an animal which succumbs to poisoning by carbonic oxide the arterial blood contains much less oxygen than the normal arterial blood and the globules are combined with a large proportion of carbonic oxide.

We know that the crystalline combination of carbonic oxide with hæmoglobin has been investigated and isolated by M. Hoppe-Seyler, and that the spectroscope supplies a qualitative test for this combination of oxygen with hæmoglobin.

But in the research which I have undertaken I had another object. I proposed to determine quantitatively the proportion of carbonic oxide combined with the red globules at the different periods of poisoning: this is why I have employed, in order to extract the carbonic oxide from blood, the following method, which offers every certitude.

After having extracted the gases from some normal blood in a vacuum at 40°, by means of a mercury pump, a volume of sulphuric acid double that of the blood was introduced into the extraction-apparatus, the bath was heated to 100°, and boiling was maintained for half an hour; under these conditions we still obtained carbonic acid, a trace of oxygen, and a little nitrogen, but no trace of carbonic oxide. But if the blood of an animal poisoned with carbonic oxide is operated on in the same manner, the vacuum alone at 40° gives carbonic acid, oxygen, and nitrogen, but no trace of carbonic oxide; whilst sulphuric acid at 100° in the vacuum destroys the globules, and completely drives away the carbonic oxide combined with the hæmoglobin.

To verify the correctness of this method, I caused blood to absorb a
known volume of carbonic oxide, and by the action of sulphuric acid at 100° I disengaged the same volume of gas.

I must here make an important remark. If, instead of heating to 100° the mixture of blood and sulphuric acid in an empty globe communicating with the mercury pump, this mixture is heated in a retort furnished with a delivery-tube, the temperature increases, and then under the ordinary pressure a very considerable volume of carbonic oxide is furnished by the decomposition of the albuminous matter and of the haemoglobine; hence this more simple method must be completely rejected.

Having thus established a method by which the disengagement of the carbonic oxide combined with haemoglobine in the poisoned blood is effected, I have been able to study the first phases of poisoning.

In a large bell-glass provided with a tubulure, a mixture of 9 litres of air and 1 litre of pure carbonic oxide was made; the tubulure of the bell-glass is closed by a three-way stopcock, which I used in measuring the volume of the lungs. The carotid artery of a dog was exposed, and a glass tube inserted which is joined to a caoutchouc tube closed by a pinch-cock; a muzzle well fitted to the head of the animal is united by a caoutchouc tube to the stopcock of the bell. The animal at first breathes the air; at the commencement of a minute noted on a seconds' watch, I open the stopcock of the bell; the animal immediately inhales the poisonous gas; between the 55th and 80th second after the commencement, I collect in a syringe, fixed in the vent-pipe of the carotid, 50 cubic centimetres of arterial blood, which is immediately injected into the apparatus for extracting the gas; the gases of the blood are extracted at 40°; then, by sulphuric acid at 100°, the carbonic oxide is extracted. The following are the results which have been furnished by the poisoned blood, and also those given by a sample of normal blood of the carotid submitted to exactly the same processes.

Dry gases at zero and under a pressure of 760 millimetres.

<table>
<thead>
<tr>
<th>Carbonic acid</th>
<th>Nitrogen</th>
<th>Oxygen</th>
<th>Carbonic oxide</th>
</tr>
</thead>
<tbody>
<tr>
<td>100 cubic centimetres of poisoned arterial blood</td>
<td>42·4</td>
<td>1·7</td>
<td>6·4</td>
</tr>
<tr>
<td>100 cubic centimetres of normal arterial blood</td>
<td>37·6</td>
<td>1·7</td>
<td>16·6</td>
</tr>
</tbody>
</table>

I repeated this experiment on another dog, but after having arranged two apparatus for the extraction of the gases from the blood, in which an absolute vacuum had first been made. The animal was connected in the same manner with the bell containing the mixture rendered poisonous by \( \frac{1}{10} \) of carbonic oxide; but some arterial blood was collected twice; the first was taken from the 10th to the 25th second, the second from the 75th to the 90th
second; then the animal was restored by air; afterwards the gases were extracted simultaneously.

\[
\begin{array}{|c|c|c|c|}
\hline
\text{Carbonic acid.} & \text{Nitrogen.} & \text{Oxygen.} & \text{Carbonic oxide.} \\
\hline
100 \text{ cubic centimetres of the arterial blood first taken} & 40.5 & 1.57 & 14.65 & 4.28 \\
100 \text{ cubic centimetres of the arterial blood afterwards taken} & 44.3 & 2.78 & 4.01 & 18.41 \\
\hline
\end{array}
\]

Other experiments, made under the same conditions, give analogous results. Hence we see that in an animal breathing air containing \(\frac{1}{10}\) of carbonic oxide (a mixture highly poisonous), the arterial blood, between the 10th and 25th second, contains already 4 per cent. of carbonic oxide and less oxygen than the normal blood (14.6 per cent.); and between 1 minute 15 seconds and 1 minute 30 seconds, 18.4 per cent. and oxygen in very diminished quantity (4 per cent.). At that time the animal was in great danger; and had the experiment lasted a minute longer, it would have died.

These indisputable results are directly applicable to man; and we may assert that if a man enters a highly deleterious atmosphere, the gaseous poison is dissolved in the arterial blood from the first minute, and brought into contact with the anatomical elements, which kills him.

Every day we have too numerous examples of sudden death happening to workmen whose occupations compel them to expose themselves to deleterious gases or vapours, either in descending wells or entering the galleries of mines, the air of which is poisoned, or more or less deprived of oxygen. But physiologists have certainly given advice which might in future free the life of man from all such dangers; and this advice ought to be carried out by law.

Before descending a well, ditch, or gallery, of which the air has not been renewed for a long time, the workman ought to lower a cage containing a bird or a small mammal, as a rat or a guinea-pig: if the animal, left in the confined atmosphere for ten to fifteen minutes, can stand this test, a man may descend without fear; if the animal succumb, ventilation must be energetically established, until another animal can resist a new proof.

The employment of an animal in this manner would preserve man from accidents too frequently mortal, as the Davy lamp, in coal-mines, has saved the lives of so many miners.—*Comptes Rendus*, May 30, 1870.
On the Cause of the Motion of Glaciers.
By James Croll, of the Geological Survey of Scotland.*

The generally accepted theory proved by the Rev. Canon Moseley to be incorrect.

Since the time that Professor Tyndall had shown that all the phenomena formerly attributed by Professor Forbes to plasticity could be explained upon the principle of regelation, discovered by Faraday, the viscous theory of glacier-motion has been pretty generally given up. The ice of a glacier is now almost universally believed to be, not a soft plastic substance, but a substance hard, brittle, and unyielding. The power that the glacier has of accommodating itself to the inequalities of its bed without losing its apparent continuity is referred to the property of regelation possessed by ice. All this is now plain; but what is it that impels the glacier forward is still a question under discussion. Various theories have been propounded regarding the cause of the descent of glaciers, all of which have been abandoned with the exception of that which attributes their descent to gravitation. But as the ice of the glacier descends with a differential motion, we have not only to explain what causes the glacier to slide on its bed, but also what displaces the particles of the ice over one another and alongside one another. What, then, is the force which shears the ice? The answer generally given is that gravitation alone is the force which does this; or, in other words, the mere weight of the ice is sufficient to overcome its cohesive force and to displace the particles over one another. The

* Communicated by the Author.

Rev. Canon Moseley has lately investigated this point, and has found that the amount of work performed on a glacier (assuming, of course, that the ice shears in the solid state) during its descent through a given space is enormously greater than the work of the weight of the glacier descending through that space. He has determined the amount of work performed by gravitation in the descent of a glacier, and the amount of internal work performed on the ice during the descent; and has found that, in respect to a glacier of the same uniform rectangular section and slope as the Mer de Glace at Les Ponts, and moving with the same uniform velocity, the aggregate work of the resistances which oppose themselves to its descent in a given time is about thirty-four times the work of the weight in the same time; consequently it is physically impossible that the mere weight alone of the glacier can be the cause of its descent.

The impression left on my mind after reading Canon Moseley’s memoir in the Proceedings of the Royal Society for January 1869 was that, unless some very serious error could be pointed out in the mathematical part of his investigation, it would be hopeless to attempt to overturn his general conclusion as regards the received theory of the cause of the descent of glaciers, by searching for errors in the experimental data on which the conclusion rests. Had the result been that the actual shearing-force of ice is by twice, thrice, four times, or even five times too great to allow of a glacier shearing by its own weight, one might then hope that, by some more accurate method of determining the unit of shear than that adopted by Canon Moseley, his objection to the received theory of glacier-motion might be met; but when the unit of shear is found to be not simply by three times, four times, or even five times, but actually by thirty, forty, or fifty times too great, all our hopes of overturning his conclusion by searching for errors in this direction vanish, even although there are some points connected with his unit of shear that are not very satisfactory.

The ice of a glacier is in the hard, solid, and crystalline state. This is now generally admitted. Then, if the particles of the ice shear in this state, Canon Moseley’s calculations show that the glacier cannot possibly descend by its weight only, as is generally supposed; and the generally received theory of glacier-motion must therefore be abandoned. I can perceive no way of escape from this conclusion.

I presume that few who have given much thought to the subject of glacier-motion have not had some slight misgivings in regard to the commonly received theory. There are some facts which I never could harmonize with this theory. For example, boulder-clay is a far looser substance than ice; its
shearing-force must be very much less than that of ice; yet immense masses of boulder-clay will lie immoveable for ages on the slope of a hill so steep that one can hardly venture to climb it, while a glacier will come crawling down a valley which by the eye we could hardly detect to be actually off the level. Again, a glacier moves faster during the day than during the night, and about twice as fast during summer as during winter. Professor Forbes, for example, found that the Glacier des Bos near its lower extremity moved sometimes in December only 11·5 inches daily, while during the month of July its rate of motion sometimes reached 52·1 inches per day. Why such a difference in the rate of motion between day and night, summer and winter? The glacier is not heavier during the day than it is during the night, or during the summer than it is during the winter; neither is the shearing-force of the great mass of the ice of a glacier sensibly less during the day than during the night, or during the summer than during the winter; for the temperature of the great mass of the ice does not sensibly vary with the seasons. Then, if this is the case, gravitation ought to be as able to move a glacier during the night as during the day, or during the winter as during the summer. At any rate, if there should be any difference it ought to be but trifling. It is true that, owing to the melting of the ice, the crevices of the glacier are more gorged with water during summer than during winter; and this, as Professor Forbes maintains*, may tend to make the glacier move faster during the former season than during the latter. But the advocates of the regelation theory cannot conclude, with Professor Forbes, that the water favours the motion of the glacier by making the ice more soft and plastic. The melting of the ice, according to the regelation theory, cannot very materially aid the motion of the glacier.

The fact that the rate of motion of a glacier depends upon the amount of heat that the ice is receiving shows that heat in some way or other stands related as a cause to the motion of the glacier.

But the point under consideration is, If the ice of a glacier shears in the solid state, as is generally supposed, has Canon Moseley proved that a glacier cannot descend by its weight only? I have carefully read the interesting memoirs by Mr. Mathews and Mr. Ball in reply to Canon Moseley; and although I agree with the most of their remarks regarding the unsatisfactory nature of Mr. Moseley's own theory of glacier-motion, yet I am unable to perceive that any thing which they have advanced materially affects his general conclusion as regards the commonly received theory. If the ice of a glacier shears, nothing which I have yet

* Occasional Papers, pp. 166, 223.
seen advanced to the contrary can, so far as I perceive, overturn Mr. Moseley's conclusion, that the glacier cannot descend by its weight only. The interesting experiment described by Mr. Mathews*, of a plank of ice supported horizontally at each end being deflected in the middle without any weight being applied to the ice, does not appear to me to prove anything either in favour of the generally received theory or against Canon Moseley's conclusion,—for this very simple reason, that whatever theory we may adopt as to the cause of the motion of glaciers, the deflection of the plank in the way described by Mr. Mathews follows as a necessary consequence. Although no weight was placed upon the plank, it does not necessarily follow that the deflection was caused by the weight of the ice alone; for, according to Canon Moseley's own theory of the motion of glaciers by heat, the plank ought to be deflected in the middle, just as it was in Mr. Mathews's experiment. A solid body, when exposed to variations of temperature, will expand and contract transversely as well as longitudinally. Ice, according to Canon Moseley's theory, expands and contracts by heat. Then if the plank expands transversely, the upper half of the plank must rise and the lower half descend. But the side which rises has to perform work against gravity, whereas the side which descends has work performed upon it by gravity; consequently more of the plank will descend than rise, and this will, of course, tend to lower or deflect the plank in the middle. Again, when the plank contracts, the lower half will rise and the upper half will descend; but as gravitation, in this case also, favours the descending part and opposes the rising part, more of the plank will descend than rise, and consequently the plank will be lowered in the middle by contraction as well as by expansion. Thus, as the plank changes its temperature, it must, according to Mr. Moseley's theory, descend or be deflected in the middle, step by step—and this not by gravitation alone, but chiefly by the motive power of heat. I do not, of course, mean to assert that the descent of the plank was thus actually caused by heat; but I assert that Mr. Mathews's experiment does not necessarily prove (and this is all that is required in the mean time) that gravitation alone was the cause of the deflection of the plank. Neither does this experiment prove that the ice was deflected without shearing; for although the weight of the plank was not sufficient to shear the ice, as Mr. Mathews, I presume, admits, yet Mr. Moseley would reply that the weight of the ice, assisted by the motive power of heat, was perfectly sufficient.

Had Mr. Mathews laid his plank horizontally across an inclined plane and fixed the two ends of the plank so as to prevent them

* Alpine Journal for February 1870; 'Nature' for March 24, 1870.
moving, everybody (whatever might be his theory as to the cause of the motion of glaciers) would at once admit that the middle of the plank (which, of course, was not fixed) would begin slowly to descend the incline in the manner that the ice of a glacier actually does, and that the plank, not being permitted to move at its ends, would become bent or deflected in the middle. Then, if everybody would admit that the plank would be deflected in the middle notwithstanding the friction of the ice on the inclined plane, and the diminished pressure of the weight of the ice in consequence of its resting on the slope, surely no one could conclude that, were the inclined plane removed and the plank suspended in the air by its two extremities, as in Mr. Mathews’s experiment, it would not descend in the middle.

I shall now briefly refer to Mr. Ball’s principal objections to Canon Moseley’s proof that a glacier cannot shear by its weight alone. One of his chief objections is that Mr. Moseley has assumed the ice to be homogeneous in structure, and that pressures and tensions acting within it are not modified by the varying constitution of the mass. Although there is, no doubt, some force in this objection (for we have probably good reason to believe that ice will shear, for example, more easily along certain planes than along others), still I can hardly think that Canon Moseley’s main conclusion can ever be materially affected by this objection. The main question is this, Can the ice of the glacier shear by its own weight in the way generally supposed? Now the shearing-force of ice, take it in whatever direction we may, so enormously exceeds that required by Mr. Moseley in order to allow a glacier to descend by its weight only, that it is a matter of indifference whether ice be regarded as homogeneous in structure or not. Mr. Ball objects also to Mr. Moseley’s imaginary glacier lying on an even slope and in a uniform rectangular channel. Surely Mr. Ball does not suppose that a glacier would descend more easily in an irregular and broken channel having a variable slope and direction than it would do in a straight channel uniform in width and slope. And if he does not, why advance such an objection? Canon Moseley assumed, as he had a perfect right to do, that if the glacier could not descend by its weight in his imaginary channel, it could much less do so in its actual one.

That a relative displacement of the particles of the ice is involved in the motion of a glacier, is admitted, of course, by Mr. Ball; but he states that the amount of this displacement is but small, and that it is effected with extreme slowness. This may be the case; but if the weight of the ice be not able to overcome the mutual cohesion of the particles, then the weight of the ice cannot produce the required displacement, however small it may be. Mr. Ball then objects to Mr. Moseley’s method of determin-
ing the unit of shear on this ground:—The shearing of the ice in a glacier is effected with extreme slowness; but the shearing in Canon Moseley’s experiment was effected with rapidity; and although it required 75 lbs. to shear one square inch of surface in his experiment, it does not follow that 75 lbs. would be required to shear the ice if done in the slow manner in which it is effected in the glacier. “In short,” says Mr. Ball, “to ascertain the resistance opposed to very slow changes in the relative positions of the particles, so slight as to be insensible at short distances, Mr. Moseley measures the resistance opposed to rapid disruption between contiguous portions of the same substance.”

There is force in this objection; and here we arrive at a really weak point in Canon Moseley’s reasoning. His experiments show that if we want to shear ice quickly a weight of nearly 120 lbs. is required; but if the thing is to be done more slowly, 75 lbs. will suffice*. In short, the number of pounds required to shear the ice depends to a large extent on the length of time that the weight is allowed to act; the longer it is allowed to act, the less will be the weight required to perform the work. “I am curious to know,” says Mr. Mathews when referring to this point, “what weight would have sheared the ice if a day had been allowed for its operation.” I do not know what would have been the weight required to shear the ice in Mr. Moseley’s experiments had a day been allowed; but I feel pretty confident that, should the ice remain unmelted, and sufficient time be allowed, shearing would be produced without the application of any weight whatever. There are no weights placed upon a glacier to make it move, and yet the ice of the glacier shears. If the shearing is effected by weight, the only weight applied is the weight of the ice; and if the weight of the ice makes the ice shear in the glacier, why may it not do the same thing in the experiment? Whatever may be the cause which displaces the particles of the ice in a glacier, they, as a matter of fact, are displaced without any weight being applied beyond that of the ice itself; and if so, why may not the particles of the ice in the experiment be also displaced without the application of weights? Allow the ice of the glacier to take its own time and its own way, and the particles will move over each other without the aid of external weights, whatever may be the cause of this; well, then, allow the ice in the experiment to take its own time and its own way, and it will probably do the same thing. There is something here unsatisfactory. If, by the unit of shear, be meant the pressure in pounds that must be applied to the ice to break the connexion of one square inch of two surfaces frozen together and

* Philosophical Magazine for January 1870, p. 8; Proceedings of the Royal Society for January 1869.
cause the one to slip over the other, then the amount of pressure required to do this will depend upon the time you allow for the thing being done. If the thing is to be done rapidly, as in some of Mr. Moseley's experiments, it will take, as he has shown, a pressure of about 120 lbs.; but if the thing has to be done more slowly, as in some other of his experiments, 75 lbs. will suffice. And if sufficient time be allowed, as in the case of glaciers, the thing may be done without any weight whatever being applied to the ice, and, of course, Mr. Moseley's argument, that a glacier cannot descend by its weight alone, falls to the ground. But if, by the unit of shear, be meant not the weight or pressure necessary to shear the ice, but the amount of work required to shear a square inch of surface in a given time or at a given rate, then he might be able to show that in the case of a glacier (say the Mer de Glace) the work of all the resistances which are opposed to its descent at the rate at which it is descending is greater than the work of its weight, and that consequently there must be some cause, in addition to the weight, urging the glacier forward. But then he would have no right to affirm that the glacier would not descend by its weight only; all that he could affirm would simply be that it could not descend by its weight alone at the rate at which it is descending.

Mr. Moseley's unit of shear, however, is not the amount of work performed in shearing a square inch of ice in a given time, but the amount of weight or pressure requiring to be applied to the ice to shear a square inch. But this amount of pressure depends upon the length of time that the pressure is applied. Here lies the difficulty in determining what amount of pressure is to be taken as the real unit. And here also lies the radical defect in Canon Moseley's result. Time as well as pressure enters as an element into the process. The key to the explanation of this curious circumstance will, I think, be found in the fact to which reference has already been made, viz. that the rate at which a glacier descends depends in some way or other upon the amount of heat that the ice is receiving. This fact shows that heat has something to do in the shearing of the ice of the glacier. But in the communication of heat to the ice time necessarily enters as an element. There are two different ways in which heat may be conceived to aid in shearing the ice: (1) we may conceive that heat acts as a force along with gravitation in producing displacement of the particles of the ice; or (2) we may conceive that heat does not act as a force in pushing the particles over each other, but that it assists the shearing processes by diminishing the cohesion of the particles of the ice, and thus allowing gravitation to produce displacement. The former is the function attributed to heat in Canon Moseley's theory of glacier-
motion; the latter is the function attributed to heat in the theory of glacier-motion which I ventured to advance some time ago*. It results, therefore, from Canon Moseley's own theory, that the longer the time that is allowed for the pressure to shear the ice, the less will be the pressure required; for, according to his theory, a very large proportion of the displacement is produced by the motive power of heat entering the ice; and, as it follows of course, other things being equal, the longer the time during which the heat is allowed to act, the greater will be the proportional amount of displacement produced by the heat; consequently the less will require to be done by the weight applied. In the case of the glacier, Mr. Moseley concludes that at least thirty or forty times as much work is done by the motive power of heat in the way of shearing the ice as is done by mere pressure or weight. Then, if sufficient time be allowed, why may not far more be done by heat in shearing the ice in his experiment than by the weight applied? In this case how is he to know how much of the shearing is effected by the heat and how much by the weight. If the greater part of the shearing of the ice in the case of a glacier is produced, not by pressure, but by the heat which necessarily enters the ice, it would be inconceivable that in his experiments the heat entering the ice should not produce, at least to some extent, a similar effect. And if a portion of the displacement of the particles is produced by heat, then the weight which is applied cannot be regarded as the measure of the force employed in the displacement, any more than it could be inferred that the weight of the glacier is the measure of the force employed in the shearing of it. If the weight is not the entire force employed in shearing, but only a part of the force, then the weight cannot, as in Mr. Moseley's experiment, be taken as the measure of the force.

How, then, are we to determine what is the amount of force required to shear ice? in other words, how is the unit of shear to be determined? If we are to measure the unit of shear by the weight required to produce displacement of the particles of the ice, we must make sure that the displacement is wholly effected by the weight. We must be certain that heat does not enter as an element in the process. But if time be allowed to elapse during the experiment, we can never be certain that heat has not been at work. It is impossible to prevent heat entering the ice. We may keep the ice at a constant temperature, but this would not prevent heat from entering the ice and producing molecular work. True that, according to Moseley's theory of glacier-motion, if the temperature of the ice be not permitted to vary, then no displacement of the particles can take place from

* Philosophical Magazine for March 1869.
the influence of heat; but according to the molecular theory of glacier-motion which I have adopted, heat will aid the displacement of the particles whether the temperature be kept constant or not. In short, it is absolutely impossible in our experiments to be certain that heat is not in some way or other concerned in the displacement of the particles of the ice. But we can shorten the time, and thus make sure that the amount of heat entering the ice during the experiments is too small to affect materially the result. We cannot in this case say that all the displacement has been effected by the weight applied to the ice, but we can say that so little has been done by heat that, practically, we may regard it as all done by the weight.

This consideration, I trust, shows that the unit of shear adopted by Canon Moseley in his calculations is not too large. For if in half an hour, after all the work that may have been done by heat, a pressure of 75 lbs. is still required to displace the particles of one square inch, it is perfectly evident that if no work had been done by heat during that time, the force required to produce the displacement could not have been less than 75 lbs. It might have been more than that; but it could not have been less. Be this, however, as it may, in determining the unit of shear we cannot be permitted to prolong the experiment for any considerable length of time, because the weight under which the ice might then shear could not be taken as the measure of the force which is required to shear ice. By prolonging the experiment we might possibly get a unit smaller than that required by Canon Moseley for a glacier to descend by its own weight. But it would be just as much begging the whole question at issue, to assume that, because the ice sheared under such a weight, a glacier might descend by its weight alone, as it would be to assume that, because a glacier shears without a weight being placed upon it, the glacier descends by its weight alone.

But why not determine the unit of shear of ice in the same way as we would the unit of shear of any other solid substance, such as iron, stone, or wood? If the shearing-force of ice be determined in this manner, it will be found to be by far too great to allow of the ice shearing by its weight alone. We shall be obliged to admit either that the ice of the glacier does not shear (in the ordinary sense of the term), or if it does shear, that there must, as Canon Moseley concludes, be some other force in addition to the weight of the ice urging the glacier forward.

Physical objections to the Rev. Canon Moseley’s own theory.

Although Canon Moseley has thus so ably and so successfully shown the insufficiency of the generally received theory of the cause of the descent of glaciers, he has, however, I venture to think,
not been so fortunate in his attempt to establish a theory of his own. And I cannot help thinking that the influence which his remarkable communication to the Royal Society, on the impossibility of the descent of glaciers by their weight alone, would have had on the minds of physicists, has been much impaired by the prominence which he has since been giving to a theory which few, I fear, will ever be able to accept. Whatever may be the fate which awaits the generally accepted theory of the cause of glacier-motion, his own theory seems to be beset by difficulties of a physical nature which will require to be removed before he can expect that it will be received by physicists in general.

Most of these difficulties have already been noticed and discussed by Professor Forbes, Mr. Mathews, Mr. Ball, and others. I shall therefore only briefly allude to a few of those that more particularly bear on some points which have not already been sufficiently discussed.

Canon Moseley has shown that the mere weight of the ice is wholly insufficient to overcome the cohesion of the crystalline particles, so as to break their connexion and cause them to be displaced one over the other. This point I regard as fully established. It is implied in the generally received theory, that, in the descent of a glacier, owing to differential motion the cohesion of the particles of the ice is broken, and that these solid particles are forced over one another and alongside one another. Mr. Moseley then concludes that it follows, as a necessary consequence, that there must be some other force, in addition to the weight of the ice, pushing the glacier forward. Here lies the fundamental error. He has not proved that in the descent of the glacier the connexion of the solid particles of the ice has to be broken. True, the ice moves with a differential motion, and, as a necessary consequence, the particles are displaced over each other. Two particles separate, and the one moves past the other; but the point to be determined is this:—were the two particles at the moment when separation took place both in the hard crystalline and solid state? Canon Moseley does not prove this; he merely assumes it to be the case; but it must be proved to be the case, not assumed to be so, before he can conclude that it necessarily follows that in the descent of the glacier some force in addition to the weight of the ice is required to push the glacier forward. Certainly he is warranted in concluding that it necessarily follows that the generally received theory is incorrect, because in this theory it is assumed that the particles shear in the solid state. He would be warranted in saying to those who believe in the generally received theory, "You assume with me that in the descent of a glacier the cohesion of the solid particles of the ice has to be overcome and the one particle forced past the other. Then
you must be wrong when you assert that the glacier descends by its weight only; for, as I have demonstrated, the mere weight of the glacier alone is not sufficient to do this." Canon Moseley has not, however, proved that the glacier cannot absolutely descend by its weight alone; he has only proved that if the glacier shears in the way that it is generally supposed to do, it cannot descend by its weight alone. Had it been established that the ice of the glacier shears in the way that it is generally supposed to do, Mr. Moseley's results would leave us no other alternative than to conclude that there must actually be some other cause in addition to the weight of the glacier impelling it forward; and we should be obliged to seek in heat or in something else for this additional impelling power.

I presume that Canon Moseley has not duly considered this point, and that consequently he has been led to the conclusion that, if his late remarkable results be received (which no doubt they will ere long), we shall then be obliged to adopt his own theory of glacier-motion, or some other similar theory which calls in the aid of forces more powerful than that of gravitation to impel the glacier downwards. That he supposes that we are forced to this alternative is, I think, apparent from the way in which he has lately introduced his theory. "The ice of a glacier," he says, "behaves itself in its descent exactly as the lead did in my experiment. The Mer de Glace moves faster by day than by night. Its mean daily motion is twice as great during the six summer as during the six winter months. The connexion between its rate of motion and the external temperature is most remarkable. It has been carefully observed, and the results, as recorded by Professor Forbes, leave no doubt of the fact, that no change of external mean temperature is unaccompanied by a corresponding change of glacier-motion. From this it follows that the two are either dependent on some common cause, or that the one set of changes stands in the relation of a cause to the other. That both sets of phenomena (the changes of the sun's heat and the changes of glacier-motion) should be due to some common independent cause seems impossible. We are forced, therefore, on the conclusion that one is caused by the other. And as the changes in the glacier-motion cannot cause the changes of solar heat, it must be the changes of solar heat which cause the changes of glacier-motion\(^*\).

It is certainly true that the fact that the glacier moves more rapidly during the day than during the night, and during summer than during winter, proves that there must be some physical connexion between the heat of the sun and the motion of the

glacier. It is also true that the changes of the sun’s heat and the changes of glacier-motion cannot be due to a common cause. And it is admitted that the changes in the glacier-motion must in some way or other be dependent upon the changes in the sun’s heat. Further, it is admitted that the changes in the sun’s heat are the cause of the changes in glacier-motion; but it entirely depends upon the meaning which we attach to the term “cause,” whether it will be admitted that the sun’s heat is the cause of the motion of the glacier. If by cause of the motion of the glacier be meant every thing without which the glacier would not descend, then it is admitted that heat is a cause of the motion of the glacier. But if by cause of the motion of the glacier be meant the energy or power that impels the glacier forward (and this is the meaning which Mr. Moseley seems to attach to the term), then we are not compelled logically to admit that heat is the cause of the motion of the glacier; for it may only be a necessary condition to the operation of the cause, whatever that cause may be, which impels the glacier forward. The absence of a necessary condition will as effectually prevent the occurrence of an effect as the absence of the cause itself. It does not follow that, because a glacier will not move without heat, heat is necessarily the cause of its motion. Gravitation may be the cause, and heat only a condition.

The fundamental condition in Mr. Moseley’s theory of the descent of solid bodies on an incline is, not that heat should maintain these bodies at a high temperature, but that the temperature should vary. The rate of descent is proportionate, not simply to the amount of heat received, but to the extent and frequency of the variations of temperature. As a proof that glaciers are subjected to great variations of temperature, he adduces the following:—“All alpine travellers,” he says, “from De Saussure to Forbes and Tyndall, have borne testimony to the intensity of the solar radiation on the surfaces of glaciers. ‘I scarcely ever,’ says Forbes, ‘remember to have found the sun more piercing than at the Jardin.’ This heat passes abruptly into a state of intense cold when any part of the glacier falls into shadow by an alteration of the position of the sun, or even by the passing over it of a cloud.”

Mr. Moseley is here narrating simply what the traveller feels, and not what the glacier experiences. The traveller is subjected to great variations of temperature; but there is no proof from this that the glacier experiences any changes of temperature. It is rather because the temperature of the glacier is not affected by the sun’s heat that the traveller is so much chilled when the

sun's rays are cut off. The sun shines down with piercing rays and the traveller is scorched; the glacier melts on the surface, but it still remains "cold as ice." The sun passes behind a cloud or disappears behind a neighbouring hill; the scorching rays are then withdrawn, and the traveller is now subjected to radiation on every side from surfaces at the freezing-point.

It is also a necessary condition in Mr. Moseley's theory that the heat should pass easily into and out of the glacier; for unless this were the case sudden changes of temperature could produce little or no effect on the great mass of the glacier. How, then, is it possible that during the heat of summer the temperature of the glacier could vary much? During that season, in the lower valleys at least, every thing, with the exception of the glacier, is above the freezing-point; consequently when the glacier goes into the shade there is nothing to lower the ice below the freezing-point; and as the sun's rays do not raise the temperature of the ice above the freezing-point, the temperature of the glacier must therefore remain unaltered during that season. It therefore follows that, instead of a glacier moving more rapidly during the middle of summer than during the middle of winter, it should, according to Moseley's theory, have no motion whatever during summer.

The following, written fifteen years ago by Professor Forbes on this very point, is most conclusive:—"But how stands the fact? Mr. Moseley quotes from De Saussure the following daily ranges of the temperature of the air in the month of July at the Col du Géant and at Chamouni, between which points the glacier lies:

At the Col du Géant . . . 4°257 Reaumur.
At Chamouni . . . . 10°092 "

And he assumes 'the same mean daily variation of temperature to obtain throughout the length' [and depth?] 'of the Glacier du Géant which De Saussure observed in July at the Col du Géant.' But between what limits does the temperature of the air oscillate? We find, by referring to the third volume of De Saussure's Travels, that the mean temperature of the coldest hour (4 A.M.) during his stay at the Col du Géant was 33°03 Fahrenheit, and of the warmest (2 P.M.) 42°61 F. So that even upon that exposed ridge, between 2000 and 3000 feet above where the glacier can be properly said to commence, the air does not, on an average of the month of July, reach the freezing-point at any hour of the night. Consequently the range of temperature attributed to the glacier is between limits absolutely incapable of effecting the expansion of the ice in the smallest degree"*.

Again, during winter, as Mr. Ball remarks, the glacier is completely covered with snow and thus protected both from the influence of cold and of heat, so that there can be nothing either to raise the temperature of the ice above the freezing-point, or to bring it below that point; and consequently the glacier ought to remain immoveable during that season also.

"There can be no doubt, therefore," Mr. Moseley states, "that the rays of the sun, which in those alpine regions are of such remarkable intensity, find their way into the depths of the glacier. They are a power, and there is no such thing as the loss of power. The mechanical work which is their equivalent, and into which they are converted when received into the substance of a solid body, accumulates and stores itself up in the ice under the form of what we call elastic force or tendency to dilate, until it becomes sufficient to produce actual dilatation of the ice in the direction in which the resistance is weakest, and by its withdrawal to produce contraction. From this expansion and contraction follows of necessity the descent of the glacier."*

When the temperature of the ice is below the freezing-point, the rays which are absorbed will, no doubt, produce dilatation; but during summer, when the ice is not below the freezing-point, no dilatation can possibly take place. All physicists, so far as I am aware, agree that the rays that are then absorbed go to melt the ice and not to expand it. But to this Mr. Moseley replies as follows:—"To this there is the obvious answer that radiant heat does find its way into ice as a matter of common observation, and that it does not melt it except at its surface. Blocks of ice may be seen in the windows of ice-shops with the sun shining full upon them, and melting nowhere but on their surfaces. And the experiment of the ice-lens shows that heat may stream through ice in abundance (of which a portion is necessarily stopped in the passage) without melting it, except on its surface." But what evidence has Mr. Moseley to conclude that if there is no melting of the ice in the interior of the lens there is a portion of the rays "necessarily stopped" in the interior? It will not do to assume a point so much opposed to all that we know of the physical properties of ice as this really is. Has Mr. Moseley, after accurately determining the amount of work performed in melting the ice of his lens during any given time, found it to fall short of the amount of work which ought to have been performed by the heat absorbed during that given time? If he has done this in a manner that can be relied upon, then he has some warrant to conclude that there is a portion of the rays stopped which goes to perform work different from that of melting

the ice, and that this work in all probability is the expansion of
the ice. Or has he determined directly that his lens, after reach-
ing the temperature which is considered to be the melting-point
of ice, actually continued to expand as the rays passed into it?
It is absolutely essential to Mr. Moseley’s theory of the motion
of glaciers, during summer at least, that ice should continue to
expand after it reaches the melting-point; and it is therefore in-
cumbent upon him to afford us some evidence that such is the
case; or he need not wonder that we cannot accept his theory,
because it demands of us the adoption of a conclusion so con-
trary to all our previous conceptions. But, as a matter of fact,
it is not strictly true that when rays pass through a piece of ice
there is no melting of the ice in the interior. Experiments made
by Professor Tyndall show the contrary.*

There is, however, one fortunate circumstance connected with
Canon Moseley’s theory. It is this; its truth can be easily
tested by direct experiment. The ice, according to this theory,
descends not simply in virtue of heat, but in virtue of change of
temperature. Try, then, Hopkins’s famous experiment, but keep
the ice at a constant temperature; then, according to Moseley’s
theory, the ice will not descend. Or try Mr. Mathews’s experi-
ment, but keep the ice-plank at a constant temperature, and the
plank ought not to sink in the middle. But let it be observed
that although the ice under this condition should descend (as
there is little doubt but it would), it would show that Mr. Mose-
ley’s theory of the descent of glaciers is incorrect, but it would
not in the least degree affect the conclusions which he has lately
arrived at in regard to the generally received theory of glacier-
motion. It would not prove that the ice sheared, in the way
generally supposed, by its weight only. It might be the heat,
after all, entering the ice, which accounted for its descent, although
gravitation (the weight of the ice) might be the impelling cause.

The present state of the question.

The condition which the perplexing question of the cause of
the descent of glaciers has now reached seems to be something
like the following. The ice of a glacier is not in a soft and
plastic state, but is solid, hard, brittle, and unyielding. It ne-
evertheless behaves in some respects in a manner very like what
a soft and plastic substance would do if placed in similar cir-
cumstances, inasmuch as it accommodates itself to all the inequa-

ties of the channel in which it moves. The ice of the glacier,
though hard and solid, moves with a differential motion; the
particles of the ice are displaced over each other, or, in other
words, the ice shears as it descends. It had been concluded that

* See Philosophical Transactions, December 1857.
the mere weight of the glacier was sufficient to shear the ice. Canon Moseley has investigated this point, and shown that it is not. He has found that for a glacier to shear in the way that it is supposed to do, it would require a force some thirty or forty times as great as the weight of the glacier. Consequently, for the glacier to descend, a force in addition to that of gravitation is required. What, then, is this force? It is found that the rate at which the glacier descends depends upon the amount of heat which it is receiving. This shows that the motion of the glacier is in some way or other dependent upon heat. Is heat, then, the force we are in search of? The answer to this, of course, is, since heat is a force necessarily required, we have no right to assume any other till we see whether or not heat will suffice. In what way, then, does heat aid gravitation in the descent of the glacier? In what way does heat assist gravitation in the shearing of the ice? There are two ways whereby we may conceive the thing to be done: the heat may assist gravitation to shear, by pressing the ice forward, or it may assist gravitation by diminishing the cohesion of the particles, and thus allowing gravitation to produce motion which it otherwise could not produce. Every attempt which has yet been made to explain how heat can act as a force in pushing the ice forward, has failed. The fact that heat cannot expand the ice of the glacier may be regarded as a sufficient proof that it does not act as a force impelling the glacier forward; and we are thus obliged to turn our attention to the other conception, viz. that heat assists gravitation to shear the ice, not by direct pressure, but by diminishing the cohesive force of the particles, so as to enable gravitation to push the one past the other. But how is this done? Does heat diminish the cohesion by acting as an expansive force in separating the particles? Heat cannot do this, because it cannot expand the ice of a glacier; and besides, were it to do this, it would destroy the solid and firm character of the ice, and the ice of the glacier would not then, as a mass, possess the great amount of shearing-force which observation and experiment show that it does. In short it is because the particles of the ice are so firmly fixed together at the time that the glacier is descending, that we are obliged to call in the aid of some other force in addition to the weight of the glacier to shear the ice. Heat does not cause displacement of the particles by making the ice soft and plastic; for we know that the ice of the glacier is not soft and plastic, but hard and brittle. The shearing-force of the ice of the moving glacier is found to be by at least from thirty to forty times too great to permit of the ice being sheared by the mere force of gravitation; how, then, is it that gravitation, without the direct assistance of any other force, can manage to shear the ice? Or to put the question
under another form: heat does not reduce the shearing-force of the ice of a glacier to something like 1.3193 lb. per square inch of surface, the unit required by Mr. Moseley to enable a glacier to shear by its weight; the shearing-force of the ice, notwithstanding all the heat received, still remains at about 75 lbs.; how, then, can the glacier shear without any other force than its own weight pushing it forward? This is the fundamental question; and the true answer to it must reveal the mystery of glacier-motion. We are compelled in the present state of the problem to admit that glaciers do descend with a differential motion without any other force than their own weight pushing them forward; and yet the shearing-force of the ice is actually found to be thirty or forty times the maximum that would permit of the glacier shearing by its weight only. The explanation of this apparent paradox will remove all our difficulties in reference to the cause of the descent of glaciers.

There seems to be but one explanation (and it is a very obvious one), viz. that the motion of the glacier is molecular. The ice descends molecule by molecule. The ice of a glacier is in the hard crystalline state, but it does not descend in this state. Gravitation is a constantly acting force; if a particle of the ice lose its shearing-force, though but for the moment, it will descend by its weight alone. But a particle of the ice will lose its shearing-force for a moment if the particle loses its crystalline state for the moment. The passage of heat through ice, whether by conduction or by radiation, in all probability is a molecular process; that is, the form of energy termed heat is transmitted from molecule to molecule of the ice. A particle takes the energy from its neighbour A on the one side and hands it over to its neighbour B on the opposite side. But the particle must be in a different state at the moment it is in possession of the energy from what it was before it received it from A, and from what it will be after it has handed it over to B. Before it became possessed of the energy, it was in the crystalline state—it was ice; and after it loses possession of the energy it will be ice; but at the moment that it is in possession of the passing energy is it in the crystalline or icy state? If we assume that it is not, but that in becoming possessed of the energy, it loses its crystalline form and for the moment becomes water, all our difficulties regarding the cause of the motion of glaciers are removed*. We know that the ice of a glacier in the mass cannot become possessed of energy in the form of heat without becoming fluid; may not the same thing hold true of the ice particle?

* See Phil. Mag. for March 1869, p. 201.
The alleged limit to the thickness of a glacier.

In his memoir "On the Mechanical Properties of Ice," published in the Philosophical Magazine for January 1870, Canon Moseley arrives at a conclusion in regard to the crushing of ice to which I am unable, without some qualifications, to agree. In his experiments ice was crushed under a pressure of 308.4 lbs. on the square inch, and he concludes that if a glacier is over 710 feet in thickness the ice at the under surface must be crushed by the incumbent weight. Professor Phillips also made some experiments on the crushing of ice, and he came to the conclusion that the height of a crushing column of ice is between 1000 and 1500 feet, and concluded also that if a glacier were to exceed this in thickness the ice would lose its solidity*. Whether the height of a crushing column of ice be 710, or 1000, or 1500 feet is of no consequence whatever as regards the possible thickness of a glacier. No doubt a piece of ice solidified not under pressure would be crushed to powder were it placed under a glacier 1000 feet in thickness or so; but after being crushed it would resolidify, and would then probably be able to sustain a pressure of 2000 feet of ice. This follows as a necessary consequence from the property of regelation. There is as yet, so far as I am aware, no known limit to the amount of pressure which ice may sustain. There probably is a limit; but what that limit is has not yet been determined. Canon Moseley says that "there is no glacier alleged to have so great a depth as 710 feet." The Humboldt glacier in North Greenland, according to Dr. Kane, has a depth of more than three times 710 feet. And Dr. Heyes found in Baffin's Bay icebergs (which are just pieces broken off the ends of glaciers) aground in about half a mile of water. And on the antarctic continent we have reasons for believing that the ice is in some places over a mile in thickness†.

XX. On the Molecular Movements and Magnetic Changes in Iron &c. at different Temperatures. By G. Gore, F.R.S.†

W. FOX§ has shown that cast iron in the melted state produces little or no magnetic effect upon a delicately poised magnetic needle placed near it during its cooling, solidification, and subsequent further cooling, until the solid metal acquires "a cherry-red colour;" it then suddenly attracts the needle with great energy. Gilbert had also many years before

* Paper on Glacial Striation read before the Geological Section of the British Association, 1865.
† Geological Magazine for June 1870, p. 276.
‡ Communicated by the Author.
shown that a piece of soft iron at a bright red heat loses all ordinary signs of magnetism; and Faraday has shown* that wrought iron retains only traces of its ordinary magnetic capacity at that temperature. He has further shown† that nickel first loses its distinctive magnetic power at about 635°F. (=335°C.), and that the temperature of boiling oil is sufficient to render large masses of that metal insensible to the action of common magnets—also that on raising the temperature of iron and nickel from 0°F. to 300°F. (=−17°C. to 149°C.) the magnetic capacity of iron remains constant, whilst that of nickel diminishes gradually. According to Matteucci‡, the magnetism of iron increases up to a certain temperature, then decreases rapidly; it retains, however, even at a white heat, a very minute degree of its magnetic capacity, calculated at only '000015 of its ordinary amount; for a globule melted in a lime spoon was still attracted by a very powerful electromagnet.

In a paper "On a Momentary Molecular Change in Iron Wire," published in the Proceedings of the Royal Society, No. 108, p. 260, January 28, 1869, I described a singular phenomenon which I observed in the cooling of iron wire which had been heated to full redness whilst under a suitable degree of longitudinal tension by means of a spring attached to one of its ends; the iron during cooling, and whilst still red-hot, gradually diminished in length, then suddenly elongated by diminution of cohesion, and finally contracted gradually nearly to its original length during the remainder of the cooling process: a corresponding but reverse phenomenon did not occur during the process of heating the wire. Various other metals were similarly examined, but no such peculiar phenomenon was found. In another paper, "On the Development of Electric Currents by Magnetism and Heat"§, I showed that, by cooling a brightly red-hot iron wire (under the influence of a permanent magnet) within the axis of a coil of thin insulated copper wire, an electric current was induced in that wire; and during the first few seconds in the process of cooling, and at apparently the same temperature at which the aforesaid elongation and loss of cohesion occurs, an irregular action took place in the induced current, which "was probably connected with the momentary molecular change." In the present paper I have employed a different method of examining these molecular movements and magnetic changes of iron and nickel under the influence of heat, and have obtained some

* Experimental Researches in Electricity, 2344–2347.
‡ Bibl. Univ. de Genève, [Arch. des Sc. xxiii.] xxiv.
further results showing the existence of additional molecular changes.

I took a well-annealed and straight bar of wrought iron 32 inches (=81.3 centims.) long and \( \frac{3}{8} \) of an inch (=9.5 millims.) in diameter, supported it in a horizontal position by two wooden clips, placed one end of the bar in a coil of thin insulated copper wire connected with a distant galvanometer, and the other end within a coil of thick insulated copper wire for attachment to a voltaic battery; the battery consisted of ten large Smee's cells. A row of five Bunsen's burners was placed beneath the middle part of the bar for the purpose of heating it to redness; and the bar at that part was between notched plates of firebrick to increase the heat. With the whole of the bar at 60° F. (=15° C.), on connecting the thick coil with the battery a deflection of 14° or 16° was obtained of the needles of the galvanometer; but with the middle of the bar at a red heat a deflection of only 4° could be obtained. In a similar experiment with a bar 2 feet (=61 centims.) long and \( \frac{1}{2} \) an inch (=12.7 millims.) thick, the same battery, and more suitable coils of wire, a deflection of 20° or 25° was obtained with the middle of the bar red-hot, and a powerful swing of 90° with the whole of the bar cold, the needles striking strongly against the stops of the galvanometer. With the bar of iron entirely absent, no perceptible electrodynamic induction took place. Similar but much less powerful results were obtained on substituting a permanent bar magnet for the battery and thick wire coil. A red heat, therefore, in the middle part of a wrought-iron bar largely diminishes, but does not entirely prevent the transmission of magnetism along the bar.

The number of molecular movements and magnetic changes which occur in wrought iron by change of temperature at a moderate red heat are quite remarkable, and were gradually revealed by the following experiments:—An iron bar 32 inches (=81.3 centims.) long and \( \frac{3}{8} \) of an inch (=9.5 millims.) thick, in a horizontal position and diagonal to the magnetic meridian, was surrounded at one end by a coil of thin insulated copper wire connected with a distant galvanometer. On gradually heating the middle part only of the bar to redness, when the temperature acquired a low red heat a slight sudden deflection of the needles occurred; and on gradually cooling the bar a similar slight and sudden deflection in an opposite direction took place, apparently at the same temperature. The directions of the induced current during the heating agreed with what would be produced by a decrease of magnetism, and during the cooling with an increase of magnetism. With a bar 2 feet (=61 centims.) long and \( \frac{1}{2} \) an inch (=12.7 millims.) thick, and the north pole of a small permanent bar magnet lying in contact with one end of the bar, the
other end of the bar being enclosed within a coil 5 inches (≈12.7 centims.) long, containing 20 layers or 2674 turns of "No. 27" (≈0.25 millim. thick) insulated copper wire, connected with the galvanometer, a slight deflection only of the needles was produced during the process of heating, and a quick motion of the needles 3° in the opposite direction during the cooling. The directions of movement agreed with the previous ones, and showed that the bar suddenly increased in magnetic capacity during cooling at a particular temperature of moderate red heat, and apparently at the same temperature at which it undergoes the anomalous diminution of cohesion and increase in length already referred to. With the same bar and fine-wire coil, and a coil 5¼ inches (≈13.3 centims.) long containing 8 layers or 464 turns of "No. 16" (≈1.5 millim. thick) insulated copper wire on the opposite end connected with the battery of ten large Smee's elements, on gradually applying the heat the following effects took place:—When the bar became red-hot in the middle, a small and irregular deflection of the needles of 1½° occurred, and the needles then returned steadily to zero, and no other deflection took place as the bar became somewhat hotter. The gas-flames being now suddenly extinguished, in less than half a minute the needles moved slowly 1° in the opposite direction, then stopped, and then went rapidly 18½° further and quickly back again, swinging nearly equally on each side of zero, and soon settled near that point, and remained there during the whole of the remainder of the cooling process. The directions of the currents agreed with those previously obtained.

In the next experiment an iron bar 2 feet (≈61 centims.) long and ¼ of an inch (≈19 millims.) thick was employed, with the same battery, the thick-wire coil being 6 inches (≈15.2 centims.) long and containing 536 turns of "No. 17" (≈1.5 millim. thick) copper wire, and the other coil being 6 inches long and containing 1960 turns of "No. 27" (≈0.25 millim. thick) copper wire. The order of procedure was the same as in the last experiment. As soon as the bar had become red-hot, several minute molecular changes and increases of magnetism in succession in the bar took place as the bar became hotter, indicated by corresponding small deflections of the needles. The bar being larger than the previous one, it did not become quite so highly heated; therefore on stopping the gas the needles were instantly deflected 25° in the opposite direction; the current which produced this deflection lasted in its full strength only a few seconds, and ceased entirely in about one minute; it was succeeded, however, by another, transient and feeble current in the same direction. By immersing about 6 inches (≈15.3 centims.) of the middle part of the bar (which was at about 50° F. =10° C.) in a freezing-mixture
at $-26^\circ$ F. ($= -32^\circ$.2 C.), composed of 4 pounds of ice and 6 pounds of crystallized chloride of calcium, an irregular deflection of the needles, indicating another feeble molecular change and increase of magnetism was observed; the general deflection, however, obtained by this artificial cooling was in accordance with a decrease of magnetism, contrary to that which resulted from cooling at higher temperatures: a repetition of this experiment gave similar results. With a bar 3 feet ($= 91.4$ centims.) long and 1 inch ($= 25.4$ millims.) thick none of the electrical effects were obtained by the heating process, owing to the heat of the gas-burners being not sufficiently powerful.

These results show that on gradually heating a bar of wrought iron, when it attains a moderate red heat a succession of distinct and separate small movements, all of a similar kind, take place amongst its molecules,—and that on gradually cooling such a heated bar, when its temperature has sunk to moderate redness three successive molecular movements and diminishations of magnetism occur—the first being a small one, the second a very large movement, and the third also small; and these movements are all of an opposite character to those which take place during the heating. It is singular that there is no sudden large decrease of magnetism, during the process of heating, to correspond with the sudden large increase of magnetism during cooling; and this precisely agrees with the phenomenon of molecular and cohesive change already referred to (page 171). It is probable that the sudden and momentary increase of length and diminution of cohesion which an iron wire under a suitable degree of longitudinal strain undergoes whilst cooling at a moderate red heat, may be due to the sudden great increase of magnetism which it then acquires, in accordance with the fact discovered by Mr. Joule, that a rod of soft iron at $60^\circ$ F. suddenly increases in length and decreases in diameter when magnetized.

With a bar of cast steel 27 inches ($= 68.6$ centims.) long and \(\frac{7}{8}\) of an inch ($= 22.2$ millims.) in diameter, similarly examined, the fine wire coil containing 2850 turns of “No. 23” ($= 0.7$ millim. thick) copper wire, the following results were obtained. During heating, a very feeble sudden molecular change, attended by decrease of magnetism, occurred below a visible red heat; and at a moderate red heat a second sudden, and more extensive, molecular change occurred of the same kind; by further heating to a higher degree of redness no other sudden change took place. The gas-flames being now extinguished, after a period of thirty seconds a slight molecular change and increase of magnetism occurred; and in fifteen seconds more a sudden and more powerful change of the same kind took place, producing a deflection of the needles of $6^\circ$; the needles then returned to their original position.
Cold water being now very freely applied to the black hot bar, a similar deflection of $1\frac{3}{4}^\circ$ occurred; part of this last deflection was probably due to the more rapid cooling.

With a bar of cast iron 30 inches (=76·2 centims.) long and $\frac{3}{4}$ of an inch (=19 millims.) in diameter, and the same wire coils and battery, the effects obtained were similar, but much more feeble than with the bar of steel. At a feeble elevation of temperature, below that at which a similar effect took place in the steel bar, a feeble sudden molecular movement and decrease of magnetism took place. At a red heat a second similar change occurred. Soon after this the gas was stopped and the bar allowed to cool; in about twenty seconds a molecular change commenced, feeble at first and then suddenly stronger, producing a deflection of $6^\circ$ and then ceasing, as with the steel bar.

Various other small irregularities in these molecular changes in wrought iron, cast iron, and steel were observed, but are not here recorded; and each different bar gave somewhat different results. If the processes of heating and cooling were more rapid, all the changes would be more powerful.

The results I have obtained with bars of wrought iron, steel, and cast iron by the foregoing plan agree in the main with those of M. Mauritius*, obtained by quite a different method. He found that at a bright red heat none of the bars were magnetic; on cooling a red-hot steel bar the magnetism increased at first very rapidly, then for a certain time slowly, and then again followed a second period of rapid increase; cast iron behaved similarly but in a less degree; and with wrought iron the second increase does not exist. He considers the magnetism of iron is developed suddenly at a particular temperature of about 1000°.

With a bar of cast nickel about 18 inches (=45·7 centims.) long and $\frac{1}{2}$ an inch (=12·7 millims.) thick the following effects were obtained:—During heating, a slow deflection of $1\frac{1}{4}^\circ$ was obtained at a particular temperature much below a red heat; and during cooling, a deflection of $1\frac{1}{2}^\circ$ in an opposite direction occurred, apparently at the same temperature; no other deflections were obtained by heating the bar gradually to redness and then cooling. With another bar of that metal, 2 feet (=61 centims.) long and $\frac{3}{4}$ of an inch (=19 millims.) in diameter†, a thick-wire coil 6 inches (=15·2 centims.) long and $2\frac{1}{4}$ inches (=5·7 centims.) in diameter, containing 8 layers or 536 turns of "No. 17" (=1·5 millim. thick) copper wire, and a thin-wire coil 6 inches long and 2 inches in diameter, containing 14 layers or 1960 turns of "No. 27" ( = 0·25 millim. thick) copper wire, on applying the

† Obtained by the kindness of H. Wiggin, Esq., of the firm of Messrs. Evans and Askin, nickel-refiners, Birmingham.
full heat of five small Bunsen's burners, in about four minutes the needles were rather suddenly deflected $2^\circ$ with irregular action at a particular temperature not even approaching that of the lowest visible redness in the dark; and by heating to a much higher temperature, but still below redness, faint signs of a second similar molecular disturbance in the same direction were manifested. The heat being now stopped and the bar allowed to cool gradually, the same phenomena, and to the same extent, took place, but in an exactly reverse order. On repeating the experiment with this bar, but cooling the bar rapidly by continuous application of cold water, a sudden deflection of $8^\circ$ occurred when the bar acquired the proper temperature. The direction of all the deflections with this bar (as with nearly all the previous bars) during heating agreed with those producible by a decrease of magnetism, and during cooling with an increase of magnetism. By substituting for the fine-wire coil another, containing 3600 turns of size "No. 29" (=0·28 millim. thick) copper wire, very feeble results only were obtained, the coil itself offering too great a degree of conduction-resistance.

In these experiments with wrought iron, steel, cast iron, and nickel there is a very gradual magnetic change, in addition to the several sudden or irregular molecular movements and changes of magnetism; this gradual change is manifested by a very faint deflection of the needles in the same direction as those produced by most of the sudden movements.

By substituting a bar of zinc 3 feet (=91 centims.) long and 1 inch (=25·4 millims.) thick, or one of antimony 30 inches (=76·2 centims.) long and 1 inch thick, for those of the other metals, and heating their middle parts to incipient fusion, I obtained no definite movements of the galvanometer-needles. With a bar of bismuth 15 inches (=38·1 centims.) long and about $\frac{1}{2}$ an inch (=12·7 millims.) thick, and heating its middle part, also no movements of the needles took place.

With a bar 9 inches (=23 centims.) long and $2\frac{1}{2}$ inches (=6·3 centims.) thick, composed of commercial antimony containing a small amount of iron, and with suitable coils about $1\frac{1}{2}$ inch (=3·8 centims.) in length upon its ends, an irregular succession of ten distinct internal changes in the bar, corresponding to decreases of magnetism, were observed during the heating. The bar was then so hot as to char the wooden wheels containing the wire, and had to be cooled; during its cooling a continued succession of changes of an opposite kind took place. With a second bar 13 inches (=33 centims.) long and $1\frac{1}{2}$ inch (=3·8 centims.) thick, containing more iron, the effects were very feeble, chiefly owing to the wire coils being much further asunder.

The method I have adopted admits of the detection of minute
magnetic changes in bars of iron, steel, nickel, &c., especially if a powerful battery and a delicate galvanometer are employed, and might probably be used to throw a light upon the influence which the presence of foreign substances have upon iron &c.

The foregoing phenomena may be employed as an illustration of a very general (I may say, universal) property of matter which has not, that I am aware, been specially recognized as such. Every substance, even those of the simplest constitution (as the elementary bodies, and even those of them which are in the gaseous state), when acted upon by a single external force, possesses the power of dividing the influence of that force in such a way that, instead of producing only one force or one effect, it produces several; or, stated more briefly, matter has a universal property of dividing and multiplying forces and effects. For instance, simply in heating a bar of iron to redness a whole series of changes occur in its molecular structure, its magnetism, its dimensions, and its cohesive power, in addition to the changes in its specific heat, its thermoelectric capacity, and its electric conducting-power. The changes produced by heat in even so simple a substance as iron were so numerous in some of these experiments as to produce the impression that the metal was endowed with vitality.

XXI. On the Measurement of Wave-lengths by means of Indices of Refraction. By Wolcott Gibbs, M.D., Rumford Professor in Harvard University*

In a brief notice† communicated to the British Association for the Advancement of Science at its Meeting in 1849, Professor Stokes has given a method for measuring wave-lengths, which depends upon the fact that, in substances of medium refractive power, the increment of the index of refraction in passing from one point of the spectrum to another is nearly proportional to the increment of the square of the reciprocal of the wave-length. The author showed that even when the intervals were taken much longer than necessary, the error in the wave-length was usually only in the eighth place of decimals. At the date of the publication of this notice the subject of wave-lengths possessed but little interest. The recent development of the spectral analysis of light has given a new impulse to this branch of optics, and has rendered necessary the construction of a normal map of the entire solar spectrum. This has been most

* From Silliman's American Journal for July 1870. Read before the National Academy of Sciences, April 12, 1870.
successfully accomplished by Ångström*; but an attentive study of his work, as well as of the elaborate researches of Van der Willigen† and Ditscheiner‡, will show that new measurements will be far from superfluous. The imperfections even of the best ruled glasses are so great that it may be reasonably doubted whether the wave-lengths of very fine lines can be satisfactorily measured directly. Methods of determining such wave-lengths, depending upon the comparison of the refraction- and diffrac-
tion-spectra, have been given by myself§ and by Thalen||. As it seems at least desirable to multiply such methods, I will here give first a discussion of the method of Stokes in its original form, and afterwards a simplification of that method, which will also have its uses.

If Cauchy's formula for dispersion,

\[ n = a + \frac{b}{\lambda^2} + \frac{c}{\lambda^4} \]

be reduced to its first two terms, and if we then eliminate the constants \(a\) and \(b\) from three of the equations of the form

\[ n = a + \frac{b}{\lambda^2} \]

we shall obtain the three following equations, involving only wave-lengths and indices of refraction:

\[ \lambda_i^2 = \frac{(n_3-n_2)}{(n_3-n_1) \frac{1}{\lambda_i^2} + (n_2-n_1) \frac{1}{\lambda_i^2}} \qquad (1) \]

\[ \lambda_i^2 = \frac{(n_3-n_1)}{(n_2-n_1) \frac{1}{\lambda_i^2} + (n_3-n_2) \frac{1}{\lambda_i^2}} \qquad (2) \]

\[ \lambda_i^2 = \frac{(n_2-n_1)}{(n_3-n_1) \frac{1}{\lambda_i^2} + (n_3-n_2) \frac{1}{\lambda_i^2}} \qquad (3) \]

Of these equations (1) and (3) serve for extrapolation and (2) for interpolation. To test the degree of accuracy attainable in determining wave-lengths by these formulae, I have selected the measurements made by Van der Willigen¶. The indices of refraction determined by the Dutch physicist are in fact the only

† Archives du Musée Teyler, vol. i. p. 1.
‡ Sitzungsberichte der k. k. Akad. der Wissenschaften, vol. i. (1864).
|| Mémoire sur la détermination des longueurs d'onde des raies métalliques, 1868.
¶ Archives du Musée Teyler, vol. i. p. 70.
by means of Indices of Refraction.

indices which are at once sufficiently exact and sufficiently nu-
merous. In addition they have the great advantage of having been made with reference to lines in the solar spectrum the wave-
lengths of which had been measured by the same observer. There can therefore be no question of identity. As a first ex-
ample of the method, I give a determination of the wave-length of C, taking B as one of the lines exterior to C, and taking in succession seven other exterior lines more refrangible than C, to combine with B. Formula (2) was therefore employed, and with the following data and results:—

\[
\begin{array}{llll}
B & 1.61079 & 687.48 & \\
C & 1.61252 & 656.56 & \\
D & 1.61436 & 628.11 & 656.70 +0.14 \\
11 & 1.61537 & 613.96 & 656.71 +0.15 \\
13 & 1.61560 & 610.52 & 656.56 0.00 \\
14 & 1.61728 & 589.56 & 656.71 +0.15 \\
16 & 1.61978 & 561.80 & 656.76 +0.20 \\
17 & 1.62064 & 553.19 & 656.79 +0.23 \\
19 & 1.62143 & 545.83 & 656.87 +0.31 \\
\end{array}
\]

Mean of the errors . . . \[+0.17\]

In this Table the first column gives the designation or num-
ber of the line, the second its index of refraction as determined by a Steinheil prism of 60°, the third the corresponding wave-
length according to Van der Willigen, and the fourth the wave-
length as found by formula (2) by combining B with each line after C in succession.

The mean of the seven values of the wave-length of C thus found is 656.70, which is in excess of Van der Willigen's own determination of the value of C by 0.14. From this it appears that the method may be applied with a tolerable degree of ap-
proximation, even in the case of a flint-glass prism of high dis-
persive power, and for indices of refraction which refer to lines at considerable angular distances from each other. The increase in the computed values of C as the intervals between B and the second line of comparison are increased, however, will clearly appear from the Table. The following results were obtained with the indices of a second Steinheil prism, No. 2, of 46° 52' 25"-8, also of flint glass:—

\[
\begin{array}{llll}
B & 1.60521 & B and 8a & 656.21 -0.35 \\
C & 1.60694 & B and 11 & 656.33 -0.23 \\
8a & 1.60872 & B and 13 & 656.31 -0.25 \\
11 & 1.60973 & B and 16 & 656.38 -0.18 \\
13 & 1.60998 & B and 17 & 656.47 -0.09 \\
16 & 1.61408 & & \\
17 & 1.61495 & & \\
\end{array}
\]
the mean of which is 656.28, the error being -0.28. To determine to what extent the method applies when flint-glass prisms are used and the indices are selected from the more refrangible portion of the spectrum, the following data were assumed:

<table>
<thead>
<tr>
<th></th>
<th>F</th>
<th></th>
<th>G</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>35</td>
<td>1.62917</td>
<td>486.39</td>
<td>F and G</td>
<td>438.88</td>
<td>+0.30</td>
</tr>
<tr>
<td>37</td>
<td>1.63244</td>
<td>467.00</td>
<td>F and 39</td>
<td>438.76</td>
<td>+0.13</td>
</tr>
<tr>
<td>38</td>
<td>1.63828</td>
<td>438.58</td>
<td>F and 38</td>
<td>438.82</td>
<td>+0.24</td>
</tr>
<tr>
<td>39</td>
<td>1.63931</td>
<td>434.28</td>
<td>G and 35</td>
<td>438.76</td>
<td>+0.18</td>
</tr>
<tr>
<td>G</td>
<td>1.64006</td>
<td>431.12</td>
<td>35 and 39</td>
<td>438.67</td>
<td>+0.09</td>
</tr>
</tbody>
</table>

In this Table line 37 is taken as the middle line in applying formula (2), and the results obtained by combining the other lines in pairs are given in columns 4, 5, and 6. It will be seen that, as in the case of the less refrangible portion of the spectrum, the results obtained are with this prism always too high. For the purpose of comparison, I have computed the same wave-length from the indices of refraction of the second prism. The data and results are as follows:

<table>
<thead>
<tr>
<th></th>
<th>F</th>
<th></th>
<th>G</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>35</td>
<td>1.62332</td>
<td>486.39</td>
<td>F and 39</td>
<td>438.97</td>
<td>+0.49</td>
</tr>
<tr>
<td>37</td>
<td>1.63267</td>
<td>467.00</td>
<td>F and 38</td>
<td>438.92</td>
<td>+0.34</td>
</tr>
<tr>
<td>38</td>
<td>1.63221</td>
<td>438.58</td>
<td>F and 37</td>
<td>439.00</td>
<td>+0.42</td>
</tr>
<tr>
<td>39</td>
<td>1.63324</td>
<td>434.28</td>
<td>G and 35</td>
<td>438.89</td>
<td>+0.31</td>
</tr>
<tr>
<td>G</td>
<td>1.63358</td>
<td>431.12</td>
<td>35 and 39</td>
<td>438.84</td>
<td>+0.26</td>
</tr>
</tbody>
</table>

In the case of the first prism the mean of the errors is +0.21, while for the second the mean of the errors is +0.35. From this it appears that in the more refrangible portion of the spectrum the errors are considerably greater than in the less refrangible portion, even for equal differences of wave-length, and, further, that the advantage in precision is with the prism having the higher dispersive power. As the probable errors of the measurements of the indices of refraction are not given, it is impossible to determine to what extent the errors in the computed wave-lengths are due solely to want of precision in the indices. It is also to be remarked that, while with the second prism the errors in the less refrangible portion of the spectrum are affected with the sign —, in the more refrangible portion they are largely positive. The close agreement in the value of the wave-length of 37, as found by Van der Willigen, with the values as found by Ditscheiner and Ångström (438.27 and 438.28), proves that the source of error is not an erroneous determination of this quantity. It seems, therefore, certain that the nearly constant errors noted above are due in part to the fact that the indices of refraction are determined only to five places
of decimals, and in part to the high dispersive powers of the prisms employed, which would render it necessary to employ more than two terms in Cauchy's formula to obtain a closer approximation. As the formulae for interpolation would in this way be rendered extremely complicated, it is better, in the case of any series of observations embracing a particular part of the scale, simply to determine the mean of the errors, and to apply this mean with its proper sign to the computed values of the particular wave-length to be determined by the measurement of indices of refraction. If we apply such a correction in the cases of the four series of data and results given above, we find for the corrected values of the wave-lengths the following numerical results:

<table>
<thead>
<tr>
<th></th>
<th>I.</th>
<th>II.</th>
<th>III.</th>
<th>IV.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>656.53</td>
<td>656.49</td>
<td>438.67</td>
<td>438.72</td>
</tr>
<tr>
<td></td>
<td>656.54</td>
<td>656.61</td>
<td>438.55</td>
<td>438.54</td>
</tr>
<tr>
<td></td>
<td>656.38</td>
<td>656.59</td>
<td>438.61</td>
<td>438.57</td>
</tr>
<tr>
<td></td>
<td>656.54</td>
<td>656.66</td>
<td>438.55</td>
<td>438.65</td>
</tr>
<tr>
<td></td>
<td>656.59</td>
<td>656.75</td>
<td>438.54</td>
<td>438.54</td>
</tr>
<tr>
<td></td>
<td>656.62</td>
<td>...</td>
<td>438.44</td>
<td>438.49</td>
</tr>
</tbody>
</table>

The true values being respectively 656.56 and 438.56. These results are, I think, sufficient to show that a valuable control for the accuracy of measurements of wave-lengths may be obtained even when prisms of high dispersive power are employed, provided that the intervals taken are not too large. It seems at least probable that a greater degree of precision is attainable in measuring indices in the case of substances of high than in those of low dispersive power, partly because the angular deviations to be measured are larger, and partly because the spectral lines are less crowded together.

The following example will serve to illustrate the advantage of taking shorter intervals:

<table>
<thead>
<tr>
<th></th>
<th>Lines.</th>
<th>(\lambda)</th>
<th>Indices.</th>
<th>(\lambda)</th>
<th>Indices.</th>
<th>(\lambda)</th>
</tr>
</thead>
<tbody>
<tr>
<td>25(b)</td>
<td>518.63</td>
<td>1.62459</td>
<td>...</td>
<td>1.61882</td>
<td>...</td>
<td></td>
</tr>
<tr>
<td>26</td>
<td>517.51</td>
<td>1.62472</td>
<td>517.61</td>
<td>1.61895</td>
<td>517.56</td>
<td>1.61901</td>
</tr>
<tr>
<td>27(\beta)</td>
<td>517.07</td>
<td>1.62479</td>
<td>...</td>
<td>1.61901</td>
<td>...</td>
<td></td>
</tr>
</tbody>
</table>

The data are here also taken from Van der Willigen's measures with the same prisms.

When the angular distances between three spectral lines are not too great, the angular deviation of the lines may, as I find, be substituted for the indices of refraction in formulae (1), (2), and (3). The differences between the angular deviations are, of course, to be converted into seconds. The following results will show the degree of accuracy attainable by this method, the data
Prof. W. Gibbs on the Measurement of Wave-lengths.

being taken from Ditscheiner’s* measurements of the indices of a flint prism by Steinheil, of refracting angle 60° 4' 59":—

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>B</td>
<td>593</td>
<td>687·06</td>
<td>40 55</td>
<td>...</td>
<td>1·61358</td>
</tr>
<tr>
<td>C</td>
<td>694</td>
<td>655·95</td>
<td>47 19 655·97</td>
<td>1·61537 655·82</td>
<td></td>
</tr>
<tr>
<td></td>
<td>877</td>
<td>613·57</td>
<td>48 8</td>
<td>...</td>
<td>1·61824</td>
</tr>
</tbody>
</table>

From this it appears that the error in the determination of the wave-length of the middle line C is only +0·02 when the angular deviations are employed, but amounts to —0·13 when the indices of refraction are taken as the elements of the calculation. Yet the interval between B and 877 is very large.

The following data are taken from another part of the scale, the measurements being made with the same prism:—

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>b</td>
<td>1648·8</td>
<td>517·13</td>
<td>49 16</td>
<td>...</td>
<td>1·62775</td>
</tr>
<tr>
<td>1655·6</td>
<td>516·58</td>
<td>49 44 516·56</td>
<td>1·62782 516·61</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1693·8</td>
<td>514·08</td>
<td>49 47</td>
<td>...</td>
<td>1·62817</td>
<td></td>
</tr>
</tbody>
</table>

Hence the error in the determination of the wave-length of 1655·6 is, when the angular deviations are taken, only —0·02, and when the indices are taken +0·03. It must be borne in mind that in all the above-mentioned examples the angles are those of minimum deviation. As the numbers upon Kirchhoff’s scale also represent angular (though not minimum) deviations, it seemed worth while to determine how far for a short interval these could be employed. Taking the three scale-numbers of the last example, the error in the wave-length of 1655·6 was found to be —0·38; and when the scale-numbers were taken as the sines or tangents of corresponding angles, +0·09.

The following data are taken from the more refrangible part of the spectrum, the measurements being also those of Ditscheiner, and made with the same prism:—

| G           | 2854·7 | 430·88              | 50 52 430·68 | 1·64334 430·83 |
|             | 2969·7 | 429·90              | 50 47 | ... | 1·64352 |

In this case the error in the wave-length of the middle line (2854·7) is —0·20, as determined from the angular deviations, and —0·05 as determined from the indices. It must be borne in mind that, in this part of the spectrum, the determination

* Bestimmung der Wellenlängen der Fraunhoferschen Linien des Sonnen-spectrums, p. 43.
both of wave-lengths and of indices of refraction is difficult, on account of the feeble intensity of the light.

Since only the differences between the angular deviations of the spectral lines are employed in the formulæ above given, it follows that, in determining wave-lengths by the method in question, it is not necessary to employ a spectrometer with a divided circle and appliances for the measurement of large angles. A common spectroscope will be sufficient if the observing-telescope be provided with a filar micrometer, by means of which the angular distances of any given line from two other lines of which the wave-lengths are known may be measured. The researches of Angström leave nothing to be desired as regards the wave-lengths of standard lines; and the method given may prove a convenient means of determining with all requisite precision the wave-lengths of metallic lines.


The discovery by M. Hock that certain comets may be so arranged in groups that all the members of the same group have directions nearly coincident and the planes of their orbits a common line of intersection, as well as the discovery by Professor Kirkwood, that there exists a connexion between the aphelion positions of comets and the direction of the sun's motion in space, tends to confirm the opinion that the parabolic comets are not permanent members of the solar system revolving in elliptic orbits of great length, but are casual visitors, coming from the interstellar regions of space, and, after passing through their perihelia, moving off, never perhaps to return again. On the other hand, the fact that the orbits of most of these comets do not sensibly differ from parabolas, has given rise to the opposite opinion that they are really ellipses of great length. No hyperbolic orbit of other than very small excentricity has been met with, contrary to what might be expected on the supposition that comets are non-periodic. For the parabolic character of a comet's orbit shows that its motion at a great distance from the sun is very nearly the same as that of the sun, and so is not independent of the sun's motion, as it might be expected to be on the hypothesis that it is not a permanent member of the solar system. Supposing that the motion of comets at a very great distance is independent of the motion of the sun, their average velocity at a great distance relatively to the sun must be at least as great as the velocity of the sun's motion in space.

* Communicated by the Author.
This follows also from another consideration. The average velocity of comets when at a great distance from any stars, with respect to any one star, must be as great as their average velocity with respect to any other star. As the average velocity of the stars is probably greater than, or at least as great as, the sun’s velocity in space, the average velocity of comets at a great distance with respect to the sun must be at least as great as the velocity of the sun in space. This velocity is, according to Struve, 1.6 radius of the earth’s orbit per annum; and this velocity at a very great distance gives for a comet whose perihelion distance is equal to the earth’s distance from the sun an orbit whose excentricity is 1.06, an excentricity much greater than any which has yet been calculated. But the average velocity at a great distance of those comets which come near enough to the sun to be observed from the earth will not be the same as the average velocity of all comets at a great distance. This is easily seen from the consideration that, of two comets which have exactly the same direction at a great distance, that which has the smaller velocity will also have the smaller perihelion distance. On this account the average velocity at a great distance of those comets which come within a sufficiently small distance of the sun to be observed from the earth will be less than the average velocity of all comets at a great distance. On the other hand, supposing that there are as many comets in space moving with one velocity relatively to the sun as with another (which supposition will within certain limits be approximately true), the number of comets whose velocity at a great distance is V which within a given time come into the sun’s sphere of attraction will be proportional to V, and thus the average velocity at a great distance of those comets which within a given time describe an orbit about the sun will on this account be greater than the average velocity of all comets. For these reasons the average excentricity will differ from 1.06.

It is my object in the present paper to calculate what is the probability that a comet which approaches near enough to the sun to be observed from the earth will have an excentricity differing by any given amount from unity, and, by comparing this theoretical probability with the facts derived from observation, to draw conclusions as to the probable character of cometary orbits.

Let us suppose a sphere described about the sun with radius R of such a magnitude that the sun’s attraction will not sensibly influence the directions and velocities of those comets which are near its surface. Leaving out of consideration for the present the existence of the fixed stars, this will be the case only when R is taken so large that the velocity which a comet has acquired from the sun’s attraction in coming to the distance R is so small that it may be
neglected in comparison with the whole velocity of the comet. We will take $R$ so large that this may be the case for all comets, except those whose velocities at $R$ are very small indeed; and we will for the present consider that even these comets with very small velocities are as likely to have one direction as another.

Let us fix our attention upon those comets which enter the sphere through any small area. About the centre of this small area describe a sphere, and let $r$, $\theta$, $\phi$ be the polar coordinates of any point on this sphere, $\theta$ being the angle which the radius through the point makes with the radius through the point opposite the sun. The probability that a comet will have a direction parallel to any of the radii which pass through the small element of the sphere $r^2 \sin \theta \Delta \theta \Delta \phi$ is proportional to this area. Hence the probable number of comets having directions parallel to radii through $r^2 \sin \theta \Delta \theta \Delta \phi$ which pass within a given time through an area perpendicular to their direction is also proportional to $r^2 \sin \theta \Delta \theta \Delta \phi$; and therefore the probable number which will pass in a given time through the small area of the sphere about the sun, which area is inclined to their direction at an angle $\frac{\pi}{2} - \theta$, is proportional to $r^2 \sin \theta \cos \theta \Delta \theta \Delta \phi$.

Hence the probable number of comets which will pass through the area in a given time and are inclined to the sun's direction at angles lying between $\theta$ and $\theta + \Delta \theta$ is proportional to $r^2 \sin \theta \cos \theta \Delta \theta$.

We have not yet considered the velocity of the comets which enter. Supposing that there are as many comets with one velocity as another, the probable number of comets which in any time enter any area and have velocities lying between $V$ and $V + \Delta V$ will be proportional to $V \Delta V$. Hence the probable number of comets which have velocities between $V$ and $V + \Delta V$ and directions inclined to the sun's direction at angles between $\theta$ and $\theta + \Delta \theta$ which pass in any time into the sphere is proportional to $V r^2 \sin \theta \cos \theta \Delta \theta \Delta V$.

We proceed now to find the probable number of comets having perihelion distances lying between $q$ and $q + \Delta q$, and eccentricities lying between $e$ and $e + \Delta e$.

We have the following equations between $V$, $\theta$, $e$, and $q$:

$$1 - e^2 = \frac{V^2 R^3 \sin^2 \theta}{\mu} \left(\frac{2}{R} - \frac{V^2}{\mu}\right)$$  \hspace{1cm} (2)

(see Tait and Steele's 'Dynamics,' p. 91), and

$$V^2 = \mu \left(\frac{2}{R} + \frac{e - 1}{q}\right)$$  \hspace{1cm} (3)

Substituting for $V^2$ in (2) from (3), we have

$$1 + e = \left( \frac{2}{R} + \frac{e - 1}{q} \right) \frac{R^2 \sin^2 \theta}{q},$$

or

$$q^2(1 + e) - 2Rq \sin^2 \theta - (e - 1)R^2 \sin^2 \theta = 0. \quad \text{(4)}$$

To a perihelion distance $q$ and an excentricity $e$ correspond a velocity $V$ and a direction $\theta$ at $R$ given by (3) and (4). To $q + \Delta q$ and $e$ correspond $V + \frac{dV}{dq} \cdot \Delta q$ and $\theta + \frac{d\theta}{dq} \cdot \Delta q$. To $q$ and $e + \Delta e$ correspond $V + \frac{dV}{de} \cdot \Delta e$ and $\theta + \frac{d\theta}{de} \cdot \Delta e$; and to $q + \Delta q$, $e + \Delta e$ correspond $V + \frac{dV}{dq} \cdot \Delta q + \frac{dV}{de} \cdot \Delta e$ and $\theta + \frac{d\theta}{dq} \cdot \Delta q + \frac{d\theta}{de} \cdot \Delta e$.

If we take $V$ and $\theta$ as the coordinates of a point referred to rectangular coordinates, then $\Delta V, \Delta \theta$ represents the area of a small rectangle whose sides are $\Delta V, \Delta \theta$, and the expression (1) may be written

$$V \sin \theta \cos \theta \times \text{rectangle } dV \cdot d\theta.$$
of Cometary Orbits.

\[ \frac{dV}{de} = \frac{\mu}{2qV}, \]
\[ \frac{dV}{dq} = -\frac{\mu(e-1)}{2q^2V}, \]
\[ \frac{d\theta}{de} = \frac{q^2 - R^2 \sin^2 \theta}{2R \sin \theta \cos \theta (2q + Re - 1)}, \]
\[ \frac{d\theta}{dq} = \frac{(1 + e)q - R \sin^2 \theta}{R \sin \theta \cos \theta (2q + Re - 1)}. \]

To simplify the last two expressions, substitute for \( R \sin^2 \theta \) its value
\[ \frac{q^2(1 + e)}{2q + Re - 1}, \]
and we have
\[ \frac{d\theta}{de} = \frac{q^2(q - R)}{R \sin \theta \cos \theta (2q + Re - 1)^2}, \]
\[ \frac{d\theta}{dq} = \frac{q(e + 1)(q + Re - 1)}{R \sin \theta \cos \theta (2q + Re - 1)^2}; \]

whence, by substitution and reduction,
\[ V \sin \theta \cos \theta \left( \frac{dV}{de} \frac{d\theta}{dq} - \frac{dV}{dq} \frac{d\theta}{de} \right) \Delta e \Delta q = \frac{\mu e}{2R(2q + Re - 1)}, \Delta e \Delta q. \]

Putting \( e = 1 + \epsilon \) and leaving out the factor \( \frac{\mu}{2R} \), this becomes
\[ \frac{1 + \epsilon}{2q + R \cdot \epsilon} \Delta e \cdot \Delta q. \]

The integral of this with respect to \( \epsilon \) is
\[ \frac{1}{R^2} \left\{ 2q + R \cdot \epsilon + (R - 2q) \log (2q + Re) \right\} \Delta q, \]
and this, taken between the limits 0 and \( \epsilon \), is
\[ \frac{1}{R^2} \left\{ R \cdot \epsilon + (R - 2q) \log \left( 1 + \frac{Re}{2q} \right) \right\} \Delta q \]
\[ = \frac{1}{R} \left\{ \epsilon + \log \left( 1 + \frac{Re}{2q} \right) \right\} \Delta q \text{ very nearly}, \]
\[ = \frac{1}{R} \log \left( 1 + \frac{Re}{2q} \right) \Delta q \text{ nearly}; \]
\[ \therefore \text{the number of comets which have perihelion distances} \]

\[ \therefore \text{O} \]
lying between \( q \) and \( q + \Delta q \) and excentricities lying between \( 1 \) and \( 1 + \epsilon \) is proportional to \( \log \left( 1 + \frac{R\epsilon}{2q} \right) \Delta q \).

This expression has been obtained on the assumption that at the distance \( R \) there are as many comets with one velocity as with another. Taking the average velocity of comets at a distance \( R \) to be \( 1.6 \), this assumption may be taken as roughly true for all velocities between \( 1.6 \) and \( 0 \), and for velocities not very much greater than \( 1.6 \). We will assume that it is true for all values between \( 3 \) and \( 0 \), and that there are no comets with velocities greater than \( 3 \). The fact that there are many comets which have their perihelion positions nearly in the direction of the sun's motion, shows that there are comets whose independent velocity in space before their attraction to the sun is greater than the velocity of the sun, or greater than \( 1.6 \). Hence there are comets whose relative velocities at a great distance with respect to the sun is greater than double the velocity of the sun, or greater than \( 3.2 \).

From the formula (3), the value of \( \epsilon \) for a velocity \( 3 \) at the distance \( R \) is, when \( R \) is very large, \( \frac{9}{\mu} q \). With our present units of time and space, \( \mu = 4\pi^2 \), and therefore \( \epsilon = \frac{9}{4\pi^2} q = 0.23 \times q \); and the whole number of comets whose perihelion distances lie between \( q \) and \( q + \Delta q \) is proportional to \( \log \left( 1 + 0.23 \times \frac{R}{2} \right) \Delta q \). The proportion of the whole number of comets to the number of comets with excentricities less than \( \epsilon \) is

\[
\log \left( 1 + 0.23 \times \frac{R}{2} \right) : \log \left( 1 + \frac{R\epsilon}{2q} \right).
\]

The greater \( R \) is assumed to be, the nearer is this ratio to unity, and consequently the smaller is the average excentricity of all the comets having perihelion distances between \( q \) and \( q + \Delta q \).

Hence the average excentricity of cometary orbits with any given perihelion distance \( q \) depends upon the distance at which we assume that the directions and velocities of comets are altogether independent of the direction in which the sun lies with respect to them. The reason for this may be explained thus. Suppose at a distance \( R \) the motions of comets are independent of the position of the sun. Describe about the sun a sphere with radius \( R \) and another with radius \( \frac{R}{1000} \). Of the comets which enter the larger sphere, those which have a very small velocity will have their directions most deflected towards the sun.
of Cometary Orbits.

Hence a greater number of comets with small velocities will enter the smaller sphere than of comets with large velocities; and, also, they will not have their directions of motion indiscriminately distributed, but the less the velocity of a comet the less will its probable direction of motion be inclined to the direction of the sun. Now, if there were no other bodies in the universe but the sun and comets, R would have to be taken infinite, and consequently there would be none but parabolic comets.

But the existence of the stars entirely alters the conditions of the problem. If we take the sphere (radius R) to enclose other stars besides the sun, the directions of the comets entering it will be altogether independent of the direction in which the sun lies with respect to them. The same will be the case if R be taken so small as to exclude all the stars; but yet not so small that the attraction of the sun will be so much superior to the attraction of the other stars as to cause the direction of the resultant of all the attractions acting on the comet to lie of necessity nearly in the direction of the sun.

We will assume that the annual parallax of the nearest stars is 1", so that their distance is $2 \times 206265$, or greater than 400000. We will take three values of R for our calculation. First, $R = 400000$, which will exclude all the stars; secondly, $R = 200000$, which will be about halfway between the sun and the nearest stars; and lastly, $R = 40000$, or about ten times nearer the sun than any star.

The following Table exhibits the results of calculations on these three assumptions:

<table>
<thead>
<tr>
<th>Value of R</th>
<th>Out of 100 comets, the probable number which have eccentricities greater than</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1.001.</td>
</tr>
<tr>
<td>$R = 400000$</td>
<td>$q = \frac{1}{2}$</td>
</tr>
<tr>
<td>$R = 400000$</td>
<td>$q = \frac{1}{2}$</td>
</tr>
<tr>
<td>$R = 200000$</td>
<td>$q = \frac{1}{2}$</td>
</tr>
<tr>
<td>$R = 200000$</td>
<td>$q = \frac{1}{2}$</td>
</tr>
<tr>
<td>$R = 40000$</td>
<td>$q = \frac{1}{2}$</td>
</tr>
<tr>
<td>$R = 40000$</td>
<td>$q = \frac{1}{2}$</td>
</tr>
</tbody>
</table>

The assumption of the values 400000 and 200000 for the radius of the sphere which we suppose the comets to enter is open to the objection that at this distance the attraction of the stars will not be so small, compared with the attraction of the sun, as not to disturb the subsequent orbits of the comets which enter it. By taking $R = 40000$, the attractions of the stars may be neglected; but the motions and directions of comets at this dis-
tance cannot be supposed entirely independent of the position of the sun.

The truth will lie within the results of the calculation on these three assumptions. We see, then, that a large percentage of those comets which approach the sun in hyperbolic orbits will have orbits with excentricities greater than 1'02. Now out of the large number of comets whose orbits have been carefully calculated not one has an excentricity greater than 1'02; and we are therefore led to the conclusion that though several known comets are probably moving in hyperbolic orbits, yet by far the greater number of those comets whose orbits are undistinguishable from parabolas are moving in elliptic orbits of very great length.

Roundhay Vicarage,
August 2, 1870.

[To be continued.]

XXIII. Experiments and Observations on the Adhesion between Solids and Liquids. By Giovanni Luvini, Professor of Physics in the Royal Military Academy of Turin*.

M. PLATEAU, in the Eighth Series of his Theoretical and Experimental Researches on Liquids†, describes the results of a very beautiful experiment from which he deduces some important consequences. In a cylindrical glass vessel, 11 centims. in diameter, he arranged a magnetic needle in the form of a rhomb 10 centims. in length, 7 millims. in width, and 0'3 millim. in thickness, turning in a horizontal plane on an axis coincident with that of the vessel. The needle being moved 90° out of its position of equilibrium and left to itself, returned back with a velocity depending on its length, on the magnetic intensity of the earth and of the needle, and on the passive resistance that the needle had to encounter during its return motion. M. Plateau arranges the experiment in such a manner as to be able to measure with great precision the time occupied by the needle, after it has been turned out of its plane, in traversing the first 85° towards its position of equilibrium. A liquid is poured into the vessel to such a height as to reach to the under surface of the needle, so that this may rest upon the liquid while the upper surface is exposed to the air. Under these conditions we have to determine the time occupied by the needle in traversing the first 85° upon the surface of the liquid. In another form of the

* Translated by Charles Tomlinson, F.R.S., from the Proceedings of the Royal Academy of Sciences of Turin of the 19th of June, 1870.
† Transactions of the Academy of Sciences of Brussels. Presented to the Academy July 4, 1868.
experiment the needle is totally immersed in the liquid, and under this modified condition we have to determine the time in which the needle traverses the first $85^\circ$. Considering that when only one face of the needle is in contact with the liquid, the other being in the air, the friction, or rather the resistance to its motion, ought to be less than when the needle is totally immersed, it might be supposed that the time occupied by the needle in traversing the $85^\circ$ would be less in the one case than in the other. When the experiment was tried with water, the very reverse of this was the result; the needle traversed more slowly on the water than in it. Many other liquids were tried, such as glycerine, solutions of sodic carbonate, of potassic nitrate, of calcic chloride, of albumen, of various kinds of soap; and the effect was the same as with water. The reverse result was obtained with alcohol, turpentine, olive-oil, ether, and carbonic disulphide. M. Plateau (ably assisted by his son) found that on the surface of the liquids last named the needle moved more quickly than when it was submerged.

This is not the place to show how M. Plateau, from these results, seeks to demonstrate the existence in liquids of a viscosity which has a different value on the surface to what it has in the interior of the same liquid—nor the ingenious manner in which he determines the relation of the viscosity to the surface tension, describing the remarkable methods adopted by himself and other physicists for measuring this last-named property. My present object is to point out some experiments of my own, and certain ideas suggested by them.

My opinions respecting cohesion and adhesion in general, and adhesion between solids and liquids in particular, as set forth in the fourth edition of my *Corso di Fisica Sperimentale*, published in 1868*, prevent me from agreeing with M. Plateau that the phenomena described by him are simple effects of viscosity. The experiments of Fusinieri, of Tomlinson, together with my own, have satisfied me that very minute, and in some cases insensible causes may greatly modify the relation of the adhesion to the cohesion of liquids, and that the smallest degree of impurity, not only in the liquid, but in the vessel in which the experiment is made, may vitiate the result. Hence, among other conditions, I have strongly insisted on the cleanliness of the apparatus and the purity of the substances used in the experiments, even before the publication of Tomlinson’s paper “On the Effects of a Chemically Clean Surface,” published in 1868†. For example, in speak-

---

* The first part of this work was published in parts in 1867.
† Philosophical Magazine for October 1868. Professor Luvini is not aware that in my first paper on the Cohesion-figures of Liquids, published in the Philosophical Magazine for October 1861, and in many other papers
ing of adhesion, I wrote in my Treatise the following paragraph on purity of surface:—"If you wish to study the adhesion between two bodies, it is of great importance that the surfaces of contact be clean and free from every other substance, such as moisture, grease, dust, &c. Indeed if there be any extraneous substance between two bodies, it will adhere to them with greater or less energy, depending on the magnitude and continuity of the stratum, and so modify the effect in question. Absolute purity of the surfaces is all but impossible to obtain, since the air itself, or the substances used in cleansing, will adhere in some way or other to the surfaces already partially clean, and will deposit on them dust or extraneous matters, which all concur to modify the force of adhesion between the surfaces."

On first reading the account of M. Plateau's experiments, it occurred to me that the superficial stratum of liquids would so far differ from the lower strata inasmuch as the former is exposed to the dust of the air. In order to test this I repeated some of M. Plateau's experiments in the air, and so far reproduced his results as to satisfy myself that I was acquainted with his manipulation. I then put the needle into water contained in a wide glass receiver, and turned over this another receiver full of water just as if I were going to collect a gas generated by the needle itself. This second receiver, which was narrower than the first, was lowered to the bottom of the first so as completely to shut in the needle. The receiver being properly supported, I began a series of observations in order to determine the time occupied by the submerged needle in traversing 85°. I then gently introduced gas into the inverted receiver until the water was so far displaced that the lower surface of the needle rested on the water while the upper was in contact with the gas. I then once more determined the time occupied by the needle in traversing the 85°. The gases used in these observations were air, hydrogen, carbonic acid, and oxygen. The results in these different gases were approximate, at least within the probable errors of experiment.

I must, however, admit that this method of experimenting is somewhat uncertain and troublesome. If the level of the liquid descend too low, one end of the needle rises up while the other end adheres to the liquid; or if the level be a little too high, the needle often floats by capillary action and so gets shifted off from its support. Moreover the needle rusts and varies in magnetic intensity, which last circumstance was noticed by Plateau.

afterwards published in this Journal and elsewhere, chemical cleanliness is insisted on, and directions are given for obtaining it, as a necessary condition to success in obtaining a large number of varied phenomena.—Tr.
A still greater evil connected with this method is that one and the same body has to contend in contact with different liquids; and is it not possible that the results will vary by varying the form and material of the solid that is set in motion on the liquid or within it? The laws of capillary action would never have been discovered if the phenomena had been studied only by means of one narrow tube of one material; so it appeared to me that the extensive class of phenomena thus indicated by M. Plateau could not be properly investigated unless a diligent examination were made as to the behaviour of solids of varied shape and material set in motion upon the surface of a liquid or below it. I held it to be impossible that the varying adhesion between solids and liquids would not produce some variation in the effects now under consideration.

In carrying out this idea, it first occurred to me to make the axis on which the needle turned moveable, and to fix to it the solid to be tried. But as the function of the needle in this experiment is to restore to the position of equilibrium a body disturbed therefrom, would not the torsion of a filament be preferable to the magnetic force? Coulomb's balance seemed to be well adapted to this form of experiment. To the filament of this balance is attached in a horizontal position the solid that is to be lowered to the surface of the liquid or to be submerged therein. The liquid is contained in a vessel placed beneath the cover of Coulomb's apparatus. The thread at its lower extremity carries a horizontal index playing within a graduated circle attached to the lower part of the case; while the aperture at the top is closed with a screw furnished with a finger moving through a small space, by means of which it can arrest the index or leave it free in its course.

The solid being put into the desired position with respect to the liquid, the filament is equilibrated so that the index may be opposite the arresting finger. This is best done by turning the upper circle of the cover. The index then stops; the filament is twisted a certain number of degrees either to the right or to the left; the arrest is removed, the index turns; but its first movement is a little uncertain, as is that of Plateau's needle. I wait until it has reached 5° from its position of equilibrium, and then, setting out from some known point, I count the number of degrees through which the index moves in a given time, such as 10, 15, 20, 60 or more beats of a common watch.

Experiments were made in this way in pairs. A first experiment, made by twisting the filament to the right, was soon succeeded by a second experiment, in which the filament was turned to the left. If in both cases the index stop exactly at the position of equilibrium of the filament, the results are identical. If
this condition fail, however little, the results are not coincident. I found, however, from a large number of trials, that in the latter case the mean of each pair of experiments gave results perfectly agreeing with those first obtained, even when the index came to rest 2° or 3° from the position of equilibrium. I should notice, however, that in my experiments the torsion of the thread was never less than 10°, but more frequently it was 40°. Knowing how much torsion to give, abridges in many cases the time required for an experiment.

I now procured a number of needles and plates of various forms and dimensions of brass, copper, iron, steel, glass, wood, bone, whalebone, &c. The plates were cut, some in the form of rhombs like a magnetic needle, some rectangular, some circular, some in segments of circles, many in the form of a disk with circular holes, or with radial openings, or with holes in the form of sectors extending to the circumference, still leaving an annular boundary.

My first trials of these materials were with water, sometimes pure and contained in vessels more or less clear; at other times with certain substances dissolved in it. At first I registered only the most salient results, such as would direct me in giving my experiments a more determined aim. When the results were concordant, I began a regular course of experiments and formed a series of 185, most of the members of which contained eight observations. The only liquids I have hitherto used are pure water, or water with substances dissolved in it, and mercury.

Although I consider the inquiry scarcely begun, yet the following principal results appear to be not unworthy of attention.

1. That the resistance of the surface of the liquid to the motion of the horizontal plates does not sensibly change, or changes very little, with the amount of immersion of the plate; for whether its lower surface scarcely touch the surface of the liquid, or the half or the whole of its thickness be submerged, the resistance it encounters is always the same. It is also the same when the disk is sunk below the level of the liquid, or when the latter is raised by capillarity round the edge of the disk, as also when the disk, having been submerged, is raised above the level and remains in contact therewith by means of the liquid stratum that it carries up.

2. That on the surface, as in the interior, the resistance of liquids to the motion of solids increases with the time of exposure of the liquids to the air, and with the duration of contact of the liquid with the solid. This result does not agree with that of Hagen*, who found the surface-tension of water in contact

* "Ueber die Oberfläche der Flüssigkeiten," Mem. of the Academy of Berlin, 1845.
with air to diminish by exposure (the tension of water at 10° being 7.53 milligrammes per millimetre of length, and after some hours not more than 4.69 milligrammes). Hence at first view there seemed to be something anomalous in the two statements; for if both be true, then it seems to follow that the superficial resistance opposed by the liquids to the motion of solids has a different origin from that of tension. Plateau refers this resistance to the viscosity of liquids.

The increased resistance of the liquids operated on by me, in consequence of a protracted exposure to the air, could only arise from a more or less rapid alteration in them, either by the adhesive action of the air or by the deposition of aerial dust. So trifling a circumstance disturbs the purity of a liquid, that it is sufficient in some cases to touch it with the tip of the finger to prevent its adhesion to other liquids. Hence the action of dust from the air will readily be understood.

The increased resistance, then, of the liquids in proportion to the continued contact with the solids is easily explained, seeing that this prolonged contact allows an increased mass of liquid to adhere to the solid. Such an increase of adhesion takes place between solids, and still more between solids and liquids. A clean plate of brass put into fresh water comes out all but dry; let it remain in the water some hours, and on taking it out the wetted portions will be found to have increased with the duration of the immersion. This effect is best seen by sinking the plate horizontally in the water and withdrawing it in the same position. This effect specially applies to plates of glass and of steel upon and within mercury. The mercury was washed, dried, and filtered, and it appeared bright and clean, but it readily dragged a tail on being moved about on glass or steel. Yet these plates, both on the surface of mercury and below it*, yielded to the torsion of the filament and began to move; but in a few minutes they met with so much resistance as no longer to move except under the torsion of a stronger filament. On examining the plates, it was found that the points attacked by the mercury became more numerous and extensive the more the contact was prolonged. I hope to repeat these experiments with purer mercury.

3. The resistance of one and the same liquid varies with the nature of the solid. If, instead of a steel needle, M. Plateau had used one of glass or other material, his results would have been

* The torsion-line has near its lower extremity a leaden bullet surmounted by a small capsule; and the body to be experimented on is attached to the bullet in various ways according to circumstances. The small capsule is for the reception of shot, for the purpose of maintaining the line at nearly constant tension, whether the body be in air, on the surface of the liquid, or below it.
different from those actually obtained by him. I will cite a single example in support of this. One day I had a plate of tinned iron in contact with the surface of water, and noticed that the water became sensibly changed by the oxidation of the iron and other influences that I could not explain. Now this plate of tinned iron and a similar plate of brass experienced an equal resistance on the surface of the water thus changed, while below the surface the resistance encountered by the brass was considerably less than that encountered by the tinned plate.

4. The resistance offered by the surface of liquids to solids may be distinguished into linear and superficial, while below the surface the resistance is superficial only. This distinction, though simple, is important. The resistance which the body encounters when its surface is in contact with the liquid is different as compared with the resistance it meets with at the line of separation between the surface of the liquid and the upper external surface of the solid. I name this latter the linear resistance, the former the superficial. The linear does not exist when the body is totally immersed in the liquid, while the superficial in the case of thin plates so immersed has a value sensibly double that which is experienced at the surface.

The apparent complications and irregularities in the results obtained when using metallic solids of various forms and contours disappeared as soon as I had made the above distinction. With plates of the same material in the same liquid I found, in some cases depending on the form, less resistance below the surface, as happened to Plateau with the needle in the water and numerous saline solutions; while in other cases there was a less amount of resistance at the surface of the liquid, contrary to the results given by Plateau.

The theory is easy, and naturally follows from the phenomena. Let \( \lambda \) be the linear resistance and \( \sigma \) the superficial. The total resistance on the surface of the liquid is \( \lambda + \sigma \). Below the surface, for thin plates it is \( 2\sigma \). But \( \lambda \) depends on the contour of the plate, \( \sigma \) on the superficies; and it will be understood that whatever be the linear resistance for every unit of length, and the superficial for every unit of superficies, dependent on the relation of the contour to the superficies of the plate, we thus get for one and the same plate, and one and the same liquid, one of three cases:

\[
\lambda + \sigma > 2\sigma, \quad \lambda + \sigma = 2\sigma, \quad \lambda + \sigma < 2\sigma.
\]

If M. Plateau will repeat his experiments with a magnetic needle in which the greater diagonal is equal to that of the needle already used by him, and with the minor diagonal equal to the half of the major, he will probably obtain results different
from those already arrived at by him. Such a needle moves more quickly on the surface of water than below it.

It must be remarked that the linear resistance does not depend on the internal contour, but solely on those parts of it which move normally or obliquely to their direction. The contour of a circular disk which turns on its centre experiences little or no resistance. The portions of the contour oblique to the direction of their motion operate sensibly as their projections on the normal to the direction of the motion are at equal distances from the centre.

Further experiments may lead to the introduction of some modifications in the foregoing conclusions.

---

XXIV. Memoir on Internal Work in Gases.
By M. Achille Cazin.

[Continued from p. 99.]

§ IX. Influence of moisture in the gas.

Sections V., VI., VII., VIII. indicate what are the indispensible precautions for a good series of experiments. In the following sections I shall adduce several facts which will prepare for a complete explanation of the phenomenon.

I shall first demonstrate that we cannot admit the depression of the curve of the $h$'s to be due to aqueous vapour in the gas.

In order to ascertain the possible value of this objection, let us examine the most unfavourable case—that in which the gas in the reservoirs is saturated with aqueous vapour.

The whole apparatus having a capacity of 42.868 litres, contains 7.181 grms. of vapour at 20°, under the tension of 17 millims. of mercury, equivalent to 125 millims. of sulphuric acid. When the whole of the gas in reservoir B is made to pass into reservoir A, there is supersaturation in the latter, and a portion of the vapour there is liquefied and deposited on the sides. If the vapour which remains were condensed during the expansion, forming a mist, the depression of $h$ would have just the value of 125 millims., and the curve would afterwards rise in proportion as the mist disappeared and the water condensed on the sides returned to a state of vapour; but this last effect would be slow, and the depression would maintain itself a long-time, probably longer than in my experiments.

Suppose the hygrometric state to be $\frac{1}{2}$ in the gas, and that we compress it to 4 atmospheres in the reservoir A; there would still be saturation in this reservoir, and the depression would still take place; but this time it could not exceed 31 millims. of mercury; and as there would be no water condensed on
its sides, this depression would disappear as rapidly as the mist. Thus we should not observe the effect described; but the presence of aqueous vapour would exercise a considerable influence on the magnitude of the depression. Hence the objection is important, and it was indispensable to be certain that the gas was completely dried.

For this purpose 2 metres of pumice-stone saturated with sulphuric acid were placed between the tubulures H and H'; and after having compressed the gas in A by means of the pump, it was allowed to return to the reservoir B, passing through the long column of pumice-stone as slowly as was wished: it was sufficient to regulate properly the stopcocks r', r'' (fig. 2). This manipulation was repeated several times before Series VII. (on hydrogen) was commenced; and it was ascertained that no appreciable moisture was deposited in a drying-tube, by weighing the latter before and after the passage of the gas. Experiments A, B, C, D of the following series were then made; the minimum of $h$ was between $-90$ millims. (sulphuric acid) and $-102$ millims. After this trial a column of anhydrous phosphoric acid of 60 centims. was placed between the tubulures H and H', and the gas was made to pass several times through this substance by the process described. The two experiments $\alpha$ and $\beta$ were then made, and the minimum of $h$ was $-108$ millims. and $-89$ millims. The six curves of this series are identical, so that it is impossible to admit the influence of moisture. Hence the drying by sulphuric acid is sufficient. In all the experiments the gas was not only dried during its introduction into the reservoirs, but also afterwards by the process described in the present paragraph.

Series VII. (September 1867).

Dry hydrogen. Metal reservoir B.

\[ p_1 = 3.80 \text{ atmospheres}, \quad p_2 = 0.22 \text{ atmosphere}. \]

Temperature between $20^\circ.7$ and $21^\circ.2$.

<table>
<thead>
<tr>
<th>$\theta$</th>
<th>$t$</th>
<th>$h_1$</th>
<th>$h_2$</th>
<th>$h_3$</th>
<th>$h_4$</th>
<th>$h_5$</th>
<th>$h_6$</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\theta = 0.12$</td>
<td>$t = 2.2$</td>
<td>$h = -22^\text{mm}$</td>
<td>$-102$</td>
<td>$-45$</td>
<td>$-3$</td>
<td>$+12$</td>
<td>$+7$</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>B</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\theta = 0.12$</td>
<td></td>
<td>$h_2 = -25$</td>
<td>$-100$</td>
<td>$-25$</td>
<td>$-4$</td>
<td>$+6$</td>
<td>$+9$</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>C</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\theta = 0.41$</td>
<td></td>
<td>$h_2 = 4.5$</td>
<td>$+6$</td>
<td>$+9$</td>
<td>$+6$</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>D</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\theta = 0.57$</td>
<td></td>
<td>$h_2 = 2.8$</td>
<td>$+10$</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>
</tr>
<tr>
<td>$\alpha$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\theta = 0.23$</td>
<td></td>
<td>$h_2 = 2.4$</td>
<td>$+10$</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>
</tr>
<tr>
<td>$\beta$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\theta = 0.91$</td>
<td></td>
<td>$h_2 = 2.9$</td>
<td>$+18$</td>
<td>$+22$</td>
<td>$+18$</td>
<td>$+6$</td>
<td></td>
</tr>
</tbody>
</table>
§ X. Influence of the dimensions and nature of the reservoirs.

This influence is ascertained by the comparison of experiments in which for the reservoir B either the cylindrical zinc vessel, containing 33·805 litres, or the glass globe of 60·617 litres was taken and the gas in reservoir A compressed under a constant pressure $p_1$. Thus we had in both cases the same mass of gas expanding from the same pressure $p_1$ to the same final pressure $p'$. But the mass of gas contained in reservoir B was different, as well as the pressure $p_2$. By increasing the capacity of the reservoir B the pressure $p_2$ was increased, and also the mass of gas compressed during the expansion. Let $\mu$ be the mass of the unit of volume of the gas under the pressure $p'$, and $v$ the increase of volume of the reservoir B; the mass of compressed gas was increased by $\mu v$, whatever the pressure $p_1$; it is the mass of 26·812 litres of gas at a pressure but little different from that of the atmosphere.

Series VIII. (November 1867).

Dry hydrogen. Metal reservoir B.

$p_1 = 3·80$ atmospheres, $p_2 = 0·24$ atmosphere. Temperature between $7^\circ$ and $11^\circ$.

<table>
<thead>
<tr>
<th>$\theta$</th>
<th>$h$</th>
<th>$t$</th>
<th>$\theta$</th>
<th>$h$</th>
<th>$t$</th>
<th>$\theta$</th>
<th>$h$</th>
<th>$t$</th>
<th>$\theta$</th>
<th>$h$</th>
</tr>
</thead>
<tbody>
<tr>
<td>0·2</td>
<td>27</td>
<td>1·9</td>
<td>0·2</td>
<td>27</td>
<td>1·9</td>
<td>0·2</td>
<td>27</td>
<td>1·9</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>1·3</td>
<td></td>
<td></td>
<td>...</td>
<td></td>
<td></td>
<td>34</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>60</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Series II., previously mentioned, and Series VIII. afford an example of comparison. The coordinates of the minimum are $h = -124$ millims., $t = 2$ seconds (Series VIII., zinc reservoir), and $h = -67$, $t = 2$ seconds (Series II., glass reservoir). We have traced in fig. 6 the line $X'X'$ which expresses the Series II., and the line $XX$ which expresses the Series VIII.

The first meets the axis of the abscissa two seconds later than the second; and the maximum of $h$ in the first has scarcely more than one-third of the value it has in the second.
The same result is obtained by comparing Series IV. and I. (air). The coordinates of the minimum are \( h = -35 \) millims. (sulphuric acid) in Series IV. (glass), and \( h = -136 \) millims. (sulphuric acid) in Series I. (zinc). Moreover the abscissa of the point \( h = 0 \) is greater, and the curve does not rise so far above the abscissa in Series IV. as in the other.

The experiment shows thus that the cause of the depression is intimately connected with the quantity of gas which the jet encounters during the expansion. We must attribute this depression to a mechanical or thermic effect counteracted by the gas contained in reservoir B. Let us examine how this can take place.

By distinguishing three parts in the gas in motion as we have done before, we see, first, that the quantity of gas left in reservoir A is the same in each pair of experiments, since the expansion commences at the same pressure \( p_1 \) and finishes at the same pressure \( p' \). The quantity of gas which passes into reservoir B is also the same, since the reservoir A always contains the gas compressed under the pressure \( p_1 \). The third part, that which is contained in reservoir B, alone changes; it increases with its capacity. The law of expansion of the first part remains the same; the gas expands by overcoming a pressure equal at each moment to its elastic force. But it is not the same for the other two parts. The second always expands, it is true, from the pressure \( p_1 \) to the pressure \( p' \), but by overcoming a pressure less than its elastic force and varying from \( p_2 \) to \( p' \); and we have seen that \( p_2 \) increases with the capacity of B. The resistance to the flow being smaller than the elastic force of the portion of gas considered, its molecules acquire certain velocities; and evidently these velocities are greater the less the counterpressure \( p_2 \). This is a difference of mechanical effect about which we can have no doubt.

The state of motion of the second part can only last a certain time; little by little the molecules lose their velocities, producing heat, so that the mechanical effect finally transforms itself into a thermal effect. Such a condition must contribute to the depression observed in experiments where the value of \( \Theta \) is very small. We are thus brought to recognize one of the causes of this depression. The greater the velocity of the efflux the greater the depression; hence it is due, at least in part, to the fact that the gaseous molecules do not instantaneously lose their velocities.

The character of the third part remains to be studied. It is compressed from \( p_2 \) to \( p' \), and consequently its temperature is raised. The compression and rise in temperature, on the one hand, are less when \( p_2 \) increases; but, on the other, the quantity of gas which constitutes the third part increases more rapidly.
M. Achille Cazin on Internal Work in Gases. 201

than $p_2$; hence it may happen that the quantity of heat created in this part during the expansion increases with the volume of reservoir B. We may prove that it is so by the following reasoning.

The gas of the second part absorbed the work of expansion of the first part, and transmitted to the third a less quantity of work, which served for the compression; the velocities acquired by the molecules of this second part correspond to the difference. In each pair of experiments the work absorbed remains the same, but the velocities change; when they diminish, the work transmitted increases, and with it the quantity of heat which it causes to appear.

The creation of this heat is evidently opposed to the depression, and the greater it is the smaller the depression; this is what occurred in our experiments.

When I shall attempt subsequently to give a complete explanation of the phenomena observed, I shall examine all the causes which may intervene; and we shall then see whether to the cause of depression which has just been noted no other cause is joined. Some new observations will have prepared for this research.

The experiments mentioned in this paragraph teach us, again, that with a glass reservoir the abscissa of the point $h=0$ is greater, and the maximum of $h$ smaller, than with a zinc reservoir; whence it follows that the curve does not rise so quickly with the first reservoir as with the second. We shall find the explanation of this in the calorific action of the sides.

If the sides were impermeable to heat, the curve of the $h$’s ought to remain below the axis of the abscissae, unless it be admitted that a gaseous mass in which some exchanges of motion and heat take place, without any external positive or negative work, can of itself create heat and become spontaneously heated. This supposition being very improbable, we must have recourse to the thermal action of the sides.

The sides of the reservoir A are cooled during the expansion, they then give up heat to the gaseous mass. On the contrary, the sides of reservoir B are heated; they withdraw heat from the gas. The quantities of heat respectively yielded and withdrawn are not equal. If they were equal, the effect would be the same as if the sides were impermeable. Since there is a maximum of $h$, it is because a certain quantity of heat has been supplied finally from without. Let us see how this can have taken place. The fall of temperature in A is greater than the rise in B; but, on the other hand, the surface of the reservoir A is smaller than the other. Assuming the same emissive power for the sides of both, we cannot foresee in what direction the inequality will take place. Let us suppose that the heat given up

by the side \( \sin A \) is greater than that withdrawn by the side \( \sin B \); the exchanges of motion and heat will be accomplished in a space which receives heat at the same time, and the temperature will end by being higher than the initial temperature. From this moment the gaseous mass will slowly return to the exterior temperature, losing exactly the heat which it had gained. It is sufficient for this effect to be possible that the heat should spread more rapidly in the gas than in the sides. Since such is the effect observed, we are able to say from experiment that the heat given up by the sides in \( A \) exceeds the heat withdrawn in \( B \).

Let us now examine what results from a modification of the reservoir \( B \).

The reservoir \( A \) being the same, the heat given up to the gas which is there does not change; but things are different in reservoir \( B \). The gas which at first filled it under the pressure \( p_2 \) has been compressed, supporting an exterior pressure nearly equal to its elastic force, and it has passed from the pressure \( p_2 \) to the final pressure \( p' \). By increasing the capacity of reservoir \( B \) we have increased \( p_2 \), so that the rise of temperature of this part of the gas has diminished. But we have increased the mass and the surface of this part, and by substituting glass for zinc we have increased the absorbing-power of the sides. Hence it may happen that the heat withdrawn from this part is increased a little. As to the gas which passes from \( A \) to \( B \), the changes which it undergoes during the passage are very complex. We know, from an experiment made by Gay-Lussac, that the temperature is different every moment at the various points of a rarefied reservoir into which a jet of gas rushes: the temperature falls in the layers near the orifice; in those more distant it rises; moreover the magnitude of these effects varies progressively with the time. Hence there are in our experiment contrary thermal actions between the sides and the jet of gas. Certain parts of the jet take heat from the sides; others give up heat to them. But this complexity only occurs during the efflux, which is of very short duration; in my experiments it was less than 0.1 second; and when the efflux stops, the mean temperature of the part considered differs less from that of the sides than does that of the other two parts. Hence the direction of the action of the sides depends especially on the condition of those two parts.

If the heat withdrawn from the gas is less in the zinc reservoir than in the glass one, the excess of heat gained in reservoir \( A \) over the heat withdrawn is greater, and consequently the curve of the \( h' \)'s rises more with the first than with the second; this is the effect observed. Thus the difference of the maximum of \( h \) is explained by the cooling action of the glass, which
is greater than that of zinc. As the heat finally taken from the sides by the total mass of the gas is less with glass, we also see why the curve of the h's rises less quickly in this case than in the other. It is evident that, in order to know exactly the influence of the nature of the sides, we ought to operate with reservoirs of the same form and size; but the foregoing is sufficient to prove the small importance of this research, and I have not considered it necessary to undertake it.

§ XI. Influence of the initial pressure of the compressed gas.

When the pressure $p_1$ is changed, and consequently the pressure $p_2$, since the final pressure $p'$ does not change, the curve of the h's is greatly modified. The following series lead to the law which governs these modifications. I shall first mention two series performed on carbonic acid, which present a very complete example.

Series IX. (November 1867).

Dry carbonic acid. Metal reservoir B.

$p_1=3\cdot 80$ atmospheres, $p_2=0\cdot 22$ atmosphere. Temperature between 5° and 8°.

<table>
<thead>
<tr>
<th>A</th>
<th>$\theta=0^\circ 13$</th>
<th>$t=2\cdot 7$</th>
<th>4</th>
<th>7</th>
<th>10</th>
<th>12</th>
<th>2</th>
<th>17</th>
<th>27</th>
<th>34</th>
<th>...</th>
<th>120</th>
</tr>
</thead>
<tbody>
<tr>
<td>$h=+221^{\text{mm}}$</td>
<td>-133</td>
<td>-80</td>
<td>-28</td>
<td>+3</td>
<td>+15</td>
<td>+21</td>
<td>+13</td>
<td>+11</td>
<td>...</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>B</td>
<td>$\theta=0\cdot 26$</td>
<td>2</td>
<td>9</td>
<td>5</td>
<td>7</td>
<td>8</td>
<td>1</td>
<td>10</td>
<td>4</td>
<td>13</td>
<td>1</td>
<td>17</td>
</tr>
<tr>
<td>$h=+53$</td>
<td>-112</td>
<td>-61</td>
<td>-29</td>
<td>-8</td>
<td>+7</td>
<td>+17</td>
<td>+19</td>
<td>+17</td>
<td>+12</td>
<td>+8</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>C</td>
<td>$\theta=1\cdot 4$</td>
<td>3</td>
<td>8</td>
<td>6</td>
<td>8</td>
<td>9</td>
<td>8</td>
<td>9</td>
<td>10</td>
<td>6</td>
<td>13</td>
<td>0</td>
</tr>
<tr>
<td>$h=-8$</td>
<td>-136</td>
<td>-83</td>
<td>-50</td>
<td>-31</td>
<td>-10</td>
<td>+5</td>
<td>+15</td>
<td>+18</td>
<td>+15</td>
<td>+10</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>D</td>
<td>$\theta=4\cdot 2$</td>
<td>5</td>
<td>8</td>
<td>...</td>
<td>10</td>
<td>3</td>
<td>13</td>
<td>5</td>
<td>18</td>
<td>6</td>
<td>3</td>
<td>25</td>
</tr>
<tr>
<td>$h=-18$</td>
<td>-35</td>
<td>...</td>
<td>+2</td>
<td>+17</td>
<td>+25</td>
<td>...</td>
<td>+22</td>
<td>+17</td>
<td>+12</td>
<td>...</td>
<td>...</td>
<td></td>
</tr>
<tr>
<td>E</td>
<td>$\theta=4\cdot 8$</td>
<td>...</td>
<td>7</td>
<td>1</td>
<td>...</td>
<td>...</td>
<td>12</td>
<td>18</td>
<td>6</td>
<td>...</td>
<td>27</td>
<td>41</td>
</tr>
<tr>
<td>$h=+20$</td>
<td>...</td>
<td>-14</td>
<td>...</td>
<td>+14</td>
<td>+30</td>
<td>...</td>
<td>+24</td>
<td>+19</td>
<td>+14</td>
<td>...</td>
<td>...</td>
<td></td>
</tr>
<tr>
<td>F</td>
<td>$\theta=5$</td>
<td>...</td>
<td>6</td>
<td>6</td>
<td>...</td>
<td>11</td>
<td>14</td>
<td>2</td>
<td>17</td>
<td>21</td>
<td>24</td>
<td>1</td>
</tr>
<tr>
<td>$h=-3$</td>
<td>...</td>
<td>-24</td>
<td>...</td>
<td>+5</td>
<td>+20</td>
<td>+25</td>
<td>+28</td>
<td>+25</td>
<td>+20</td>
<td>+13</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>G</td>
<td>$\theta=5\cdot 1$</td>
<td>...</td>
<td>6</td>
<td>7</td>
<td>...</td>
<td>11</td>
<td>2</td>
<td>17</td>
<td>5</td>
<td>24</td>
<td>9</td>
<td>2</td>
</tr>
<tr>
<td>$h=+10$</td>
<td>...</td>
<td>-13</td>
<td>...</td>
<td>+10</td>
<td>...</td>
<td>+30</td>
<td>+25</td>
<td>+20</td>
<td>+15</td>
<td>+10</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>H</td>
<td>$\theta=5\cdot 2$</td>
<td>...</td>
<td>7</td>
<td>3</td>
<td>...</td>
<td>12</td>
<td>2</td>
<td>17</td>
<td>2</td>
<td>21</td>
<td>9</td>
<td>...</td>
</tr>
<tr>
<td>$h=+2$</td>
<td>...</td>
<td>-18</td>
<td>...</td>
<td>+13</td>
<td>...</td>
<td>+24</td>
<td>+20</td>
<td>...</td>
<td>...</td>
<td>+13</td>
<td>0</td>
<td></td>
</tr>
</tbody>
</table>

The curves of this series have been traced in fig. 7; and the line X X was obtained by following the general rule given in § IV. It represents the actual variable pressure deduced from the observed variation of the manometric level. It cuts the axis of the abscissee in the point $x=9$ seconds; and its maximum corresponds to $y=+30$ millims., $x=18$ seconds. The greatest depression of h, for the curve C, occurs where $h=-136$ millims. (sulphuric acid).

We must notice the first curve, A, which remains below X X. It represents an experiment in which the level of the manometer was very low at the moment the valve was opened. We see that
at the end of 0·13 second the actual pressure was less than the final pressure \( p' \) by a quantity smaller than 221 millims. (sulphuric acid).

From curve B, at the end of 1·4 second, the difference was greater than 136 millims. The curve XX satisfies these various conditions.

Series X. (December 1867).

Dry carbonic acid. Metal reservoir B.

\( p_1 = 2·42 \) atmospheres, \( p_2 = 0·60 \) atmosphere. Temperature between 5° and 8°.

<table>
<thead>
<tr>
<th>( \theta = 0°11 )</th>
<th>( t = 2°3 )</th>
<th>5·1</th>
<th>8·4</th>
<th>12·3</th>
<th>17·4</th>
<th>22</th>
<th>27</th>
<th>33</th>
<th>120</th>
</tr>
</thead>
<tbody>
<tr>
<td>( h = -23 \text{mm} )</td>
<td>-43</td>
<td>-10</td>
<td>+10</td>
<td>+25</td>
<td>+28</td>
<td>+25</td>
<td>+20</td>
<td>+15</td>
<td>0</td>
</tr>
<tr>
<td>( \theta = 0°12 )</td>
<td>2·6</td>
<td>5·3</td>
<td>7·9</td>
<td>10·3</td>
<td>16·5</td>
<td>20</td>
<td>26</td>
<td>31</td>
<td>120</td>
</tr>
<tr>
<td>( h = +5 )</td>
<td>-32</td>
<td>-8</td>
<td>+12</td>
<td>+22</td>
<td>+30</td>
<td>+27</td>
<td>+22</td>
<td>+18</td>
<td>0</td>
</tr>
<tr>
<td>( \theta = 1\cdot2 )</td>
<td>4·2</td>
<td>7</td>
<td>9·9</td>
<td>11·8</td>
<td>14</td>
<td>17·5</td>
<td>23</td>
<td>35</td>
<td>120</td>
</tr>
<tr>
<td>( h = -3 )</td>
<td>-25</td>
<td>-5</td>
<td>+15</td>
<td>+25</td>
<td>+30</td>
<td>+32</td>
<td>+28</td>
<td>+19</td>
<td>0</td>
</tr>
<tr>
<td>( \theta = 1°9 )</td>
<td>3·2</td>
<td>6·9</td>
<td>9·8</td>
<td>14·2</td>
<td>...</td>
<td>25·3</td>
<td>32</td>
<td>120</td>
<td></td>
</tr>
<tr>
<td>( h = -24 )</td>
<td>-26</td>
<td>0</td>
<td>+20</td>
<td>...</td>
<td>+31</td>
<td>...</td>
<td>+25</td>
<td>+20</td>
<td>0</td>
</tr>
<tr>
<td>( \theta = 3°8 )</td>
<td>5</td>
<td>7·5</td>
<td>11</td>
<td>...</td>
<td>17·8</td>
<td>24</td>
<td>31</td>
<td>120</td>
<td></td>
</tr>
<tr>
<td>( h = -5 )</td>
<td>-10</td>
<td>0</td>
<td>+20</td>
<td>...</td>
<td>+29</td>
<td>+25</td>
<td>+20</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>( \theta = 3°8 )</td>
<td>5·2</td>
<td>7·8</td>
<td>12</td>
<td>...</td>
<td>19</td>
<td>25</td>
<td>34</td>
<td>120</td>
<td></td>
</tr>
<tr>
<td>( h = -4 )</td>
<td>-9</td>
<td>+1</td>
<td>+20</td>
<td>...</td>
<td>...</td>
<td>+26</td>
<td>+29</td>
<td>+21</td>
<td>0</td>
</tr>
<tr>
<td>( \theta = 4°8 )</td>
<td>7·2</td>
<td>...</td>
<td>13</td>
<td>18·2</td>
<td>26</td>
<td>31</td>
<td>37</td>
<td>120</td>
<td></td>
</tr>
<tr>
<td>( h = -5 )</td>
<td>+3</td>
<td>...</td>
<td>+20</td>
<td>...</td>
<td>...</td>
<td>+28</td>
<td>+33</td>
<td>+28</td>
<td>0</td>
</tr>
<tr>
<td>( \theta = 5°4 )</td>
<td>7·7</td>
<td>10·7</td>
<td>15</td>
<td>19·6</td>
<td>30</td>
<td>42</td>
<td>46</td>
<td>120</td>
<td></td>
</tr>
<tr>
<td>( h = -5 )</td>
<td>+5</td>
<td>...</td>
<td>+20</td>
<td>+30</td>
<td>+33</td>
<td>+25</td>
<td>+15</td>
<td>+12</td>
<td>0</td>
</tr>
<tr>
<td>( \theta = 7°4 )</td>
<td>...</td>
<td>9·4</td>
<td>15</td>
<td>...</td>
<td>20</td>
<td>31</td>
<td>44</td>
<td>47</td>
<td>120</td>
</tr>
<tr>
<td>( h = -9 )</td>
<td>...</td>
<td>+8</td>
<td>+28</td>
<td>...</td>
<td>+31</td>
<td>+23</td>
<td>+13</td>
<td>+11</td>
<td>0</td>
</tr>
</tbody>
</table>

The results of this series are collected in fig. 7, where the line X'X' of the real pressure is traced in order to compare it with the preceding. It cuts the axis of the abscissae at the point \( x = 6 \) seconds; and its maximum is approximately at the point \( y = 30 \) millims., \( x = 18 \) seconds. The greatest depression of \( h \) occurs when for the first curve \( h = -43 \) (sulphuric acid). Let us first remark that the maximum is nearly at the same place in the two series, so that the elevation of the line XX above the axis of the abscissae cannot be attributed to the motion of the gaseous mass, since in Series X. the excess of pressure \( p_1 - p_2 \) is half that of Series IX. This confirms the opinion advanced in the preceding Section; for the fall of temperature in reservoir A diminishes with \( p_1 \), and consequently the heat given off by the sides also diminishes. The heating also diminishes in reservoir B, and with it the heat withdrawn by the sides: as in Series IX. the quantity of gas contained in reservoir B is greater than in the other series, the heat withdrawn by reservoir B may have diminished as much as the heat given up by reservoir A; whence
it would follow that there would be nearly the same excess of heat given off by the sides.

As to the depression, it seems to diminish more rapidly than the pressure $p_1$, which also confirms the opinion advanced with respect to one of its causes. By diminishing $p_1$ we diminish the velocities acquired during the expansion, and with them the temporary diminution of pressure which the gas undergoes.

The same results have been obtained by the comparison of several other series observed at various times and under different arrangements. I shall mention a few of them.

Series XI. (September 1867).

Dry hydrogen. Metal reservoir B.

$p_1 = 2.70$ atmospheres, $p_2 = 0.52$ atmosphere. Temperature $20^\circ$.

<table>
<thead>
<tr>
<th>$t$</th>
<th>$\theta = 0.10$</th>
<th>$h = -6$</th>
<th>$\theta = 0.19$</th>
<th>$h = +3$</th>
<th>$\theta = 0.20$</th>
<th>$h = -2$</th>
<th>$\theta = 0.34$</th>
<th>$h = -5$</th>
<th>$\theta = 0.07$</th>
<th>$h = -4$</th>
</tr>
</thead>
<tbody>
<tr>
<td>2.1</td>
<td>5.6</td>
<td>8.4</td>
<td>11.3</td>
<td>16.2</td>
<td>25</td>
<td>...</td>
<td>...</td>
<td>0</td>
<td>...</td>
<td>0</td>
</tr>
</tbody>
</table>

This series must be compared with Series VII., in which the excess of pressure was nearly double; the maximum of depression was also nearly double; finally the line XX cuts the x-axis nearer the origin in the series where the value of $p_1$ is the least.

I shall mention also a series on air, with the glass reservoir and a water-manometer with a spherical enlargement; we shall be able to compare it with Series IV. performed under the same conditions, but with an excess of pressure $p_1 - p_2$ nearly double.

Series XII. (May 1867).

Dry air. Glass reservoir B. Water-manometer with a spherical enlargement.

$p_1 = 2.76$ atmospheres, $p_2 = 0.73$ atmosphere. Temperature $14^\circ$.

<table>
<thead>
<tr>
<th>$t$</th>
<th>$\theta = 0.12$</th>
<th>$h = 0$</th>
<th>$\theta = 0.12$</th>
<th>$h = +9$</th>
<th>$\theta = 0.16$</th>
<th>$h = +11$</th>
<th>$\theta = 0.20$</th>
<th>$h = +16$</th>
</tr>
</thead>
<tbody>
<tr>
<td>...</td>
<td>4.6</td>
<td>-19</td>
<td>4.6</td>
<td>-23</td>
<td>5.2</td>
<td>-14</td>
<td>5</td>
<td>-8</td>
</tr>
</tbody>
</table>
The maximum depression is about one-third of that in Series IV., and the curves rise considerably above the axis of the abscissae. We must remark that the curves of this latter series affect the form $a'd'b'n'c'$ (fig. 1), although the water-manometer communicated with the tubulure H of the rarefied reservoir. This denotes friction in the tube which joined the two reservoirs. Without this disturbing influence the depression would have been greater in Series XII. In mentioning these experiments (which were made at the outset of this research) I wished to convey some notion of the difficulties which present themselves in researches of this kind, and of the progressive course which has led me to adopt the present apparatus, the results of which present no uncertainty.

§ XII. Influence of the nature of the gas.

We are now going to compare with one another Series I. (air), IX. (carbonic acid), and VIII. (hydrogen). All these series were made with the apparatus ultimately adopted, and under the same circumstances. Their curves are traced in figs. 4, 7, 6. The curves $XX$ present considerable differences, which we will examine.

Let us consider the gases in the following order—hydrogen, air, and carbonic acid. We see from figs. 6, 4, and 7 that the maximum of $h$ is nearly the same for the three gases; but it is reached more or less rapidly, so that the abscissae of this maximum go on increasing; the abscissae of the point where the line $XX$ cuts the axis of the $x's$ also increase; finally this line inclines more and more to the axis of the $x's$. I conclude from these observations that the point $g$ (fig. 5) is lowest for hydrogen and highest for carbonic acid.

In fact, let us suppose the point $g$ to be at the same height for hydrogen and carbonic acid, and that the valve is opened at the end of the time $of$ (fig. 5) which corresponds to the point $g$. Whilst the level of the manometer descends, the more rapidly the pressure of the gas increases, the more quickly will the minimum $b$ be reached, but the less will it have receded from the axis of the $x's$. The ordinate $kb$ would thus be smaller for hydrogen than for carbonic acid, which is contrary to observation.

The point $g$ cannot have the same abscissa for the three gases; this abscissa must increase from hydrogen to sulphuric acid, in the same way as the abscissa $ok$ of the point $b$.

Thus when the stopcock which separates the two reservoirs is opened, equality of pressure is established soonest with hydrogen, latest with carbonic acid; and, moreover, the instant this equality of pressure begins to take place, the pressure is smallest for hydrogen, and greatest for carbonic acid.
If these effects be compared with what is known of the velocity of the efflux of these gases, it will be found that the greater the velocity the smaller the abscissa of; hence the idea of a relation of cause and effect between this velocity and the depression observed in my experiments. We have thus a confirmation of the opinion advanced in Sections X. and XI. The depression is due to the velocities acquired during the expansion. If it is due to this cause alone, it must disappear simultaneously with the velocities; and the prompter the extinction of these velocities, the closer the point \( h \) will be to the origin of coordinates, \( o \).

The mobility of hydrogen is certainly greater than that of carbonic acid. The molecules of hydrogen are to those of carbonic acid as a very elastic ball is to one which is but slightly so. Consequently the agitation, the whirlings, which gradually destroy the motion in reservoir \( B \), must continue longer with hydrogen than with other gases. On the other hand, the velocities acquired at the moment when the expansion ceases are greater for the former gas. This tends to increase the duration of the agitation. Since, according to my experiments, the duration of the depression is, on the contrary, for this gas less than half of that for carbonic acid, it must be admitted that some other cause influences this effect.

We cannot find it in the action of the sides, because the line \( X X \) rises less above the axis of the abscissae for hydrogen than for the other two gases; so that the excess of heat given out over that withdrawn by the sides is the least for hydrogen. Now a diminution of this excess can only cause the point \( h \) to recede.

But this effect is explained very well by a spontaneous cooling which is greater for carbonic acid than for air, and independent of the velocities acquired during the expansion, because the sides must take so much more time to let the cooling disappear the more considerable it is. Hence the question is resolved in the same way as that which has occupied Messrs. Joule and Thomson; but the method which I have employed enables us to distinguish the influence of each of the circumstances which play a part in the phenomenon; and it is that which must be done when it is impossible to eliminate them. I think the same circumstances must present themselves in the experiments of the English physicists, and that their method, more simple in appearance than mine, cannot give exact quantities.

The experiments which I have described do not prove that there is a spontaneous cooling in hydrogen; but they demonstrate that there is in air and carbonic acid. As to the numerical values, they can only be obtained approximately, as we shall presently see.

We have now to examine the differences which the parts of the
curve XX above the axis of the abscissæ present. They may be due to several causes—such as the difference of temperature acquired during the expansion by the various parts of the gaseous mass, the difference of the specific heats, that of the conductivity, and, finally, that of the emissive and absorbent powers.

Let us first compare hydrogen and air. Both must undergo almost the same thermometric effects; for we have approximately, for the gas expanded in A and the gas compressed in B, the usual formula

\[ 1 + \alpha t = \left( \frac{p}{p_0} \right)^{K-1}, \]

with

\[ K = 1.41. \]

Besides, these two gases have the same specific heat for a constant volume, and also the same emissive or absorbing-power; but they have not the same conductivity. That of hydrogen is the greatest. Hence the less the conductivity the higher the curve XX rises. What can be the effect of conductivity? The variations of temperature due to the action of the sides will be more rapid for hydrogen than for air. If the temperature of the total mass is greater than the temperature of the sides when the mixture is effected, it will descend more rapidly; hence the maximum of \( h \) must be less for hydrogen, and the curve must reach the axis of the abscissæ more quickly.

Let us now compare air and carbonic acid. The thermometric effects are less for the latter, and the specific heat is greater; it is the same with regard to its emissive or absorbent-power. As to the conductivity, it cannot differ much. The first cause acts in a direction contrary to the other two: it tends to diminish the quantities of heat taken up or given off by the sides, and consequently to lower the maximum of the curve XX; the other two causes tend to raise it. There may be compensation; and we cannot foresee the direction of the resulting effect. But experiment has given the same maximum, so that we can admit the compensation.

These various considerations appear sufficient to enable me to give a complete explanation of all the particular points which my experiments present.

We can calculate approximately the temperatures which correspond to the various stages of an experiment.

The mixture is completely effected at the moment the curve XX reaches its maximum; the greatest value observed is 36 millims. of sulphuric acid, which represents 4 millims. of mercury. The excess of temperature of the gaseous mass being \( \delta t \), we have, for \( t = 10^{\circ}, p = 760, \delta p = 4, \)
\[
\frac{1 + \alpha(t + \delta t)}{1 + at} = \frac{p + \delta p}{p},
\]
whence
\[
\delta t = 283 \frac{\delta p}{p} = 1.5.
\]

Until the gaseous mass has reached this state, its various parts are not of the same temperature, and consequently it is impossible to deduce from the pressure observed the thermometric state of the gas. We can only estimate what must have been the temperature of the whole mass for its pressure to be that which we observed at a certain period. For example, as \( h = 0 \) for 5 seconds with hydrogen, we may admit that all the velocities are extinguished after 5 seconds, and we then see in fig. 7 that \( h = -40 \) millims. with carbonic acid, equivalent to \( \delta p = -5 \) millims. of mercury. The preceding formula gives \( \delta t = -1.8 \). The excess of pressure \( p_1 - p \) was 2.85 atmospheres. This estimation is evidently uncertain, and can only give a notion of the order of magnitude of the effects produced.

I have made a series of experiments on protoxide of nitrogen with the glass reservoir. This series should be compared with Series II. (hydrogen).

### Series XIII. (August 1867).

Protoxide of nitrogen. Glass reservoir B.

\( p_1 = 3.95 \) atmospheres, \( p_2 = 0.49 \) atmosphere. Temperature between 20° and 23°.

<table>
<thead>
<tr>
<th>( \theta )</th>
<th>( t )</th>
<th>( x )</th>
<th>( x )</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.5</td>
<td>0.45</td>
<td>11.7</td>
<td>15.8</td>
</tr>
<tr>
<td>-1.8</td>
<td>5.9</td>
<td>-48</td>
<td>-35</td>
</tr>
<tr>
<td>0.45</td>
<td>6.3</td>
<td>10.6</td>
<td>13.9</td>
</tr>
<tr>
<td>-1</td>
<td>-44</td>
<td>-33</td>
<td>-23</td>
</tr>
<tr>
<td>0.55</td>
<td>6.9</td>
<td>12.2</td>
<td>17</td>
</tr>
<tr>
<td>1</td>
<td>-43</td>
<td>-26</td>
<td>-10</td>
</tr>
<tr>
<td>0.52</td>
<td>6.9</td>
<td>12.1</td>
<td>15.5</td>
</tr>
<tr>
<td>0.55</td>
<td>6.9</td>
<td>12.1</td>
<td>15.5</td>
</tr>
<tr>
<td>0.64</td>
<td>7</td>
<td>11.1</td>
<td>16.7</td>
</tr>
<tr>
<td>1.9</td>
<td>-40</td>
<td>-29</td>
<td>-19</td>
</tr>
<tr>
<td>0.97</td>
<td>7.3</td>
<td>12.6</td>
<td>16.6</td>
</tr>
<tr>
<td>1.4</td>
<td>7.8</td>
<td>12.5</td>
<td>15.9</td>
</tr>
<tr>
<td>2.0</td>
<td>-30</td>
<td>-21</td>
<td>-10</td>
</tr>
<tr>
<td>1.4</td>
<td>9.1</td>
<td>14.2</td>
<td>19.1</td>
</tr>
<tr>
<td>1.4</td>
<td>9.1</td>
<td>12.5</td>
<td>17.2</td>
</tr>
<tr>
<td>6.3</td>
<td>-19</td>
<td>-10</td>
<td>0</td>
</tr>
</tbody>
</table>

The curves XX of Series II. (hydrogen) and XIII. (protoxide of nitrogen) are traced in fig. 8. They meet the axis of the abscissæ in the points \( x = 6 \) seconds and \( x = 16 \) seconds, or
thereabouts. Hence the difference is still more marked than in
the preceding series. We also see from the Table of Series II.
that the initial depression is greater for hydrogen—which con-
firms a conclusion already given, because for this gas the velocities
of efflux are greatest. It is certainly convenient to make reser-
voir B of greater capacity than the zinc reservoir; but the neces-
sity of keeping up a current of water around it makes me prefer
the latter in final experiments.

§ XIII. Summary and conclusions of the experiments.

All the observations above given may be summed up in the
following statement:—

When a reservoir containing compressed gas is placed in com-
unication, by a large orifice, with a reservoir containing the
same gas rarefied, equality of pressure establishes itself very
quickly in the two reservoirs (0·1 second). When equality of
pressure commences, the velocities acquired have not completely
disappeared. At this instant there is a mechanical work pro-
duced in the first reservoir and a certain quantity of heat has
disappeared. There is, on the contrary, mechanical work ex-
pended in compression and heat created in the second reservoir;
but a part only of the vires vitæ produced during the efflux has
been transformed into heat. Whilst the velocities acquired con-
tinue to disappear in creating more heat, the pressure increases.
When all agitation has ceased, the heat which has disappeared
from the gas of the first reservoir remains greater than that which
has been created in the gas of the second reservoir. If the sides
were impermeable to heat, equilibrium of temperature would re-
establish itself by exchanges of heat between the cold and hot
parts, and the final temperature would be less than the initial.
As the sides are not impermeable to heat, those of the first reser-
voir give heat, those of the second take heat from the gas, and
finally there is an introduction of exterior heat, so that the ex-
changes between the cold and hot parts bring about a tempera-
ture greater than the initial temperature.

One part of the heat introduced remains in the gas and re-
places the heat which has disappeared in the operation; the rest
is retaken by the sides during the very slow cooling which re-
establishes the initial temperature. Hence, during the entire
operation, there is at the same time, change of distribution of heat
between the various portions of the sides, and destruction of a
certain quantity of heat, which is so much greater the more the
gas diverges from the laws of Mariotte and Gay-Lussac. It is
this latter quantity which is converted into internal work in the
gas.

[To be continued.]
XXV. Remarks on a Paper by Dr. Sondhauss.

By the Hon. J. W. STRUTT, Fellow of Trinity College, Cambridge*

In Nos. 5 and 6 of Poggendorff's Annalen for this year there is a paper by Dr. Sondhauss "On the Tones of Heated Tubes and Aërial Vibrations in Pipes of various forms," in which are given formulæ of considerable generality embodying the results of original and other experiments. Many years ago† Dr. Sondhauss had investigated the influence of the size and form of flask- or bottle-shaped vessels on the pitch of the sounds produced when a stream of air is blown across their mouth, and had obtained as an empirical formula for flasks with rather long cylindrical necks,

\[ n = C \sqrt{\frac{\sigma}{SL}}, \]

where \( n \) is the number of vibrations per second, \( \sigma \) the area of the section of the neck whose length is \( L \), and \( S \) the volume of the body of the flask. \( C \) is a constant determined by the experiments. On the other hand, when \( L \) is very small compared with the diameter of the neck, which then becomes a mere hole,

\[ n = C \frac{\sigma^{\frac{1}{2}}}{S^{\frac{1}{2}}}. \]

In the paper now under discussion it is sought to fill up the gap, as it were, and the following formula is arrived at as applicable for all proportions of \( L \) and \( \sigma^{\frac{1}{2}} \),

\[ n = \frac{a}{4\sqrt{c}} \sqrt{\frac{\sigma}{(Sc + L\sigma)(L + \sqrt{\sigma})}}, \]

or, as I prefer to write it,

\[ n = \frac{a}{4\sqrt{c}} \sqrt{\frac{\sigma}{S + \frac{L\sigma}{c}}}, \]

in which \( a = \) velocity of sound. \( c \) is a constant, of which Dr. Sondhauss says that it relates to the change in the velocity of sound in closed spaces from which the sound-waves have only a restricted exit; and its value, as found from the experiments, is approximately 2.3247.

In (VII.), if \( \sigma \) be small,

\[ n = \frac{a}{4\sqrt{c}} \sqrt{\frac{\sigma}{LS}}. \]

If, on the contrary, \( L \) be very small,

\[ n = \frac{a}{4\sqrt{c}} \frac{\sigma^{\frac{1}{2}}}{S^{\frac{1}{2}}}. \]

* Communicated by the Author.
If in (VII.) we further put \( S = 0 \),

\[
n = \frac{a}{4} \sqrt{\frac{1}{L(L + \sqrt{\sigma})}} \quad \ldots \quad (X.)
\]

a result which Dr. Sondhauss applies to cylindrical tubes closed at one end. This being admitted, it readily follows that for a pipe open at both ends,

\[
n = \frac{a}{2} \sqrt{\frac{1}{L(L + 2\sqrt{\sigma})}} \quad \ldots \quad (XI.)
\]

An extension is next made to the case of more than one neck, but it will not be necessary for my purpose to repeat the formulæ. A few days before I saw Dr. Sondhauss’s work I had myself completed a paper on a similar subject, which has since been sent to the Royal Society. The formulæ there given were in the first instance obtained theoretically, though some of them were afterwards verified by a rather laborious series of experiments. But on the present occasion I shall leave the theory on one side, and wish only to discuss some differences between the results of Dr. Sondhauss and my own, regarded from an experimental point of view. The rational formula corresponding to (VII.) is

\[
n = \frac{a}{2\pi} \sqrt{\frac{\sigma}{S\left(L + \frac{\sqrt{\pi}}{2}\sqrt{\sigma}\right)}} \quad \ldots \quad (A)
\]

where, however, \( S \) has not quite the same meaning as with Dr. Sondhauss, but includes the volume of about half the neck, and is therefore nearly identical with the \( (S + \frac{L\sigma}{c}) \) of (VII.). On this understanding (VII.) may be written

\[
n = \frac{a}{6.0988} \frac{\sigma^{\frac{1}{2}}}{S^{\frac{1}{2}}\sqrt{L + \sigma^{\frac{1}{2}}}} \quad \ldots \quad (VII.)
\]

while (A) expressed in numbers is

\[
n = \frac{a}{6.2832} \frac{\sigma^{\frac{1}{2}}}{S^{\frac{1}{2}}\sqrt{L + 8.863\sqrt{\sigma}}} \quad \ldots \quad (A)
\]

If \( \sigma^{\frac{1}{2}} \) be very small against \( L \),

\[
n = \frac{a}{6.0988} \frac{\sigma^{\frac{1}{2}}}{S^{\frac{1}{2}}L^{\frac{1}{2}}} \quad \ldots \quad (VIII.)
\]

\[
n = \frac{a}{6.2832} \frac{\sigma^{\frac{1}{2}}}{S^{\frac{1}{2}}L^{\frac{1}{2}}} \quad \ldots \quad (B)
\]

But if \( L \) be very small,
The rational formula (C) was first given by Helmholtz in his admirable paper in Crelle, on Vibrations in open Pipes; it is only strictly applicable to openings of circular form. The difference between (A) and (VII.) is never very great, being on one side when \( L \) is small, and on the other when \( L \) is large, and accordingly vanishing for some intermediate value. The greatest difference is shown in (VIII.) and (B) when \( L \) is very large. I therefore consider Dr. Sondhauss's opinion and anticipation to be in the main justified by my investigation, when he says, "I remark that I regard the formula (VII.) . . . not merely as an empirical formula useful for interpolation, but am convinced that it forms the theoretical expression of a natural law. From the zeal with which the field of mathematical physics is now cultivated, we may expect that the laws which I have discovered experimentally will soon be proved by analysis." But I must observe that (A) is only true subject to a series of limitations, which Dr. Sondhauss seems scarcely, if at all, to have contemplated. All the dimensions of the vessel (with a partial exception of the length of the neck) must be small compared with the quarter wave-length, and the diameter of the neck must be small against the linear dimension of the body of the vessel. The latter condition excludes the case of \( S \) small or nothing, to which Dr. Sondhauss pushes the application of his formula. But there is a rational formula proper for closed cylindrical tubes, as has been proved by Helmholtz in his paper on open pipes, to which Dr. Sondhauss refers, but apparently without availing himself of the results. It runs,

\[
n = \frac{a}{4 \left( L + \frac{\sqrt{\pi}}{4} \sigma \right)} \cdot \cdot \cdot \cdot \cdot \cdot \cdot (D)
\]

but is only strictly true when \( \sigma \) is small against \( L \). Although I am of opinion that (A) and (D) and the transition between them cannot be comprehended in the same theoretical investigation, yet it is easy to adjust (A) so as algebraically to include (D). Thus

\[
n = \frac{a}{2\pi} \frac{\sigma}{\sqrt{\left( S + \frac{4}{\pi^2} \sigma L \right) \left( L + \frac{\sqrt{\pi}}{2} \sigma \right)}} \cdot \cdot \cdot (A)
\]

becomes, when \( S \) (which now refers to the volume of the body only) is put equal to zero,
The Hon. J. W. Strutt’s Remarks on

\[ n = \frac{a}{4} \frac{1}{\sqrt[4]{L + \frac{\sqrt{\pi}}{2} \rho}} \]

\[ = \frac{a}{4(L + \frac{\sqrt{\pi}}{4} \rho)^\frac{1}{4}} \] approx., . . . (D)

supposing the condition fulfilled as to the relative magnitudes of \( L \) and \( \rho^\frac{1}{4} \). Now this form of (A) is perfectly legitimate, the value of \( \frac{4}{\pi^\frac{1}{2}} \) being \( \cdot405 \). In the formula (VII.) \( \cdot405 \) is replaced by \( \frac{1}{c} \) or \( \cdot430 \).

But Dr. Sondhauss will naturally point to the comparison of (X.) with the experiments of Wertheim, which he justly regards as very satisfactory. I have examined the series of twenty-two experiments with cylindrical tubes of circular form closed at one end, and have calculated for comparison the results of (D). It will be seen from the annexed Table that, good as is the agreement of the observations with (X.), it is still better (on the whole) with (D). Indeed I must confess that the differences in some cases, where \( \rho^\frac{1}{4} \) is by no means small compared with \( L \), are much less than I should have expected.

<table>
<thead>
<tr>
<th>( a )</th>
<th>( L )</th>
<th>Diameter</th>
<th>( n ) observed by Wertheim.</th>
<th>( n ), calculated by Sondhauss from (X.).</th>
<th>( n ), calculated by me from (D.).</th>
</tr>
</thead>
<tbody>
<tr>
<td>3371</td>
<td>785</td>
<td>80</td>
<td>103.4</td>
<td>102.9</td>
<td>103.2</td>
</tr>
<tr>
<td>3395</td>
<td>500</td>
<td>80</td>
<td>158.0</td>
<td>158.3</td>
<td>159.7</td>
</tr>
<tr>
<td>3413</td>
<td>355</td>
<td>80</td>
<td>219.2</td>
<td>219.4</td>
<td>220.8</td>
</tr>
<tr>
<td>3371</td>
<td>280</td>
<td>80</td>
<td>272.4</td>
<td>268.8</td>
<td>270.6</td>
</tr>
<tr>
<td>3418</td>
<td>157</td>
<td>60</td>
<td>460.4</td>
<td>451.8</td>
<td>453.5</td>
</tr>
<tr>
<td>3413</td>
<td>1000</td>
<td>60</td>
<td>83.3</td>
<td>83.1</td>
<td>83.3</td>
</tr>
<tr>
<td>3418</td>
<td>640</td>
<td>60</td>
<td>128</td>
<td>128.3</td>
<td>128.8</td>
</tr>
<tr>
<td>3413</td>
<td>340</td>
<td>48</td>
<td>237</td>
<td>233.7</td>
<td>235</td>
</tr>
<tr>
<td>3413</td>
<td>1000</td>
<td>48</td>
<td>84</td>
<td>83.6</td>
<td>83.8</td>
</tr>
<tr>
<td>3427</td>
<td>350</td>
<td>48</td>
<td>234.4</td>
<td>231.2</td>
<td>232.3</td>
</tr>
<tr>
<td>3413</td>
<td>1000</td>
<td>25</td>
<td>238.3</td>
<td>237.4</td>
<td>238.1</td>
</tr>
<tr>
<td>3413</td>
<td>1000</td>
<td>20</td>
<td>239.7</td>
<td>238.8</td>
<td>239.3</td>
</tr>
<tr>
<td>3413</td>
<td>1000</td>
<td>11</td>
<td>241.5</td>
<td>241.5</td>
<td>241.8</td>
</tr>
<tr>
<td>3390</td>
<td>266</td>
<td>11</td>
<td>243.8</td>
<td>243.3</td>
<td>243.4</td>
</tr>
<tr>
<td>3390</td>
<td>266</td>
<td>5</td>
<td>243.8</td>
<td>243.4</td>
<td>243.4</td>
</tr>
<tr>
<td>3471</td>
<td>200</td>
<td>60</td>
<td>640</td>
<td>627.6</td>
<td>631.0</td>
</tr>
<tr>
<td>3471</td>
<td>116</td>
<td>68</td>
<td>372.1</td>
<td>361.8</td>
<td>364.2</td>
</tr>
<tr>
<td>3390</td>
<td>208</td>
<td>46.5</td>
<td>640</td>
<td>627.6</td>
<td>631.0</td>
</tr>
<tr>
<td>3390</td>
<td>208</td>
<td>43.5</td>
<td>376.5</td>
<td>374.2</td>
<td>376.4</td>
</tr>
<tr>
<td>3471</td>
<td>200</td>
<td>43.5</td>
<td>376.5</td>
<td>361.8</td>
<td>364.2</td>
</tr>
<tr>
<td>3471</td>
<td>116</td>
<td>200</td>
<td>154.2</td>
<td>152.6</td>
<td>153.5</td>
</tr>
<tr>
<td>3390</td>
<td>200</td>
<td>200</td>
<td>274.1</td>
<td>271.1</td>
<td>269.5</td>
</tr>
<tr>
<td>3390</td>
<td>200</td>
<td>5</td>
<td>416.9</td>
<td>455.0</td>
<td>433.4</td>
</tr>
<tr>
<td>3390</td>
<td>200</td>
<td>25</td>
<td>270.1</td>
<td>268.3</td>
<td>269.2</td>
</tr>
</tbody>
</table>

The foregoing Table shows that although (X.) represents Wertheim’s observations with considerable accuracy, yet, Helmholtz’s rational formula (D) is on all grounds to be preferred.
Dr. Sondhauss expresses himself strongly as to the difficulty which exists in determining accurately the pitch of the very uncertain sound produced by tubes whose diameter is not small compared with their length, an opinion which I entirely share. It is indeed difficult to understand how Wertheim obtained results of such precision. But I cannot agree with Dr. Sondhauss when he goes on to say that resonance is not a sure guide in determining accurately the pitch of a pipe; for it was by this method exclusively that the determinations recorded in my paper were made. I have there given at length my reasons for adopting it, and for doubting the results of the method of blowing, although such experiments as those of Wertheim go to show *à posteriori* that in his hands at least it was not unworthy of dependence.

Other experiments of Wertheim are calculated from formula (IX.) and show a tolerable agreement. The difference between (IX.) and Helmholtz’s theoretical formula (C) relates only to a constant multiplier, and corresponds to a difference of pitch of about a quarter of a semitone. The discordances are attributed (no doubt correctly) to the unsuitable form of some of the vessels, and consequent imperfect fulfilment of the theoretical condition to which (C) is subject.

We come next to vessels in the form of flasks with a cylindrical neck of sensible length. Dr. Sondhauss gives a Table containing the results of a comparison of (VII.) with some experiments of his own. The average discordance amounts to about a semitone. Although it was evident beforehand that in most cases the limitations on formula (A) were grossly violated, I thought it worth while to calculate in accordance with (A) the theoretical pitch, and have given the results in the form of a Table:

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1. 341260</td>
<td>Sphere.</td>
<td>17:1</td>
<td>60</td>
<td>5:5</td>
<td>241:6</td>
<td>246:9</td>
<td>251:3</td>
<td></td>
</tr>
<tr>
<td>2. 341260</td>
<td>Cylinder.</td>
<td>60:9</td>
<td>19</td>
<td>12:5</td>
<td>430:5</td>
<td>454:5</td>
<td>453:6</td>
<td></td>
</tr>
<tr>
<td>3. .........</td>
<td>Cylinder.</td>
<td>10:7</td>
<td>15</td>
<td>10</td>
<td>966:5</td>
<td>959</td>
<td>970</td>
<td></td>
</tr>
<tr>
<td>4. .........</td>
<td>......</td>
<td>97:7</td>
<td>13</td>
<td>9</td>
<td>287:3</td>
<td>311:2</td>
<td>309:2</td>
<td></td>
</tr>
<tr>
<td>5. .........</td>
<td>Cylinder.</td>
<td>66:2</td>
<td>175</td>
<td>11:6</td>
<td>143:7</td>
<td>155:2</td>
<td>158:8</td>
<td></td>
</tr>
<tr>
<td>6. .........</td>
<td>Cylinder.</td>
<td>117:8</td>
<td>183</td>
<td>18</td>
<td>170:9</td>
<td>170:4</td>
<td>178:5</td>
<td></td>
</tr>
<tr>
<td>7. .........</td>
<td>Octagon.</td>
<td>65:4</td>
<td>193</td>
<td>26:5</td>
<td>114</td>
<td>106:7</td>
<td>107:6</td>
<td></td>
</tr>
<tr>
<td>8. .........</td>
<td>Sphere.</td>
<td>76:3</td>
<td>118</td>
<td>20:5</td>
<td>256:9</td>
<td>285:6</td>
<td>306:1</td>
<td></td>
</tr>
<tr>
<td>9. .........</td>
<td>Cylinder.</td>
<td>117:8</td>
<td>15</td>
<td>25:3</td>
<td>574:7</td>
<td>589:5</td>
<td>600:2</td>
<td></td>
</tr>
<tr>
<td>10. .........</td>
<td>.....</td>
<td>132:4</td>
<td>44</td>
<td>27</td>
<td>362</td>
<td>429:2</td>
<td>441:4</td>
<td></td>
</tr>
<tr>
<td>11. 342740</td>
<td>Sphere.</td>
<td>923</td>
<td>205</td>
<td>25</td>
<td>85:4</td>
<td>82:3</td>
<td>83:7</td>
<td></td>
</tr>
<tr>
<td>12. 344210</td>
<td>Cylinder.</td>
<td>8920</td>
<td>30</td>
<td>36</td>
<td>76:1</td>
<td>76:6</td>
<td>76:6</td>
<td></td>
</tr>
<tr>
<td>13. 341260</td>
<td>Sphere.</td>
<td>178</td>
<td>160</td>
<td>18:5</td>
<td>152:2</td>
<td>155:8</td>
<td>159:1</td>
<td></td>
</tr>
<tr>
<td>14. 344210</td>
<td>Cylinder.</td>
<td>1-09</td>
<td>11</td>
<td>2</td>
<td>812:7</td>
<td>812:6</td>
<td>825:4</td>
<td></td>
</tr>
<tr>
<td>15. .........</td>
<td>Cylinder.</td>
<td>.80</td>
<td>2-2</td>
<td>2-2</td>
<td>1933</td>
<td>1925:2</td>
<td>1902</td>
<td></td>
</tr>
</tbody>
</table>

* The result of the formula (VII.) ought evidently here to be greater than that of (A). On a recalculation I find 85-8 instead of 83-2.
The Hon. J. W. Strutt's Remarks.

All the columns except the last are copied from Dr. Sondhauss's paper. It will be found that the observations are better represented by (VII.) than by (A); but it must be remembered that (VII.) contains an arbitrary constant, $c$, which acts nearly as a constant multiplier, although, if I understand Dr. Sondhauss aright, its value was not determined from this series of experiments. However this may be, it is certain that nearly all the values of $n$ calculated from (A) are too great. The fact is that (A) is scarcely applicable to the experiments at all. In only five cases is the ratio of the diameter of the neck to the dimension of the vessel even tolerably small. These are 1, 4, 11, 12, 14; but in 1, on account of the extremely small diameter of the neck and its considerable length, the influence of friction is probably sensible; and its effect would be to lower the pitch. The body in 4 is cylindrical, and perhaps too long in proportion to the quarter wave-length. In 11, 12, and 14 the agreement is sufficiently good. I consider accordingly that there is no evidence in the Table unfavourable to formula (A), supposed to be stated with the proper restrictions. In my own experiments, made by the method of resonance, I found a very good agreement between the directly observed and the calculated pitch, the average error being under a quarter of a semitone. Even with formula (VII.) as the basis of calculation there would be a fair agreement, certainly better than is the case with Dr. Sondhauss's own experiments. The difference between (VII.) and (A) is, as I have already remarked, comparatively small, and could only be certainly distinguished under favourable circumstances. Not finding the necessary data in Dr. Sondhauss's paper, I venture to quote some experiments from the paper on Resonance. There are seven observations in which the necks were sufficiently long to bring out the difference between the formulae, being more than four times the diameter. It will be seen that the alteration is in every case for the worse if the formula (VII.) is substituted for (A).

<table>
<thead>
<tr>
<th>$n$, observed.</th>
<th>$n$, calculated from (A).</th>
<th>$n$, calculated approx. from (VII.).</th>
</tr>
</thead>
<tbody>
<tr>
<td>126</td>
<td>127:7</td>
<td>131</td>
</tr>
<tr>
<td>108:7</td>
<td>107:7</td>
<td>110</td>
</tr>
<tr>
<td>189</td>
<td>179:7</td>
<td>184</td>
</tr>
<tr>
<td>228</td>
<td>233:7</td>
<td>239</td>
</tr>
<tr>
<td>204</td>
<td>201:9</td>
<td>207</td>
</tr>
<tr>
<td>182</td>
<td>186:3</td>
<td>190</td>
</tr>
<tr>
<td>384</td>
<td>391:6</td>
<td>400</td>
</tr>
</tbody>
</table>

These experiments seem to decide the question; but it would be interesting to see if Dr. Sondhauss obtained a similar result.
by the method of blowing. The difference, amounting in (VIII.) and (B) to half a semitone, is far greater than any error to be feared in the measurement of pitch or of the dimensions of the vessel, and ought therefore to give a sufficient handle to decide between the formulæ, if proper attention is given to the choice of a suitable resonator. In the foregoing remarks I have naturally dwelt most on my differences with Dr. Sondhaus; but I should be sorry to have it supposed that I write in a hostile spirit, or do not recognize the claims of one to whom the science of acoustics is so largely indebted.

Terling Place, Witham,
August 12, 1870.

Postscript, August 19.

I have since calculated the results of the experiments of Wertheim on pipes open at both ends, and find that in this case also the rational formulæ $n = \frac{a}{4(\frac{1}{2}L + \frac{\pi}{8}D)}$ agrees best with the observations:

<table>
<thead>
<tr>
<th>a.</th>
<th>L.</th>
<th>D.</th>
<th>$n$, observed by Wertheim</th>
<th>$n$, calculated by Sondhaus</th>
<th>$n$, calculated by myself</th>
</tr>
</thead>
<tbody>
<tr>
<td>3371</td>
<td>785</td>
<td>80</td>
<td>202.0</td>
<td>197.6</td>
<td>203.4</td>
</tr>
<tr>
<td>3395</td>
<td>500</td>
<td>...</td>
<td>306.9</td>
<td>299.6</td>
<td>301.5</td>
</tr>
<tr>
<td>3413</td>
<td>355</td>
<td>...</td>
<td>418.3</td>
<td>406.3</td>
<td>408.4</td>
</tr>
<tr>
<td>3371</td>
<td>280</td>
<td>...</td>
<td>512.0</td>
<td>490.4</td>
<td>491.7</td>
</tr>
<tr>
<td>3418</td>
<td>157</td>
<td>...</td>
<td>836.6</td>
<td>789.2</td>
<td>777.3</td>
</tr>
<tr>
<td>3413</td>
<td>1000</td>
<td>60</td>
<td>164.3</td>
<td>162.2</td>
<td>163.0</td>
</tr>
<tr>
<td>3418</td>
<td>640</td>
<td>...</td>
<td>251.0</td>
<td>247.3</td>
<td>248.8</td>
</tr>
<tr>
<td>...</td>
<td>340</td>
<td>...</td>
<td>450.7</td>
<td>438.8</td>
<td>441.5</td>
</tr>
<tr>
<td>3413</td>
<td>1000</td>
<td>48</td>
<td>165.6</td>
<td>163.8</td>
<td>164.5</td>
</tr>
<tr>
<td>3427</td>
<td>350</td>
<td>...</td>
<td>452.3</td>
<td>439.1</td>
<td>442.0</td>
</tr>
<tr>
<td>3413</td>
<td>1000</td>
<td>25</td>
<td>167.5</td>
<td>167.0</td>
<td>167.4</td>
</tr>
<tr>
<td>...</td>
<td>...</td>
<td>20</td>
<td>168.2</td>
<td>167.7</td>
<td>168.0</td>
</tr>
<tr>
<td>...</td>
<td>...</td>
<td>11</td>
<td>169.1</td>
<td>169.0</td>
<td>169.2</td>
</tr>
<tr>
<td>...</td>
<td>...</td>
<td>5</td>
<td>170.0</td>
<td>169.3</td>
<td>169.9</td>
</tr>
<tr>
<td>3390</td>
<td>906</td>
<td>30</td>
<td>182.5</td>
<td>181.8</td>
<td>182.3</td>
</tr>
<tr>
<td>...</td>
<td>307</td>
<td>21</td>
<td>526.7</td>
<td>521.2</td>
<td>524.0</td>
</tr>
<tr>
<td>...</td>
<td>541.5</td>
<td>20</td>
<td>306.2</td>
<td>303.2</td>
<td>304.3</td>
</tr>
<tr>
<td>...</td>
<td>392</td>
<td>...</td>
<td>419.6</td>
<td>413.1</td>
<td>415.7</td>
</tr>
<tr>
<td>...</td>
<td>676</td>
<td>38</td>
<td>240.0</td>
<td>239.1</td>
<td>240.1</td>
</tr>
<tr>
<td>...</td>
<td>343</td>
<td>...</td>
<td>458.8</td>
<td>451.8</td>
<td>454.6</td>
</tr>
</tbody>
</table>

XXVI. On the Principles of Thermodynamics.
By the Rev. J. M. Heath*.

I HAVE for some time past been challenging the attention of scientific men to the consideration of a very important question:—whether some of the most elementary principles in dynamics have not been overlooked by those who originally framed the language of thermodynamics; and whether their successors have not indolently adopted that language, and the notions which it was formed to embody, without using sufficient care to purify it of those errors which the prepossessions and imperfect knowledge of the first investigators had made almost unavoidable in them.

Mr. Rankine has replied to me, in the August Number of this Journal, to the effect that the principles I contend for are (as far as he understands me) right; and I think he must be taken as admitting, by his silence, that they were in fact ignored by the earliest original investigators in the science. But he tells me that the more modern writers, at least the best among them, had long ago perceived all that I have been pointing out, and have so altered their creed and their language as to be free from the reproach which I appear to make against them.

In support of this statement, Mr. Rankine has given us two propositions embodying the creed which is considered orthodox at the present moment. Mr. Rankine's name is so high an authority upon this point, that there can be no question that we have from him an authentic statement of what the present doctrine is, which it is said is in strict harmony with all that I have called for. If, therefore, I myself can make a similar statement of my own opinions, we shall at once be enabled to judge both whether the two doctrines are essentially one and the same, as is alleged, or, if different, which of them has the best claim to be accepted as representing the truth of the matter.

I understand Mr. Rankine's first proposition to be, that if the elasticity of a body results from the mutual attraction or repulsion of its particles acting upon one another, any forcible compression of such a body would result in "stored-up energy," but would be wholly incapable of producing (molecular motion or) heat. And I think he imagines that this is the doctrine which I have only just now come to perceive for myself and am bringing forward as somewhat of a novelty; whereas it has long ago been perceived by the best writers of the present time, and they are one and all careful in all cases to separate the amount of energy expended in this "storing-up of energy" by "overcoming the resistance of repulsive forces" from the total amount, before they estimate the remainder, which alone can produce heat.

* Communicated by the Author.
Mr. Rankine's second proposition is, that if the body's elasticity is the result of the mere motion of particles having inertia but not acting upon each other by forces of attraction and repulsion, then in the compression of such a body by external force all the force employed imparts motion to the particles of the gas, and that the addition so made to the previous vis viva of those particles is the same as the vis viva which the same force acting through the same space would have generated supposing the particles had been previously at rest,—or, in other words, that the compressing force will descend through the same space whether there be any resistance to its motion or none.

These two propositions (if indeed I have rightly understood them) appear to me to involve a conclusion completely subversive of the whole doctrine of thermodynamics, viz. that the production of heat in different bodies by compression depends upon their molecular constitution—and, furthermore, that upon no hypothesis as to the constitution of a body will the production of heat be in accordance with the true mechanical laws of the production of vis viva. If the body consists of particles which mutually repel each other, Mr. Rankine thinks no heat can ever be generated by impact or compression in such a body, although it is certain that vis viva can be generated among its particles. If the body consists of only moving and impinging particles without repulsive forces, Mr. Rankine thinks that the new heat generated by condensation in such a body will be equal to what a merely mechanical solution of the question teaches us would be the total vis viva—i. e. the sum of what there was at first, added to the addition made by the work done by the force.

I believe my own analysis of what is objectionably called the "overcoming of resistance by a force" to be true in mechanics, and to be free from each of the objections above stated.

If the area of the piston by which a volume of gas is compressed is unity, and \( P \) is the external load put upon it, and \( p \) is the internal pressure of the gas opposing its descent, then \( P \) may, at every point of the descent, be considered as consisting of two parts, one \( =p \) which we will call \( p' \), and the remainder, which therefore will be \( P-p' \).

\( p \) and \( p' \) constantly increase as the piston descends and the volume of the gas diminishes; and \( P-p' \) consequently is diminishing during the whole descent. Let \( P-p'=0 \), or \( P=p' \), when \( v \) has become \( v' \). The whole amount of work, therefore, which has to be made from the action of \( P \) through the space \( v-v' \) is \( \int_{v}^{v'} p' \, dv \). The question is, What has been the employment of this force? My answer is, that it is the sum of all the pressures which, at every point of the descent, have held the elas-
city of the gas below in stational equilibrium. It has generated no motion whatever, although it is a force exercised by a moving body; but it has neutralized the resistance; so that they might both of them be at once entirely struck out of the equations of solution, and the problem so converted into a more simple one of the condensation of a gas possessing no elasticity by the action of the force \( \int_{v}^{v'} (P - p_1) dv \).

The corrections which this solution supplies to the two propositions of Mr. Rankine respectively are, that, in the first, it admits the generation of heat by the force \( \int_{v}^{v'} (P - p_1) dv \), which Mr. Rankine denies; and in the second it denies the generation of heat by the force \( \int_{v}^{v'} p dv \), which Mr. Rankine asserts. I believe my solution to be right in both instances; but it is upon the latter point that I anticipate there will be the greatest difficulty of agreement between us, as the appeal is tacitly to a proposition which I think Mr. Rankine will at first sight consider inadmissible.

In speaking of a gas whose elasticity results from the motion of its particles, Mr. Rankine says, "work done in diminishing the capacity of this vessel wholly takes effect in accelerating the motions of the confined particles." I believe that a gas of this nature may be condensed and its pressure increased (as is required by the law of Mariotte) without increasing the vis viva of the particles, and therefore without altering the heat.

The pressure at any given point on the surface depends upon the number and frequency of the impacts of the particles, and the vis viva of each of them. We may confine our attention to the impacts of one particle only of a given force. This particle will repeat its impact upon a given spot the more frequently, the shorter the path is which it traverses between two successive impulses; in other words, it will return the more frequently, the more the volume of the gas is contracted. If, therefore, immediately after any one appulse of the particle the piston is made to take a new position (still one of rest) immediately below its former position, or to descend through the infinitesimal space dv and there remain stationary, the particle will return to it sooner than it would do in its former position. It will strike it and will be reflected by it without any increase of its motion; in other words, the Pressure may be increased and the Volume contracted without evolution of Heat.

Liphook, August 11, 1870.
XXVII. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 136.]

June 16, 1870.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communication was read:—

"On Supersaturated Saline Solutions."—Part II. By Charles Tomlinson, F.R.S.

The object of this paper is to develop more fully the principles attempted to be established in Part I., not only by clearer definitions of terms, but also by new facts and conclusions. The paper is divided into two sections; in the first of which are stated the conditions under which nuclei act in separating salt or gas or vapour from their supersaturated solutions, while in the second section is shown the action of low temperatures on supersaturated saline solutions.

The first section opens with definitions of the terms used.

A nucleus is a body that has a stronger attraction for the gas or vapour or salt of a solution than for the liquid that holds it in solution.

A body is chemically clean the surface of which is entirely free from any substance foreign to its own composition. Oils and other liquids are chemically clean if chemically pure, and contain no substance, mixed or dissolved, that is foreign to their composition. But with respect to the nuclear action of oils &c., the behaviour is different when such bodies exist in the mass, such as a lens or globule, as compared with the same bodies in the form of films.

Catharization is the act of clearing the surface of bodies from all alien matter; and the substance is said to be catharized when its surface is so cleared.

As every thing exposed to the air or to the touch takes more or less a deposit or film of foreign matter, substances are classed as catharized or uncatharized accordingly as they have or have not been so freed from foreign matter.

Referring to the definition of a nucleus, substances are divided into nuclear or non-nuclear.

The nuclear are those that may, per se, become nuclei. The non-nuclear are those that have not that quality.

The nuclear substances would seem to be comparatively few, the larger number of natural substances ranking under the other division.

Under nuclear substances are those vapours and oily and other liquids that form thin films on the surfaces of liquids and solids; and generally all substances in the form of films, and only in that form. Thus a stick of tallow, chemically clean, will not act, but a film of it will act powerfully; and, again, a globule of castor-oil will not act, if chemically clean; but in the form of a film, whether chemically clean or not, it will act powerfully.

If a drop of a liquid be placed on the surface of another liquid, it

* Phil. Mag. September 1868.
will do one of three things (apart from chemical action): (1) it will diffuse through the liquid, and in general, under such circumstances, not act as a nucleus; or (2) it will spread out into a film, or (3) remain in a lenticular shape. It becomes a film or a lens according to the general proposition that, if on the surface of the liquid $A$, whose surface-tension is $a$, we deposit a drop of the liquid $B$, whose surface-tension, $b$, is less than $a$, the drop will spread into a film; but if, on the contrary, $b$ be greater than $a$, or only a little less, the drop will remain in the form of a lens. Hence if $B$ spread on $A$, $A$ will not spread on the surface of $B$.

This general proposition may not always apply in the case of supersaturated saline solutions, on account of the superficial viscosity, or the greater or less difficulty of the superficial molecules to be displaced.

A glass rod drawn through the hand becomes covered with a thin film; or the same rod by exposure to the air contracts a film by the condensation of floating vapours, dust, &c.; and in either case it is brought into the nuclear condition.

A second class of nuclear bodies are permanently porous substances, such as charcoal, coke, pumice, &c. The action of these is chiefly confined to vaporous solutions; and if catharized they have no power of separating salts from their supersaturated solutions.

Under the non-nuclear, forming by far the larger class of substances, are glass, the metals, &c., while their surfaces are chemically clean.

Among the non-nuclear substances will be found air; for its ascribed nuclear character is due, not to itself, but to the nuclear particles of which it is the vehicle. Thus, as stated in Part I., if air be filtered through cotton-wool it loses its apparent nuclear character; so also if heated.

When a catharized body is placed in a supersaturated solution, such solution, as explained in Part I., adheres to it as a whole; but if such body be non-catharized, the gas or vapour or salt of the solution adheres to it more strongly than the liquid portion, and hence there is a separation. In the present paper it is shown that an active or non-catharized surface is one contaminated with a film of foreign matter, which filmy condition is necessary to that close adhesion which brings about the nuclear action; for it can be shown that an oil, for example, is non-nuclear in the form of a lens or globule, but powerfully nuclear in the form of a film.

Some liquids (absolute alcohol for example) form films, and act as nuclei by separating water instead of salt from supersaturated solutions.

Other liquids (glycerine for example) diffuse through the solutions without acting as nuclei.

Fatty oils may slowly saponify, or oil of bitter almonds form benzoic acid in contact with supersaturated solutions of Glauber's salt without acting as nuclei.

The solutions (say of Glauber's salt) are prepared with 1, 2, or 3 parts of the salt to 1 part of water; they are boiled, filtered into
clean flasks, and covered with watch-glasses. When cold, the watch-glass being lifted off, a drop of oil is deposited on the surface of the supersaturated solution. In an experiment described, a drop of pale seal-oil formed a well-shaped film, with a display of iridescent rings; and immediately from the lower surface of the film there fell large flat prisms with dihedral summits of the 10-atom sodic sulphate. The prisms were an inch or an inch and a half in length, and three eighths of an inch across. The crystallization proceeded from every part of the lower surface of the film, and as one set of crystals fell off, another set was formed, until the whole solution became a mass of fine crystals in a small quantity of liquid, an effect quite different from the usual crystallization which takes place when a supersaturated solution of Glauber’s salt is subjected to the action of a nucleus at one or two points in its surface, as when motes of dust enter from the air, or the surface is touched with a nuclear point. In such case small crystalline needles diverge from the point and proceed rapidly in well-packed lines to the bottom, the whole being too crowded and too rapid to allow of the formation of regular crystals.

Similar experiments were made on solutions of Glauber’s salt of different strengths, with drops of ether, absolute alcohol, naphtha, benzole, the oils of turpentine, cajeput, and other volatile oils, sperm, herring, olive, linseed, castor, and other fixed oils of animal and vegetable origin, with this general result, that, whenever the liquid drop spread out into a film, it acted as a powerful nucleus; but when the oil formed a lens there was no separation of salt, even when the flasks were shaken so as to break up the lens into small globules. If, however, a sudden jerk were given to the flask so as to flatten some of the globules against its sides into films, the whole solution instantly became solid. A similar effect was produced by introducing a clean inactive solid, for the purpose of flattening a portion of oil against the side of the flask.

Stearine from sheep’s tallow that had been exposed to the air produced immediate crystallization; but by boiling the solution and covering the flasks, the stearine, now catharized, had lost its nuclear character on the cold solution. Similar observations were made with the fixed oils that form lenses or globules in the solution. So also volatile oils containing products of oxidation, dust, &c. are nuclear; but when catharized by being redistilled, they are inactive in the globular state, active in the form of films.

Supersaturated solutions of potash alum, ammonia alum, sodic acetate, and magnesia sulphate were also operated on, with results similar to those obtained with solutions of Glauber’s salt.

When a liquid forms a film on the surface of a supersaturated solution, the surface-tension of the solution is so far diminished as to bring the film into contact with the solution, when that differential kind of action takes place whereby, the salt of the solution adhering more strongly to the film than the water of the solution, the action of separation and crystallization, thus once begun, is propagated throughout. A similar action takes place with solid bodies that have contracted filmy nuclei of being touched or drawn through
the hand, or merely exposed to the air; they are active or nuclear by virtue of the films of matter which more or less cover them.

On the other hand, when a drop of oil (or many drops) is placed on the surface of a supersaturated saline solution, and it assumes the lenticular form, or even flattens into a disk, such lens or disk is separated from actual contact with the solution by surface-tension. That the adhesion is very different from that of a film may be shown by pouring a quantity of recently distilled turpentine, for example, on the surface of chemically clean water, and scraping upon it some fragments of camphor; these will be immediately covered with a solution of camphor in the oil, which solution will form iridescent films, and sail about with the camphor, vigorously displacing the turpentine, and cutting it up into smaller disks and lenses. So in the case of supersaturated saline solutions; the oil-lens is not sufficiently in contact with the surface of the solution to allow of the exertion of that differential kind of action whereby salt is separated. Even when, by shaking, the oil is broken up into globules, and these are submerged, they are still so far separated from the solution by surface-tension as to prevent actual contact.

In the second section it is shown that solutions of certain salts which remain liquid and supersaturated at and about the freezing-point of water, by a further reduction in temperature, to from 0° F. to —10° and in the absence of a nucleus, rather solidify than crystallize, but on being restored to 32° recover their liquid state without any separation of salt.

A solution of ferrous sulphate, for example, at 0° Fahr. formed tetrahedral crystals at the surface, which spread downwards until the contents of the tube became solid. In snow-water at 32° the frozen mass shrank from the sides of the tube, formed into a smooth rounded mass, and gradually melted, leaving the solution clear and bright without any deposit. On removing the cotton-wool from the mouth of the tube, small but well-shaped rhomboidal crystals soon filled the solution.

A similar experiment was tried with the double salt formed by mixing in atomic proportions solutions of the zincic and magnesic sulphates. A supersaturated solution of this salt at about —8° F. became solid, and it melted quickly at 32°. Such a solution may be solidified and melted a number of times, provided it be protected from the action of nuclei; but if the cotton-wool be removed from the tube, even when the contents are solid, and be immediately re-inserted, there will be a separation of the salt during the melting, in consequence of the entrance of nuclear particles from the air.

Solutions of such a strength as to be only saturated at ordinary temperatures, and therefore not sensitive to the action of nuclei, become very much so by reduction of the temperature below 32° Fahr. Salts that contain a large amount of water of crystallization, such as the zincic and magnesic sulphates, require only a small portion of added water in order to form supersaturated solutions, and they become more sensitive to the action of nuclei as the temperature falls,
or, in other words, as they become more highly supersaturated. Thus a very strong solution of calcichloride, which is not sensitive to nuclei at 40° or 45°, becomes very much so at 24° or 34°.

The sodio-zincic sulphate contains only 4 proportions of water of crystallization; and hence its supersaturated solutions are not stable at low temperatures. When freshly made, they may be reduced to 10° Fahr. without separation of the salt; but by repose, even in clean tubes and in the absence of nuclei, long crystals of the separated salts occupy the length of the tube, but they are invisible on account of having the same refractive index as that of the solution in which they are immersed. The course of time, probably from the escape of vapour of water through the porous plug, they become visible.

A solution of the ammonia zincic sulphate at 4° Fahr. formed beautiful large feathery crystals of an opaque white, which gradually filled the tube. They melted rapidly at 32°.

A supersaturated solution of nickel sulphate resisted a temperature of 6° Fahr. Mixed with an equivalent weight of cupric sulphate, the two salts separate if the solution is exposed to the air; but in closed tubes the solution at 0° Fahr. forms beautiful feathery crystals, which melt rapidly at 32°, without any separation of salt.

Similar phenomena are produced by a supersaturated solution of zinc sulphate and potash alum in equivalent proportions exposed to a temperature of 4° Fahr. A similar solution of the cupric and magnesic sulphates at -4° also became solid, and melted rapidly at 32°.

Experiments were also made with the sodic and magnesic sulphates, cadmic, and some other sulphates. The addition of potassic sulphate to other sulphates, in atomic proportions, forms double salts, which, so far as they were examined, do not form supersaturated solutions.

The effect of low temperatures was in some cases to throw down a portion of the salts in the anhydrous form, upon which were formed by repose crystals of a lower degree of hydration than the normal salt. Some cases of this kind are described in the paper.

GEOLOGICAL SOCIETY.

[Continued from p. 142.]

March 9th, 1870.—Warington W. Smyth, Esq., F.R.S.,
Vice-President, in the Chair.

The following communications were read:—


The author described the characters of the fossil stem of a Fern obtained by George Dowker, Esq., F.G.S., from the beach at Herne Bay, and stated that in its structure it agreed most closely with the living Osmunda regalis, and certainly belonged to the Osmundaceæ.
The broken petioles show a single crescentic vascular bundle. The section of the true stem shows a white parenchymatous medulla, a narrow vascular cylinder interrupted by long slender meshes from which the vascular bundles of the petioles spring, and a parenchymatous cortical layer. The author described the arrangement of these parts in detail, and indicated their agreement with the same parts in *Osmunda regalis*. He did not venture to refer the Fern, to which this stem had belonged, positively to the genus *Osmunda*, but preferred describing it as an *Osmundites*, under the name of *O. Dowkeri*. The specimen was silicified; and the author stated that even the starch-grains contained in its cells, and the mycelium of a parasitic Fungus traversing some of them, were perfectly represented. Its precise origin was unknown; it was said to be probably derived from the London Clay, or from the beds immediately below.

2. "On the Oolites of Northamptonshire." By Samuel Sharp, Esq., F.G.S.

The author stated that his ultimate purpose was to describe severally the Oolitic beds occurring in the Northampton district, in the more northerly parts of the county, and in the neighbourhood of Stamford, to exhibit fossils gathered from each locality, and to correlate the several series and thus to endeavour to establish the character and sequence of the Oolites of this Midland district. He anticipated, however, that the publication by the Geological Survey of their maps and memoir of North Northamptonshire (the work of Mr. Judd) would intervene and might render superfluous the carrying out this work in its entirety; but in the meantime he submitted his first part, "The Oolites of Northampton and Neighbourhood."

The author stated that there were four areas within a comparatively small space in which the whole of the beds occurring in each, from the Great Oolite down to the Upper Lias (inclusive), were accessible. These were situated at or about:—1, Kingsthorpe; 2, Northampton; 3, Duston; 4, Blisworth. The Oolitic beds in these several areas were described in detail, the beds of the Northampton Sand (as comparatively little known) being those to which the greatest interest attached. These he proposes to class in three divisions—the "Upper," the "Middle," and the "Lower" Northampton Sand.

The individual beds of the several localities were shown to vary considerably; but collectively they would present the following general section, the maximum thicknesses being given in feet:

<table>
<thead>
<tr>
<th>Name</th>
<th>Description</th>
<th>Thicknesses</th>
</tr>
</thead>
<tbody>
<tr>
<td>GREAT OOLITE</td>
<td>A. White Limestone, disposed in beds of from a few inches above the base, varying in character, and containing characteristic Great-Oolite fossils</td>
<td>25</td>
</tr>
<tr>
<td></td>
<td>B. Blue and grey clay, dug for brick-making, with a ferruginous band at base, and Great-Oolite fossils</td>
<td>15</td>
</tr>
</tbody>
</table>

[Line of unconformity.]
C. White or grey sand, more or less coherent, and with occasional ferruginous stains, sometimes quarried for building-stone. A plant-bed is usually found in this sand .......................................................... 12

D. A series of very variable beds, composed sometimes of ferruginous sandstone in thin layers, which overlie calcareous beds containing shelly zones, false bedding being frequent. Sometimes the whole section consists of calcareous rock with false bedding; sometimes it presents a series of beds of compact ferruginous sandstone with no fossils. In one instance the entire section consists of white sand and sandstone, with no fossils ........ 30

Coarse Oolitic or subcrystalline Limestone, with fossils, overlying a calcareo-arenaceous slate, like Colleyweston Slate .......................... 4

E. Beds chiefly consisting of Ironstone, containing Rhynchonella variabilis and R. cynocephala, and Ammonites bifrons at the base ........................................... 35

This general section, the author stated, might be accepted as a typical section of a considerable portion of the county of Northampton.

In his concluding remarks the author referred to the great limestone which marked the country about Stamford and, traversing Rutland, attained its greatest thickness in Lincolnshire. This limestone was proved by its palæontological contents to be Inferior Oolite; and its place, with reference to the beds described in the paper, was shown to be in the interval (marking the line of unconformity) between B and C of the general section. It thus tended to confirm the statement of the author that the line of division between the Great and the Inferior Oolite in the neighbourhood of Northampton occurred at that point.

The paper was illustrated by the exhibition of a large collection of fossils from the several areas, including some new species, prominent among which was a new Starfish, named, in compliment to the author, "Stellaster Sharpii," by Dr. Wright, F.R.S.E., F.G.S., and described by him in a Note appended to this paper.

XXVIII. Intelligence and Miscellaneous Articles.

ON THE EXTENSION OF OHM'S LAWS TO ELECTROLYTES, AND ON THE NUMERICAL DETERMINATION OF THE RESISTANCE OF DILUTE SULPHURIC ACID BY MEANS OF ALTERNATE CURRENTS.

BY MM. F. KOHLRAUSCH AND A. NIPPOLDT.

The special resistance of electrolytes is complicated with the phenomena of polarization which most frequently accompany electrolysis. To determine the first element it must be freed from the influence of the second, which presents great difficulties. The authors think they have removed these difficulties by causing induction-currents to pass through the liquids alternately in opposite directions. This method is not new; it has already been employed by MM. De la
Rive, Lenz, Poggendorff, and Vorssellmann de Heer*; and it has been proved that the polarization was not completely destroyed by this means. But it may be hoped that it can be completely annulled by diminishing the duration of the alternate currents, which diminishes the perturbing electrolytic effect—and by increasing the size of the electrodes, which diminishes the intensity of the currents, and consequently the polarization, which is nearly proportional to it. It will be acknowledged that this object is attained when the resistance of the liquid follows Ohm’s law—that is to say, when this resistance could be replaced by that of the wire of a metallic rheostat, whatever the intensity.

In order to apply this method, three apparatus are indispensable—an induction-apparatus which gives currents alternately in opposite directions, an apparatus for their measurement which admits of these currents being compared, and also a rheostat.

For the induction-apparatus the siren by Weber and R. Kohlrausch was chosen†. It is an ordinary siren having the moveable disk of magnetized steel. This magnet rotates inside a rectangular multiplier similar to those of ordinary galvanometers; and its rotation produces in the wire of the multiplier induced currents which change their direction at each half-revolution.

There is no better instrument with which to measure these alternate currents than Weber’s bifilar dynamometer. Its deviation, δ, is proportional to the square of the intensity of the induced currents which traverse it; and these latter vary with the velocity of the rotation of the disk of the siren, a velocity which may be measured from the sound produced. When, for all velocities of the siren, the resistance of the column of liquid could be replaced by that of the same length of wire of the rheostat, it was clear that the polarization had disappeared, and that the rheostat measured the special resistance of the liquid. Now this is what always occurred when the electrodes had the greatest dimensions (2900 square millims.).

In these experiments the velocity of the disk varied from 2·3 to 76·9 turns per second; the electromotive force of the induced current varied consequently from 141 to 3 of that of a Grove’s element. The authors wished to operate on weaker currents. They used the thermoelectric currents produced by an iron-copper element, which gives for 1° of difference of temperature an electromotive force equal to 1/429 of that of Grove, this difference not having in their experiments exceeded 4°. This current passed through a solution of sulphate of zinc 83 millims. in length and having a section of 2400 square millimetres; in order to measure it the dynamometer had to be replaced by a very sensitive galvanometer. In these experiments the electromotive force descends to 1/429 of Grove’s, and the liquid follows Ohm’s laws in all cases.

The memoir ends with a determination of the resistance of water acidulated by sulphuric acid at different degrees of concentration. By collecting all the experiments at the same temperature, the fol-

The resistances in the above Table have been corrected by a diminution of 0.2 per cent., in accordance with a note, at the end of the memoir. They can be represented by a very regular curve, which has a minimum ordinate when the density of the solution is 1.233. Hence acidulated water which contains 31.6 per cent. of monohydrated acid offers the least resistance to the passage of the current. The coefficient of reduction with the temperature is supposed constant, although this constancy has only been demonstrated directly for solutions of sulphate of zinc; hence this coefficient must not be applied to great variations of temperature.—Poggendorff’s *Annalen*, vol. cxxxviii.; *Annales de Chimie et de Physique*, April 1870.

---

**ON LIQUIDS OF HIGH DISPERSIVE POWER.** BY WOLCOTT GIBBS, M.D., RUMFORD PROFESSOR IN HARVARD UNIVERSITY.

Of the liquids which have hitherto been proposed for the construction of prisms, bisulphide of carbon unquestionably presents the greatest advantages. It is cheap, colourless, and unites a moderately high mean refractive to a very high dispersive power. By tacit consent a prism of 60° filled with this liquid has come to be adopted as a sort of standard. The disadvantages of the bisulphide are equally well known; and I have spent no little time and labour in the endeavour to find a liquid with a still higher dispersive power, less volatile, less sensitive optically to changes of temperature, and less offensive in odour. In these efforts I have not been altogether successful, no one liquid examined possessing all the qualities desired. Many organic liquids with high dispersive powers are difficult to prepare in a state of purity, and speedily become discoloured by absorption of oxygen from the air. Such are oil of cassia, the colourless oil obtainable from balsam of Peru, and others. The thallie al-
Intelligence and Miscellaneous Articles.

cohol of Lamy* is far too costly. The solution of silico-tungstate of sodium†, of metatungstate of sodium‡, and of soluble tungstic acid§ as obtained by dialysis, all promised good results from their extraordinary densities; but all proved difficult to prepare in a state of purity, and extremely easy of decomposition.

A solution of phosphorus in bisulphide of carbon has, according to Messrs. Dale and Gladstone∥, a dispersion of 0·225¶, or nearly one and a half times as great as bisulphide of carbon alone, but becomes turbid on exposure to sunlight from the formation of amorphous phosphorus. It occurred to me that, by dissolving sulphur with the phosphorus, the formation of amorphous phosphorus might be prevented; and experiment proved that this was the case. The solution, as thus obtained, has a pale yellow colour, but is perfectly clear and undergoes no change by the action of light even when long continued. I have been in the habit of preparing it by dissolving one part of dry flowers of sulphur and two parts of phosphorus in four or five parts of bisulphide of carbon, and filtering the liquid through a well-dried ribbed paper filter, which is easily done. The refractive and dispersive power of the solution will of course vary with the quantity of phosphorus and sulphur dissolved. By a gentle heat the whole, or nearly the whole, of the bisulphide of carbon may be driven off, a liquid compound of sulphur and phosphorus remaining, which has so high a mean refractive power that it cannot be employed with prisms having a refractive angle of more than 45°–50°. The same end may, however, also be attained by continually adding phosphorus to a saturated solution of sulphur in bisulphide of carbon, in which phosphorus appears to be soluble without limit.

With a strong and probably saturated solution of sulphur in CS₂ the angle between Li and D was 0° 50′ 10″. When phosphorus was added the angle was 2° 25′ 30″, the refracting angle of the prism being 60°. In this last case the angle between Na₁ and Na₂ was 0° 2′ 20″. The spectrum was perfectly clear, the definition of the dark lines leaving nothing to be desired. In consequence, however, of the yellow colour of the liquid, there is always a marked absorption of the violet end of the spectrum.

In working with the above described solution I have employed hollow glass prisms with refracting plates cemented on with a mixture of glue and molasses. These were found to be perfectly tight and to last for months without change. The great disadvantage in the use of a solution of sulphur and phosphorus consists in the danger of breaking the prisms, the liquid taking fire spontaneously when it has been a few seconds in contact with any porous material like wood or paper. On the other hand, however, the large quantity of sulphur

† Ibid. p. 5.
¶ The number 0·225 is the difference between the indices for the extreme red and violet rays.
present prevents the fire from spreading, a drop placed upon a piece of wood leaving after combustion only a charred spot. When not in use, the prisms should be kept in an iron pot with a tight cover. In this manner I have employed and preserved two during a long and hot summer. The viscid or, rather oily nature of the solution serves to prevent, to a great extent, the formation of ascending and descending currents from slight changes of temperature; and when the prisms are well shaken before use, the definition remains perfect for a long time. In my spectroscope the prisms rest upon a plate of glass instead of upon one of metal.—Silliman's American Journal, July 1870.

ON THE PROPAGATION OF SOUND IN TUBES.
BY M. AD. SEEBECK.

It is admitted that the velocity of sound propagated in a tube is less than in the open air, and the smaller the tube the less the velocity*. Kirchhoff has given the following formula for the velocity of sound:—

\[ V = A \left( 1 - \frac{K}{2r \sqrt{n \pi}} \right), \]

in which \( A \) is the velocity in air, \( r \) the radius of the tube, \( n \) the number of vibrations, \( K \) a constant dependent on the calorific conductivity and on friction. This formula is the same as the one given by Helmholtz, except the meaning of the constant \( K \).

The method employed by Seebeck does not differ materially from that which Schneebeli adopted in order to verify Helmholtz's formula, and of which he has already given an account†.

The apparatus used was constructed of a horizontal tube, closed near one end by a moveable piston; near the other a very narrow lateral tube was soldered, to which a caoutchouc tube was fitted which communicated with the ear. A sound was produced in front of the opening of the tube by means of a tuning-fork fixed horizontally to a piece of wood insulated by some caoutchouc supports. That the ear may hear distinctly the sound produced by the tuning-fork, at the junction of the large tube closed by the piston and the small tube communicating with the ear there must be a node, due to the coexistence of direct and reflected waves against the piston. If a loop is produced, the sound perceived by the ear will have its minimum intensity; it is this character which is to be appreciated; and it is then known whether the distance from the piston to the opening of the small tube is equal to \( \frac{\lambda}{4} \), or in general to \( (2n+1) \frac{\lambda}{4} \), \( \lambda \) being the wave-length of the sound produced; from this we get the velocity of sound by the formula \( V = n\lambda \), \( n \) being the number of vibrations of the corresponding sound. The whole section of the tube was agitated, which Kirchhoff's formula presupposes, except for the largest

* Annales de Chimie et de Physique, S. 4. vol. xv. pp. 487, 492,
† Ibid. vol. xvii. p. 512.
tubes used. The observations were reduced to zero by means of the
formula \( \lambda = \frac{\lambda_1}{\sqrt{1 + at}} \), by observing at the commencement and the
end of each series of observations a thermometer placed close at
hand; the air in the tube was first dried by chloride of calcium.
Seebeck used König's four tuning-forks, which give the notes \( ut \),
\( sol \), \( mi \), \( ut \), of which the numbers of vibrations were 512, 384, 320,
256. He found the following results:

<table>
<thead>
<tr>
<th>Diameter</th>
<th>Tuning-forks.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>( ut )=256.</td>
</tr>
<tr>
<td>2r</td>
<td>millims.</td>
</tr>
<tr>
<td>3·4</td>
<td>...</td>
</tr>
<tr>
<td>9</td>
<td>325·63</td>
</tr>
<tr>
<td>17·5</td>
<td>327·82</td>
</tr>
<tr>
<td>29</td>
<td>324·54</td>
</tr>
</tbody>
</table>

The velocity of the sound diminishes with the diameter of the tube,
except for the largest (2r=29 millims.), which Seebeck attributes
to the whole of the section not having been agitated; it may also
be that in the large tubes the motion of the gaseous molecules is not
parallel to their axis. By adopting for the velocity of sound in air
a the number 332·77 millims., a result obtained from the determina-
tions by Moll and Van Beek, Seebeck has calculated the difference
A—V. According to the formula by Kirchhoff, we must have

\[
A - V = \Lambda \frac{K}{2r \sqrt{n \pi}},
\]

and consequently, for the same sound and various tubes,

\[
(A - V_1) \times 2r_1 = (A - V_2) \times 2r_2,
\]

an equation which the determinations of Seebeck satisfy, except for
the largest tube (2r=29 millims.). But it was also found that
\((A - V) \sqrt{n}\), for the same value of r, is no longer constant. Seebeck
recognized, on the contrary, that the product \((A - V)n^\frac{3}{2}\) was ob-
viously constant; and consequently the diminution of velocity of sound in a tube varies in inverse ratio with the power \(\frac{3}{2}\) of the num-
ber of vibrations of the corresponding sound.

Seebeck also made some investigations when the interior of his
tubes was covered with leather or flannel: he proved that there was
a considerable diminution in the velocity of sound; in the latter case
it fell to 281·7 millims. These experiments, however, leave some-
thing to be desired, because the interior of the tube was not com-
pletely covered. A small slit was made that the position of the piston might be observed: would it not be possible to determine the position of the piston exactly by means of the rod by which it is re-
gulated?—Poggendorff's *Annalen*, vol. cxxxix. p. 104; *Annales de
Chimie*, April 1870.

[Continued from vol. xxxix. p. 194.]

There is no point connected with ocean-currents on which more diversity of opinion has existed than in regard to their origin. At present, however, there may be said to be only two theories held on the subject, viz. that which attributes the currents to the influence of the trade and other winds, and that which attributes them to differences in specific gravity between the waters of intertropical and polar regions. The latter theory appears at present to be the more prevalent of the two, although, perhaps, not so among scientific men. It is difficult to conceive how a theory so manifestly erroneous should have gained such general acceptance. Its popularity is no doubt chiefly owing to the very great prominence given to it by Lieut. Maury in his interesting and popular work 'The Physical Geography of the Sea.' Another cause which must have favoured the reception of this theory is the ease with which it is perceived how, according to it, circulation of the waters of the ocean is supposed to follow. One has no difficulty, for example, in perceiving that if the intertropical waters of the ocean are expanded by heat, and the waters around the poles contracted by cold, the surface of the ocean will stand at a higher level at the equator than at the poles. Equilibrium being thus disturbed, the water at the equator will tend to flow towards the poles as a surface-current, and the water at the poles towards the equator as an undercurrent. This, at first sight, looks well, especially to those who take but a superficial view of the matter.
We shall examine this theory at some length, for two reasons: 1, because it lies at the root of a great deal of the confusion and misconception which have prevailed in regard to the whole subject of ocean-currents; 2, because, if the theory is correct, it militates strongly against the physical theory of secular changes of climate advanced in the preceding part of this paper. We have already seen that when the excentricity of the earth’s orbit reaches a high value, a combination of physical circumstances tends to lower the temperature of the hemisphere which has its winter solstice in aphelion, and to raise the temperature of the opposite hemisphere, whose winter solstice will, of course, be in perihelion. The direct result of this state of things, as was shown, is to strengthen the force of the trade-winds on the cold hemisphere, and to weaken their strength on the warm hemisphere; and this, in turn, we also saw tends to impel the warm water of the intertropical region over on the warm hemisphere, and to prevent it, in a very large degree, from passing into the cold hemisphere. This deflection of the ocean-currents tends to an enormous extent to increase the difference of temperature previously existing between the two hemispheres. In other words, the warm and equable condition of the one hemisphere, and the cold and glacial condition of the other, are, to a great extent, due to this deflection of ocean-currents. But if the theory be correct which attributes the motion of ocean-currents to a difference in density between the sea in intertropical and polar regions, then it follows that these currents (other things being equal) ought to be stronger on the cold hemisphere than on the warm, because there is a greater difference of temperature and, consequently, a greater difference of density between the polar seas of the cold hemisphere and the equatorial seas, than between the polar seas of the warm hemisphere and the equatorial seas. And this being the case, notwithstanding the influence of the trade-winds of the cold hemisphere blowing over upon the warm, the currents will, in all probability, be stronger on the cold hemisphere than on the warm. In other words, the influence of the powerful trade-winds of the cold hemisphere to impel the warm water of the equator over upon the warm hemisphere will probably be more than counterbalanced by the tendency of the warm and buoyant waters of the equator to flow towards the dense and cold waters around the pole of the cold hemisphere. But if ocean-currents are due not to difference in specific gravity, but to the influence of the trade-winds, then it is evident that the waters at the equator will be impelled, not into the cold hemisphere, but into the warm.

As Lieut. Maury appears to be the acknowledged exponent of the theory which attributes ocean-currents to the difference of
specific gravity between the waters at the equator and the poles, I shall now proceed to consider at some length his views on the subject, the more especially as we find in his work on the physical geography of the sea almost every argument that can be advanced in favour of the theory which he advocates.

Although considerable diversity of opinion has prevailed in regard to the cause of ocean-currents, yet it is remarkable how little is to be found of a purely scientific character bearing directly on the dynamics of the subject.

Another reason which has induced me to select Maury’s work is, that it not only contains a much fuller discussion on the cause of the motion of ocean-currents than is to be found anywhere else, but also that it has probably passed through a greater number of editions than any other book of a scientific character in the English language in the same length of time.

Lieut. Maury on the Cause of the Motion of Ocean-currents.

Although Lieut. Maury has expounded his views on the cause of ocean-currents at great length in the various editions of his work, yet it is somewhat difficult to discover what they really are. This arises chiefly from the generally confused and sometimes contradictory nature of his hydrodynamical conceptions. After a repeated perusal of several editions of his book, the following, I trust, will be found to be a pretty accurate representation of his theory:—

Ocean-currents, according to Maury, due to difference of specific gravity.—Although Maury alludes to a number of causes which, he thinks, tend to produce currents, yet he deems their influence so small that, practically, all currents may be referred to difference of specific gravity.

“If we except,” he says, “the tides, and the partial currents of the sea, such as those that may be created by the wind, we may lay it down as a rule that all the currents of the ocean owe their origin to the differences of specific gravity between sea-water at one place and sea-water at another; for wherever there is such a difference, whether it be owing to difference of temperature or to difference of saltiness, &c., it is a difference that disturbs equilibrium, and currents are the consequence” (§ 467)*. To the same effect see §§ 896, 37, 512, 520, and 537.

Notwithstanding the fact that Maury is continually referring to difference of specific gravity as the great cause of currents, it is difficult to understand in what way he conceives this difference to act as a cause.

Difference of specific gravity between the waters of the ocean

* The edition from which I quote, unless stated to the contrary, is the one published by Messrs. T. Nelson and Sons, 1870.
at one place and another can give rise to currents only through the influence of the earth's gravity. All currents resulting from difference of specific gravity can be ultimately resolved into the general principle that the molecules that are specifically heavier descend and displace those that are specifically lighter. If, for example, the ocean at the equator be expanded by heat or by any other cause, it will be forced by the denser waters in temperate and polar regions to rise so that its surface shall stand at a higher level than the surface of the ocean in these regions. The surface of the ocean will become an inclined plane, sloping from the equator to the poles. Hydrostatically, the ocean, considered as a mass, will then be in a state of equilibrium; but the individual molecules will not be in equilibrium. The molecules at the surface in this case may be regarded as lying on an inclined plane sloping from the equator down to the poles, and as these molecules are at liberty to move they will not remain at rest, but will descend the incline towards the poles. When the waters at the equator are expanded, or the waters at the poles contracted, gravitation makes, as it were, a twofold effort to restore equilibrium. It in the first place sinks the waters at the poles, and raises the waters at the equator, in order that the two masses may balance each other; but this very effort of gravitation to restore equilibrium to the mass destroys the equilibrium of the molecules by disturbing the level of the ocean. It then, in the second place, endeavours to restore equilibrium to the molecules by pulling the lighter surface-water at the equator down the incline towards the poles. This tends not only to restore the level of the ocean, but to bring the lighter water to occupy the surface and the denser water the bottom of the ocean; and when this is done, complete equilibrium is restored, both to the mass of the ocean and to its individual molecules, and all further motion ceases. But if heat be constantly applied to the waters of the equatorial regions, and cold to those of the polar regions, and a permanent disturbance of equilibrium maintained, then the continual effort of gravitation to restore equilibrium will give rise to a constant current. In this case, the heat and the cold (the agents which disturb the equilibrium of the ocean) may be regarded as causes of the current, inasmuch as without them the current would not exist; but the real efficient cause, that which impels the water forward, is the force of gravity. But the force of gravity, as has already been noticed, cannot produce motion (perform work) unless the thing acted upon descend. Descent is implied in the very conception of a current produced by difference of specific gravity.

But Maury speaks as if difference of specific gravity could give rise to a current without any descent.
Mr. J. Croll on the Physical Cause of Ocean-currents.

"It is not necessary," he says, "to associate with oceanic currents the idea that they must of necessity, as on land, run from a higher to a lower level. So far from this being the case, some currents of the sea actually run up hill, while others run on a level. The Gulf-stream is of the first class" (§ 408). "The top of the Gulf-stream runs on a level with the ocean; therefore we know it is not a descending current" (§ 18). And in § 9 he says that between the Straits of Florida and Cape Hatteras the waters of the Gulf-stream "are actually forced up an inclined plane, whose submarine ascent is not less than 10 inches to the mile." To the same effect see §§ 25, 59.

It is perfectly true that "it is not necessary to associate with ocean-currents the idea that they must of necessity, as on land, run from a higher to a lower level." But the reason of this is that ocean-currents do not, like the currents on land, owe their motion to the force of gravitation. If ocean-currents result from difference of specific gravity between the waters in tropical and polar regions, as Maury maintains, then it is necessary to assume that they are descending currents. Whatever be the cause which may give rise to a difference of specific gravity, the motion which results from this difference is due wholly to the force of gravity; but gravity can produce no motion unless the water descend.

This fact must be particularly borne in mind while we are considering Maury's theory that currents are the result of difference of specific gravity.

Ocean-currents, then, according to Maury, owe their existence to the difference of specific gravity between the waters of intertropical and polar regions. This difference of specific gravity he attributes to two causes—(1) to difference as to temperature, (2) to difference as to saltness. There are one or two causes of a minor nature affecting the specific gravity of the sea, to which Maury alludes; but these two determine the general result. Let us begin with the consideration of the first of these two causes, viz.:

_Difference of specific gravity resulting from difference of temperature._—Maury explains his views on this point by means of an illustration. "Let us now suppose," he says, "that all the water within the tropics, to the depth of one hundred fathoms, suddenly becomes oil. The aqueous equilibrium of the planet would thereby be disturbed, and a general system of currents and counter currents would be immediately commenced—the oil, in an unbroken sheet on the surface, running toward the poles, and the water, in an undercurrent, toward the equator. The oil is supposed, as it reaches the polar basin, to be reconverted into water, and the water to become oil as it crosses Cancer and Capricorn, rising to the surface in intertropical
regions, and returning as before” (§20). “Now,” he says (§22), “do not the cold waters of the north, and the warm waters of the Gulf, made specifically lighter by tropical heat, and which we see actually preserving such a system of countercurrents, hold, at least in some degree, the relation of the supposed water and oil?”

In §24 he calculates that at the Narrows of Bemini the difference in weight between the volume of the Gulf-water that crosses a section of the stream in one second, and an equal volume of water at the ocean temperature of the latitude, supposing the two volumes to be equally salt, is fifteen millions of pounds. Consequently the force per second operating to propel the waters of the Gulf towards the pole would in this case, he concludes, be the “equilibrating tendency due to fifteen millions of pounds of water in the latitude of Bemini.” In §§511 and 512 he states that the effect of expanding the waters at the torrid zone by heat, and of contracting the waters at the frigid zone by cold, is to produce a set of surface-currents of warm and light water from the equator towards the poles, and another set of undercurrents of cooler and heavy water from the poles towards the equator. See also to the same effect §§513, 514, 896.

There can be no doubt that Maury concludes that the waters in intertropical regions are expanded by heat, and those in polar regions are contracted by cold, and that this tends to produce a surface-current from the equator to the poles, and an undercurrent from the poles to the equator.

We shall now consider his second great cause of ocean-currents, viz.:

Difference of specific gravity resulting from difference in degree of saltness.—Maury maintains, and that correctly, that saltness increases the density of water—that, other things being equal, the saltest water is the densest. He suggests “that one of the purposes which, in the grand design, it was probably intended to accomplish by having the sea salt and not fresh, was to impart to its waters the forces and powers necessary to make their circulation complete” (§495).

Now it is perfectly obvious that if difference in saltness is to cooperate with difference in temperature in the production of ocean-currents, the saltest waters, and consequently the densest, must be in the polar regions, and the waters least salt, and consequently lightest, must be in equatorial and intertropical regions. Were the saltest waters at the equator, and the freshest at the poles, it would tend to neutralize the effect due to heat, and, instead of producing a current, would simply tend to prevent the existence of the currents which otherwise would result from difference of temperature.
Mr. J. Croll on the Physical Cause of Ocean-currents. 239

A very considerable portion of Maury’s book, however, is devoted to proving that the waters of equatorial and intertropical regions are saltier and heavier than those of the polar regions; and yet, notwithstanding this, he endeavours to show that this difference in respect to saltiness between the waters of the equatorial and the polar regions is one of the chief causes, if not the chief cause, of ocean-currents. In fact, it is for this special end that so much labour is bestowed in proving that the saltiest water is in the equatorial and intertropical regions, and the freshest in the polar.

“In the present state of our knowledge,” he says, “concerning this wonderful phenomenon (for the Gulf-stream is one of the most marvellous things in the ocean) we can do little more than conjecture. But we have two causes in operation which we may safely assume are among those concerned in producing the Gulf-stream. One of these is the increased saltiness of its water after the trade-winds have been supplied with vapour from it, be it much or little; and the other is the diminished quantum of salt which the Baltic and the Northern Seas contain” (§ 37). “Now here we have, on one side, the Caribbean Sea and Gulf of Mexico, with their waters of brine; on the other, the great Polar basin, the Baltic, and the North Sea, the two latter with waters that are but little more than brackish. In one set of these sea-basins the water is heavy, in the other it is light. Between them the ocean intervenes; but water is bound to seek and to maintain its level; and here, therefore, we unmask one of the agents concerned in causing the Gulf-stream” (§ 38). To the same effect see §§ 52, 522, 523, 524, 525, 526, 528, 530, 554, 556.

Lieut. Maury’s two causes neutralize each other. Here we have two theories put forth regarding the cause of ocean-currents, the one in direct opposition to the other. According to the one theory, ocean-currents exist because the waters of equatorial regions, in consequence of their higher temperature, are less dense than the waters of the polar regions; but according to the other theory, ocean-currents exist because the waters of equatorial regions, in consequence of their greater saltiness, are more dense than the waters of the polar regions. If the one cause be assigned as a reason why ocean-currents exist, then the other can be equally assigned as a reason why they do not exist. According to both theories it is the difference of density between the equatorial and polar waters that gives rise to currents; but according to the one theory the equatorial waters are lighter than the polar, whilst according to the other theory they are heavier than the polar. Either the one theory or the other may be true, or neither; but it is logically impossible that both of them can, for the simple reason that the waters of the equator cannot
at the same time be both lighter and heavier than the water at the poles. They may be either the one or the other, but they cannot be both. Let it be observed that it is not two currents, the one contrary to the other, with which we have at present to do; it is not temperature producing currents in one direction, and saltness producing currents in the contrary direction. We have two theories regarding the origin of currents, the one diametrically opposed to the other. The tendency of the one cause assigned is to prevent the action of the other cause. If temperature is allowed to act, it will make the intertropical waters lighter than the polar, and then, according to theory, a current will result. But if we bring saltness into play (the other cause) it will do the reverse: it will increase the density of the intertropical waters and diminish the density of the polar; and so far as it acts it will diminish the currents produced by temperature, because it will diminish the difference of specific gravity between the intertropical and polar regions which had been previously caused by temperature. And when the effects of saltness are as powerful as those of temperature, the difference of specific gravity produced by temperature will be completely effaced, or, in other words, the waters of the equatorial and polar seas will be of the same density, and consequently no current will exist. And so long as the two causes continue in action, no current can arise, unless the energy of the one cause should happen to exceed that of the other; and even then a current will only exist to the extent by which the strength of the one exceeds that of the other.

The contrary nature of the two theories will be better seen by considering the way in which he supposes difference in saltness is produced and acts as a cause.

If there is a constant current resulting from the difference in saltness between the equatorial and polar waters, then there must be a cause which maintains this difference in saltness. The current is simply the effort to restore the equilibrium lost by this difference; and the current would very soon do this, and then all motion would cease, were there not a constantly operating cause maintaining this disturbance. What, then, according to Maury, is the cause of this disturbance, or, in other words, what is it that keeps the equatorial waters saltier than the polar?

The agencies in operation which keep the waters in equatorial regions saltier than the polar are stated by him to be heat, radiation, evaporation, precipitation, and secretion of solid matter in the form of shells, &c. The two most important, however, are evaporation and precipitation.

The trade-winds enter the equatorial regions as relatively dry winds thirsting for vapour; consequently they absorb far more
moisture than they give out; and the result is that, in intertropical regions, evaporation is much in excess of precipitation; and as fresh water only is taken up, the salt being left behind, the process, of course, tends to increase the saltness of the intertropical seas. Again, in polar and extratropical regions the reverse is the case; precipitation is in excess of evaporation. This tends in turn to diminish the saltness of the waters of those regions. See on these points §§ 31, 33, 34, 37, 179, 517, 526, and 552.

In the system of circulation produced by difference of temperature, as we have already seen, the surface-currents flow from the equator to the poles, and the under or return currents from the poles to the equator; but in the system produced by difference of saltness, the surface-currents flow from the poles to the equator, and the return undercurrents from the equator to the poles. That the surface-currents produced by difference of saltness flow from the poles to the equator, Maury thinks is evident for the two following reasons:—

(1) As evaporation is in excess of precipitation in intertropical regions, more water is taken off the surface of the ocean in those regions, than falls upon it in the form of rain. This excess of water falls in the form of rain on temperate and polar regions, where, consequently, precipitation is in excess of evaporation. The lifting of the water off the equatorial regions and its deposit on the polar tend to lower the level of the ocean in equatorial regions and to raise the level in polar; consequently, in order to restore the level of the ocean, the surface-water at the polar regions flows towards the equatorial regions.

(2) As the water taken up at the equator is fresh, and the salt is left behind, the ocean, in intertropical regions, is thus made saltier and consequently denser. This dense water, therefore, sinks and passes away as an undercurrent. This water, evaporated from intertropical regions, falls as fresh and lighter water in temperate and polar regions; and therefore not only is the level of the ocean raised, but the waters are made lighter. Hence, in order to restore equilibrium, the waters in temperate and polar regions will flow as a surface-current towards the equator. Undercurrents will flow from the equator to the poles, and surface or upper currents from the poles to the equator. Difference in temperature and difference in saltness, therefore, in every respect tend to produce opposite effects.

That the above is a fair representation of the way in which Maury supposes difference in saltness to act as a cause in the production of ocean-currents will appear from the following quotations:—

"In those regions, as in the trade-wind region, where evapo-
ration is in excess of precipitation, the general level of this sup-
posed sea would be altered, and immediately as much water as
is carried off by evaporation would commence to flow in from
north and south toward the trade-wind or evaporation region,
to restore the level" (§ 509). "On the other hand, the winds
have taken this vapour, borne it off to the extratropical regions,
and precipitated it, we will suppose, where precipitation is in
excess of evaporation. Here is another alteration of sea-level, by
elevation instead of by depression; and hence we have the motive
power for a surface-current from each pole towards the equator,
the object of which is only to supply the demand for evaporation
in the trade-wind regions" (§ 510).

The above result would follow, supposing the ocean to be
fresh. He then proceeds to consider an additional result that
follows in consequence of the saltiness of the ocean.

"Let evaporation now commence in the trade-wind region, as
it was supposed to do in the case of the fresh-water seas, and as
it actually goes on in nature—and what takes place? Why, a
lowering of the sea-level as before. But as the vapour of salt
water is fresh, or nearly so, fresh water only is taken up from
the ocean; that which remains behind is therefore more salt.
Thus, while the level is lowered in the salt sea, the equilibrium
is destroyed because of the saltiness of the water; for the water
that remains after evaporation takes place is, on account of the
solid matter held in solution, specifically heavier than it was
before any portion of it was converted into vapour" (§ 517).

"The vapour is taken from the surface-water; the surface-
water thereby becomes more salt, and, under certain conditions,
heavier. When it becomes heavier, it sinks; and hence we
have, due to the salts of the sea, a vertical circulation, namely, a
descent of heavier—because saltier and cooler—water from the
surface, and an ascent of water that is lighter—because it is not
so salt—from the depths below" (§ 518).

In section 519 he goes on to show that this vapour removed
from the intertropical region is precipitated in the polar regions,
where precipitation is in excess of evaporation. "In the precip-
itating regions, therefore, the level is destroyed, as before ex-
plained, by elevation, and in the evaporating regions by depres-
sion; which, as already stated, gives rise to a system of surface-
currents, moved by gravity alone, from the poles towards the
equator" (§ 520).

"This fresh water being emptied into the Polar Sea and agi-
tated by the winds, becomes mixed with the salt; but as the agita-
tion of the sea by the winds is supposed to extend to no great
depth, it is only the upper layer of salt water, and that to a mo-
derate depth, which becomes mixed with the fresh. The specific
gravity of this upper layer, therefore, is diminished just as much as the specific gravity of the sea-water in the evaporating regions was increased. And thus we have a surface-current of saltish water from the poles towards the equator, and an undercurrent of water saltier and heavier from the equator to the poles' (§ 522).

"This property of saltiness imparts to the waters of the ocean another peculiarity, by which the sea is still better adapted for the regulation of climates, and it is this: by evaporating fresh water from the salt in the tropics, the surface-water becomes heavier than the average of sea-water. This heavy water is also warm water; it sinks, and being a good retainer, but a bad conductor of heat, this water is employed in transporting through undercurrents heat for the mitigation of climates in far distant regions" (§ 526).

"For instance, let us suppose the waters in a certain part of the torrid zone to be 90°, but, by reason of the fresh water which has been taken from them in a state of vapour, and consequently, by reason of the proportionate increase of salts, these waters are heavier than waters that may be cooler, but not so salt. This being the case, the tendency would be for this warm but salt and heavy water to flow off as an undercurrent towards the polar or some other regions of lighter water" (§ 554).

That Maury supposes the warm water at the equator to flow to the polar regions as an undercurrent is further evident from the fact that he maintains that the climate of the arctic regions is mitigated by a warm undercurrent, which comes from the equatorial regions, and passes up through Davis Straits. See §§ 534–544.

The question now suggests itself: to which of these two antagonistic causes does Maury really suppose ocean-currents must be referred? Whether does he suppose, difference in temperature or difference in saltiness, to be the real cause? I have been unable to find any thing from which we can reasonably conclude that he prefers the one cause to the other. It would seem that he regards both as real causes, and that he has failed to perceive that the one is destructive of the other. But it is difficult to conceive how he could believe that the sea in equatorial regions, by virtue of its higher temperature, is lighter than the sea in polar regions, while at the same time it is not lighter but heavier, in consequence of its greater saltiness—how he could believe that the warm water at the equator flows to the poles as an upper current, and the cold water at the poles to the equator as an undercurrent, while at the same time the warm water at the equator does not flow to the poles as a surface-current, nor the cold water at the poles to the equator as an undercurrent, but the reverse. But, unless these absolute impossibilities be
possible, how then can an ocean-current be the result of both causes?

The only explanation of the matter appears to be that Maury has failed to perceive the contradictory nature of his two theories. This fact is particularly seen when he comes to apply his two theories to the case of the Gulf-stream. He maintains, as has already been stated, that the waters of the Gulf-stream are saltier than the waters of the sea through which they flow (see §§ 8, 28, 29, 30, 34, and several other places). And he states that one of the chief causes of the Gulf-stream is this, that "we have on one side the Caribbean Sea and Gulf of Mexico, with their waters of brine; on the other the great Polar Basin, the Baltic, and the North Sea, the two latter with waters that are but little more than brackish. In one set of these sea-basins the water is heavy; in the other it is light. Between them the ocean intervenes; but water is bound to seek and to maintain its level; and here, therefore, we unmask one of the agents concerned in causing the Gulf-stream" (§ 38). There can be no doubt whatever that it is the density of the waters of the Gulf-stream at its fountain-head, the Gulf of Mexico, resulting from its superior saltness, and the deficiency of density of the waters in polar regions and the North Sea &c., that is here considered to be unmasked as one of the agents. If this be a cause of the motion of the Gulf-stream, how then can the difference of temperature between the waters of intertropical and polar regions assist as a cause? This difference of temperature will simply tend to undo all that has been done by difference of saltness; for it will tend to make the waters of the Gulf of Mexico lighter, and the waters of the polar regions heavier. But Maury maintains, as we have seen, that this difference of temperature is also a cause, which shows that he does not perceive the contradiction.

This is still further apparent. Maury maintains, as stated, that "the waters of the Gulf-stream are saltier than the waters of the sea through which they flow," and that this excess in saltness, by making the water heavier, is a cause of the motion of the stream. But he maintains that, notwithstanding the effect which greater saltness has in increasing the density of the waters of the Gulf-stream, yet, owing to their higher temperature, they are actually lighter than the water through which they flow; and as a proof that this is the case, he adduces the fact that the surface of the Gulf-stream is roof-shaped (§§ 39–41), which it could not be were its waters not actually lighter than the waters through which the streams flow. So it turns out, in contradiction to what he had already stated, that it is the lesser density of the waters of the Gulf-stream that is the real cause of
their motion. The greater saltness of the waters, to which he attributes so much, can in no way be regarded as a cause of motion. Its effect, so far as it goes, is to stop the motion of the stream rather than to assist it.

But, again, although Maury maintains that difference of salt-ness and difference of temperature are both causes of ocean-currents, yet he appears actually to admit that temperature and saltness neutralize each other so as to prevent change in the specific gravity of the ocean, as will be seen from the following quotation:—

"It is the trade-winds, then, which prevent the thermal and specific-gravity curves from conforming with each other in intertropical seas. The water they suck up is fresh water; and the salt it contained, being left behind, is just sufficient to counterbalance, by its weight, the effect of thermal dilatation upon the specific gravity of sea-water between the parallels of 34° north and south. As we go from 34° to the equator, the water grows warmer and expands. It would become lighter; but the trade-winds, by taking up vapour without salt, make the water salter, and therefore heavier. The conclusion is, the proportion of salt in sea-water, its expansibility between 63° and 83°, and the thirst of the trade-winds for vapour are, where they blow, so balanced as to produce perfect compensation; and a more beautiful compensation cannot, it appears to me, be found in the mechanism of the universe than that which we have here stumbled upon. It is a triple adjustment: the power of the sun to expand, the power of the winds to evaporate, and the quantity of salts in the sea—these are so proportioned and adjusted that when both the wind and the sun have each played with its forces upon the intertropical waters of the ocean, the residuum of heat and of salt should be just such as to balance each other in their effects; and so the aqueous equilibrium of the torrid zone is preserved" (§ 436, eleventh edition).

"Between 35° or 40° and the equator evaporation is in excess of precipitation; and though, as we approach the equator on either side from these parallels, the solar ray warms and expands the surface-water of the sea, the winds, by the vapour they carry off, and the salt they leave behind, prevent it from making that water lighter" (§ 437, eleventh edition).

"Philosophers have admired the relations between the size of the earth, the force of gravity, and the strength of fibre in the flower-stalks of plants; but how much more exquisite is the system of counterpoises and adjustments here presented between the sea and its salts, the winds and the heat of the sun!" (§ 438, eleventh edition).

How can this be reconciled with all that precedes regarding
ocean-currents being the result of difference of specific gravity caused by a difference of temperature and difference of saltness? Here is a distinct recognition of the fact that difference in saltness, instead of producing currents, tends rather to prevent the existence of currents, by counteracting the effects of difference in temperature. And so effectually does it do this, that for 40°, or nearly 3000 miles, on each side of the equator there is absolutely no difference in the specific gravity of the ocean, and consequently nothing, either as regards difference of temperature or difference of saltness, that can possibly give rise to a current.

But it is evident that, if between the equator and latitude 40° the two effects completely neutralize each other, it is not at all likely that between latitude 40° and the poles they will not to a very large extent do the same thing. And if so, how can ocean-currents be due either to difference in temperature or to difference in saltness, far less to both. If there be any difference of specific gravity of the ocean between latitude 40° and the poles, it must be only to the extent by which the one cause has failed to neutralize the other. If, for example, the waters in latitude 40°, by virtue of higher temperature, are less dense than the waters in the polar regions, they can be so only to the extent that difference in saltness has failed to neutralize the effect of difference in temperature. And if currents result, they can do so only to the extent that difference in saltness has thus fallen short of being able to produce complete compensation. Maury, after stating his views on compensation, seems to become aware of this; but, strangely, he does not appear to perceive, or, at least, he does not make any allusion to the fact, that all this is fatal to the theories he had been advancing about ocean-currents being the combined result of differences of temperature and difference of saltness. For, in opposition to all that he had previously advanced regarding the difficulty of finding a cause sufficiently powerful to account for such currents as the Gulf-stream, and the great importance that difference in saltness had in the production of currents, he now begins to maintain that so great is the influence of difference in temperature in causing currents that difference in saltness, and a number of other compensating causes are actually necessary to prevent the ocean-currents from becoming too powerful.

"If all the intertropical heat of the sun," he says, "were to pass into the seas upon which it falls, simply raising the temperature of their waters, it would create a thermo-dynamical force in the ocean capable of transporting water scalding hot from the torrid zone, and spreading it while still in the tepid state around the poles . . . ." Now, suppose there were no
trade-winds to evaporate and to counteract the dynamical force of the sun, this hot and light water, by becoming hotter and lighter, would flow off in currents with almost mill-tail velocity towards the poles, covering the intervening sea with a mantle of warmth as a garment. The cool and heavy water of the polar basin, coming out as undercurrents, would flow equatorially with equal velocity."

"Thus two antagonistic forces are unmasked, and, being unmasked, we discover in them a most exquisite adjustment—a compensation—by which the dynamical forces that reside in the sun-beam and the trade-wind are made to counterbalance each other, by which the climates of intertropical seas are regulated, and by which the set, force, and volume of oceanic currents are measured" (§§ 437 and 438, eleventh edition).

The force resulting from difference of specific gravity not sufficient to produce motion.—I shall now consider whether the forces to which Maury appeals have the potency that he attributes to them. Is the force derived from the difference of specific gravity between the waters of the ocean in intertropical and polar regions sufficient to account for the motion of ocean-currents?

The utter inadequacy of this cause has been so clearly shown by Sir John Herschel, that one might expect that little else would be required than simply to quote his words on the subject, which are as follows:

"First, then, if there were no atmosphere, there would be no Gulf-stream, or any other considerable ocean-current (as distinguished from a mere surface-drift) whatever. By the action of the sun's rays, the surface of the ocean becomes most heated, and the heated water will, therefore, neither directly tend to ascend (which it could not do without leaving the sea) nor to descend, which it cannot do, being rendered buoyant, nor to move laterally, no lateral impulse being given, and which it could only do by reason of a general declivity of surface, the dilated portion occupying a higher level. Let us see what this declivity would amount to. The equatorial surface-water has a temperature of 84°. At 7200 feet deep the temperature is 39°, the level of which temperature rises to the surface in latitude 56°. Taking the dilatability of sea-water the same as that of fresh, a uniformly progressive increase of temperature, from 39° to 84° Fahr., would dilate a column of 7200 feet by 10 feet, to which height, therefore, above the spheroid of equilibrium (or above the sea-level in lat. 56°), the equatorial surface is actually raised by dilatation. An arc of 56° on the earth's surface measures 3560 geographical miles; so that we have a slope of 1-28th of an inch per geographical mile, or 1-32nd of an inch per statute mile for the water so raised to run down. As the accelerating
force corresponding to such a slope (of 1-10th of a second, $0^{\circ}1\text{.1}$) is less than one two-millionth part of gravity, we may dismiss this as a cause capable of creating only a very trifling surface-drift, and not worth considering, even were it in the proper direction to form, by concentration, a current from east to west, which it would not be, but the very reverse."—Physical Geography, article 57.)

It is singular how any one, even though he regarded this conclusion as but a rough approximation to the truth, could entertain the idea that ocean-currents can be the result of difference in specific gravity. There are, however, one or two reasons which may be assigned why the above has not been generally received as conclusive. These calculations refer to the difference of gravity resulting from difference of temperature; but this is only one of the causes to which Maury appeals, and even not the one to which he most frequently alludes. Maury insists so strongly on the effects of difference of saltiness, that many would no doubt suppose that, although Herschel may have shown that difference in specific gravity arising from difference of temperature could not account for the motion of ocean-currents, yet nevertheless this, combined with the effects resulting from difference in saltiness, might account for their motion. This, of course, would not be the case with those who perceived the contradictory nature of Maury's two causes; but most people probably read the 'Physical Geography of the Sea' without being aware that the one cause is destructive of the other. Another reason is, a few very plausible-looking objections have been strongly urged by Maury and others against the theory that ocean-currents can be caused by the impulses of the trade-winds, which have not been duly considered; and probably these objections appear to many as formidable against this theory as Herschel's arguments appear against Maury's theories.

There is one slight objection to Herschel's result: he takes 39° as the temperature of maximum density. This, however, as we shall see, does not materially affect his conclusions.

Observations on the temperature of the maximum density of sea-water have been made by Erman, Despretz, Rossetti, Neumann, Marcet, Hubbard, Horner, and others. No two of them have arrived at exactly the same conclusion. This probably results from the fact that the temperature of maximum density depends upon the amount of salt held in solution. No two seas, unless they are equal as to saltiness, have the same temperature of maximum density. The following Table of Despretz will show how rapidly the temperature of both the freezing-point and of maximum density is lowered by additional amounts of salt.
He found the temperature of maximum density of sea-water, whose density at 20° C. was 1·0273, to be −3°67 C. (25°4 F.), and the temperature of freezing-point −2°55 (27°4 F.)*. Somewhere between 25° and 26° F. may therefore be regarded as the temperature of maximum density of sea-water of average saltness. We have no reason to believe that the ocean, from the surface to the bottom, even at the poles, is at 27°4 F., the freezing-point. An error to the extent of a degree or two, however, will not materially affect the conclusion at which we may arrive. Let us therefore assume the temperature of the ocean at the poles to be 32°, and the surface-temperature at the equator to be 80°. Maury states that at the depth of 7200 feet at the equator the temperature is about 36° (§ 440, eleventh edition). Although this agrees pretty nearly with the results arrived at by several observers who have attempted to determine the temperature of the ocean at great depths in equatorial regions, still 36°, the temperature assigned at 7200 feet below the surface, is probably too high; for these observations were made with thermometers unprotected from the pressure of the water on their bulbs, which at so great a depth would equal more than 200 atmospheres; 32°, at a depth of 7200 feet, may probably be nearer the truth than 36°. But we shall assume that we must descend to a depth of, say, 10,000 feet, before the temperature of 32°, that of the poles, is reached. Let us also assume that the temperature decreases at a uniform rate from the surface downwards to that depth. Calculating, then, from Muncke’s Table of the Expansion of Sea-water, we have about 18 feet as the height at which the water at the equator stands above the level of the ocean at the poles. The distance from the equator to the poles is about 6200 miles. The force impelling the water down this slope of 18 feet in 6200 miles would therefore be equal to about \( \frac{1.5250000}{6200} \) that of gravity. For example, the force impelling a cubic foot (64 lbs.) of water at the surface of the ocean would scarcely be equal to the weight of one-fourth of a grain.

But in reality it would not nearly equal this; for we have been assuming in our calculations that the temperature of the


ocean at the equator decreases at a uniform rate from the surface downwards, which is far from being the case. The rate of decrease is most rapid at the surface, and decreases as we descend. The principal part of the decrease of temperature takes place within no very great depth from the surface; consequently the greater part of the excess of temperature at the equator over that at the poles affects the sea to no great depth. But there is another reason why the expansion of the waters at the equator cannot amount to near 18 feet. It is this: the rate at which water expands as its temperature rises is not uniform, but increases with the temperature. Sea-water, according to Muncke's Table, in rising for example from 32° to 42° expands 0.00047, whereas in rising from 70° to 80° it expands no less than 0.00152. But these higher temperatures affect only a small quantity of water near the surface; the great depth of water below is affected by the lower temperatures, which do not produce much expansion. As no reliable observations, so far as I am aware, have been made to ascertain the rate at which the temperature of the waters at the equator decreases from the surface downwards to great depths, it is impossible to determine with any thing like accuracy the height at which the ocean, in virtue of higher temperature, should stand above the level of the ocean at the poles. But one thing we are certain of is that it must be very much under 18 feet, and that the force acting on the waters of the ocean to impel them forward as a current resulting from the difference of specific gravity between the sea in intertropical and polar regions, is very much under one-fourth of a grain per cubic foot. And if the sea in intertropical regions is much saltier than the sea in polar regions, as Maury strongly insists, then this will make the force still less; for this will go so far to neutralize the effects due to difference of temperature between the waters of equatorial and polar regions.

It is perfectly evident that a pressure of one-fourth of a grain on the cubic foot of water, were it even so great as that, would be totally inadequate to overcome the mere molecular resistance of the water to go into motion, far less to produce the great currents of the ocean. It is therefore certain that ocean-currents are in no way whatever due to differences of specific gravity.

But it must be observed that this force of one-fourth of a grain per cubic foot would affect only the water at the surface; a very short distance below the surface the force would be absolutely insensible.

If water were perfectly fluid, and offered no resistance to motion, it would not only flow down an incline, however small it might be, but would flow down with an accelerated motion.
But water is not a perfect fluid, and its molecules do offer considerable resistance to motion. Water flowing down an incline, however steep it may be, soon acquires a uniform motion. There must therefore be a certain inclination below which no motion can take place. Experiments were made by M. Dubuat, with the view of determining this limit*. He found that when the inclination was 1 in 500,000, the motion of the water was barely perceptible; and he came to the conclusion that when the inclination is reduced to 1 in 1,000,000, all motion ceases. But the inclination afforded by the difference of temperature between the sea in equatorial and polar regions does not exceed the half of this, and consequently it can have absolutely no effect whatever in producing currents, no, not even the "trifling surface-drift" which Sir John Herschel is willing to attribute to it.

There is an error into which some writers appear to fall to which I may here refer. Suppose that at the equator we have to descend 10,000 feet before water equal in density to that at the poles is reached. We have in this case a plain with a slope of 10,000 feet in 6200 miles, forming the upper surface of the water of maximum density. Now this slope exercises no influence in the way of producing a current, as some seem to suppose; for this is not a case of disturbed equilibrium, but the reverse. This slope is the condition of static equilibrium when there is a difference between the temperature of the water at the equator and the poles. The only slope that has any tendency to produce motion of the water is the slope formed by the surface of the ocean in the equatorial regions being higher than the surface at the poles; but this is a slope of only 18 feet in 6200 miles.

Objections to Dr. Carpenter's theory of a general interchange of equatorial and polar waters.

Lieut. Maury's theory of a general interchange of water between the equator and the poles resulting from a difference of specific gravity, caused by difference of temperature, has lately been advocated by Dr. Carpenter†. He considers that the great masses of warm water found by him and his colleagues in their late important dredging-expeditions in the depths of the North Atlantic must be referred, not to the Gulf-stream, but to a general movement of water from the equatorial regions. "The inference seems inevitable," he says,

* Dubuat's 'Hydraulique,' tome i. p. 64 (1516). See also British Association Report for 1834, pp. 422, 451.
Mr. J. Croll on the Physical Cause of Ocean-currents.

"that the bulk of the water in the warm area must have come thither from the south-west. The influence of the Gulf-stream proper (meaning by this the body of superheated water which issues through the 'Narrows' from the Gulf of Mexico), if it reaches this locality at all (which is very doubtful), could only affect the most superficial stratum; and the same may be said of the surface-drift caused by the prevalence of south-westerly winds, to which some have attributed the phenomena usually accounted for by the extension of the Gulf-stream to these regions. And the presence of the body of water which lies between 100 and 600 fathoms deep, and the range of whose temperature is from 48° to 42°, can scarcely be accounted for on any other hypothesis than that of a great general movement of equatorial water towards the Polar area, of which movement the Gulf-stream constitutes a peculiar case modified by local conditions. In like manner the Arctic stream which underlies the warm superficial stratum in our cold area constitutes a peculiar case, modified by the local conditions to be presently explained, of a great general movement of polar water towards the equatorial area, which depresses the temperature of the deepest parts of the great oceanic basins nearly to the freezing-point."

In support of this theory of a general movement of water between equatorial and polar regions, Dr. Carpenter adduces the authority of Humboldt and of Prof. Buff*. I have been unable to find any thing in the writings of either from which it can be inferred that they have given this matter special consideration. Humboldt merely alludes to the theory, and that in the most casual manner; and that Prof. Buff has not carefully investigated the subject is apparent from the very illustration quoted by Dr. Carpenter from the 'Physics of the Earth.' "The water of the ocean at great depths," says Prof. Buff, "has a temperature, even under the equator, nearly approaching to the freezing-point. This low temperature cannot depend on any influence of the sea-bottom. . . . . The fact, however, is explained by a continual current of cold water flowing from the polar regions towards the equator. The following well-known experiment clearly illustrates the manner of this movement. A glass vessel is to be filled with water with which some powder has been mixed, and is then to be heated at bottom. It will soon be seen, from the motion of the particles of powder, that currents are set up in opposite directions through the water. Warm water rises from the bottom up through the middle of the vessel, and spreads over the surface, while the colder and therefore heavier liquid falls down at the sides of the glass."

This illustration is evidently intended to show not merely the

form and direction of the great system of oceanic circulation, but also the way that the circulation is caused by heat. It is no doubt true that if we apply heat (say that of a spirit-lamp) to the bottom of a vessel filled with water, the water at the bottom of the vessel will become heated and rise to the surface; and if the heat be continued an ascending current of warm water will be generated; and this, of course, will give rise to a compensating undercurrent of colder water from all sides. In like manner it is also true that, if heat were applied to the bottom of the ocean in equatorial regions, an ascending current of hot water would be also generated, giving rise to an undercurrent of cold water from the polar regions. But all this is the diametrically opposite of what actually takes place in nature. The heat is not applied to the bottom of the ocean, so as to make the water there lighter than the water at the surface, and thus to generate an ascending current; but the heat is applied to the surface of the ocean, and the effect of this is to prevent an ascending current rather than to produce one, for it tends to keep the water at the surface lighter than the water at the bottom. In order to show how the heat of the sun produces currents in the ocean, Prof. Buff should have applied the heat, not to the bottom of his vessel, but to the upper surface of the water. But this is not all, the form of the vessel has something to do with the matter. The wider we make the vessel in proportion to its depth, the more difficult is it to produce currents by means of heat. But in order to represent what takes place in nature, we ought to have the same proportion between the depth and the superficial area of the water in our vessel as there is between the depth and the superficial area of the sea. The mean depth of the sea, according to Sir John Herschel, may be taken at about four miles*. It may be somewhat more, or it may be somewhat less, than this; but that will not materially affect our result. The distance between pole and pole we shall take in round numbers as 12,000 miles. The sun may therefore be regarded as shining upon a circular sea 12,000 miles in diameter and four miles deep. The depth of the sea to its diameter is therefore as 1 to 3000. Suppose, now, that in our experiment we make the depth of our vessel 1 inch, we shall require to make its diameter 3000 inches, or 250 feet. Let us, then, take a pool of water 250 feet in diameter, and 1 inch deep. Suppose the water to be at 32°. Apply heat to the upper surface of the pool, so as to raise the temperature of the surface of the water to 80° at the centre of the pool, the temperature diminishing towards the edge, where it is at 32°. It is found that at a depth of two miles the tempera-

* Physical Geography, article 17.
ture of the water at the equator is about as low as that of the poles. We must therefore suppose the water at the centre of our pool to diminish in temperature from the surface downwards, so that at a depth of half an inch the water is at 32°. We have in this case a thin layer of warm water half an inch thick at the centre, and gradually thinning off to nothing at the edge of the pool. The lightest water, be it observed, is at the surface, so that an ascending or a descending current is impossible. The only way whereby the heat applied can have any tendency to produce motion is this:—The heating of the water expands it, consequently the surface of the pool must stand at a little higher level at its centre than at its edge, where no expansion takes place; and therefore, in order to restore the level of the pool, the water at the centre will tend to flow towards the sides. But what is the amount of this tendency? Is it sufficient to overcome the molecular resistance of the water to go into motion? The amount of this tendency depends upon the amount of the slope. We have already seen that unless the slope exceeds 1 in 1,000,000, no motion can take place; but the slope in the case under consideration amounts to only 1 in 1,820,000; consequently motion is absolutely impossible.

That the great masses of warm water found by Dr. Carpenter in the North Atlantic cannot be due to currents produced by difference of temperature, as he supposes, can be proved in another way.

According to his theory there ought to be as much warm water flowing from intertropical regions towards the Antarctic regions as towards the Arctic. We may therefore, in our calculations, consider that the heat which is received in tropical regions to the south of the equator goes to warm the southern hemisphere, and the heat which it receives on the north side of the equator goes to warm the northern hemisphere. The warm currents found in the North Atlantic in temperate regions we may conclude came from the regions lying to the north of the equator—or, in other words, from that part of the Atlantic lying between the equator and the tropic of Cancer. At least, according to Dr. Carpenter’s theory, we have no reason to believe that the quantity of warm water flowing from the tropical regions to the temperate and polar in the Atlantic is greater than the area between the equator and the tropic of Cancer can supply—because he maintains that a very large proportion of the cold water found in the North Atlantic came, not from the Arctic, but from the Antarctic regions. But if the North Atlantic is cooled by a cold stream from the southern hemisphere, the southern hemisphere in turn must be heated by a warm current from the North Atlantic—unless we assume, which is very
improbable, that the compensating current flowing from the Atlantic into the southern hemisphere is as cold as the Antarctic current. But Dr. Carpenter admits that the quantity of warm water flowing from the Atlantic in equatorial regions towards the south is even greater than towards the north. "The unrestricted communication," he says, "which exists between the Antarctic area and the great Southern Ocean-basins would involve, if the doctrine of a general oceanic circulation be admitted, a much more considerable interchange of waters between the Antarctic and the Equatorial areas than is possible in the northern hemisphere"*. And as a proof that this is actually the case, he adduces the fact known to navigators that in the Southern Ocean there is a perceptible "set" of warm surface-water towards the Antarctic Pole.

We have already seen that, were it not for the great mass of warm water which finds its way to the polar regions, the temperature of these regions would be enormously lower than they really are. It was seen also that the comparatively high temperature of North-eastern Europe was due also to the same cause. But if it is doubtful whether the Gulf-stream reaches our shores, and if it is true that, even supposing it did, it "could only affect the most superficial stratum," and that the great mass of warm water found by Dr. Carpenter in his dredging-expeditions came directly from the equatorial regions, and not from the Gulf-stream, then the principal part of the heating-effect must be attributed, not to the Gulf-stream, but to the general flow of water from the equatorial regions. It surely would not, then, be too much to assume that the quantity of heat conveyed from equatorial regions by this general flow of water into the North Atlantic is at least equal to that conveyed by the Gulf-stream. Let us, then, assume that the total quantity of heat conveyed from equatorial regions into the North Atlantic and Arctic Ocean by all the various processes, the Gulf-stream included, is equal to twice that conveyed by the Gulf-stream.

We shall now consider whether the area of the Atlantic to the north of the equator is sufficient to supply the amount of heat demanded by Dr. Carpenter's theory.

The entire area of the Atlantic, extending from the equator to the tropic of Cancer, including the Caribbean Sea and the Gulf of Mexico, is about 7,700,000 square miles. In a former part of this paper† it was shown that, even assuming the volume of the Gulf-stream to be considerably less than one half what either Sir John Herschel or Lieut. Maury estimates it to be, the quantity of heat conveyed by the stream through the Straits

† Phil. Mag. S. 4. vol. xxxix. p. 89.
of Florida is equal to all the heat received from the sun by 1,560,935 square miles at the equator. The annual quantity of heat received from the sun by the torrid zone per unit surface, taking the mean of the whole zone, is to that received by the equator as 39 to 40, consequently the quantity of heat conveyed by the Gulf-stream is equal to all the heat received by 1,600,960 square miles of the Atlantic in the torrid zone.

Dr. Carpenter is mistaken in supposing that "all the calculations which have been made as to the quantity of water which issues from the Narrows, and the amount of heat which it conveys, are based upon the assumption that both its temperature and its rate of movement are the same throughout its depths as they are at its surface". The surface-temperature of the stream at the Narrows is somewhat about 85°; but I have taken the mean temperature of the water at this place as only 65°. The cold return current, according to Dr. Carpenter, has a temperature as low as 30° or 32°; but, not to overestimate the quantity of heat derived from the Gulf-stream, I have taken the return current at 40°. In this case the quantity of heat conveyed through the Narrows I estimate to be 25 thermal units per pound of water. But had I taken the surface-temperature of the stream and Dr. Carpenter's estimate as to the temperature of the cold return current, I should have had 53 or 55 thermal units per pound as the amount conveyed. My data were derived, not from popular treatises on physical geography, but from a careful analysis of the sections and charts of the United-States Coast Survey; and any one who will be at the trouble to examine these will easily satisfy himself that I have underestimated both the temperature and volume of the stream.

But if, according to Dr. Carpenter's views, the quantity of heat conveyed from the tropical regions is double that conveyed by the Gulf-stream, the amount of heat in this case conveyed into the Atlantic in temperate regions will be equal to all the heat received from the sun by 3,201,920 square miles of the Atlantic between the equator and the tropic of Cancer. This is \(\frac{3}{7}\) of all the heat received from the sun by that area.

Taking the annual quantity received per unit surface at the equator at 1000, the quantities received by the three zones would be respectively as follows:

<table>
<thead>
<tr>
<th>Zone</th>
<th>Quantity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Equator</td>
<td>1000</td>
</tr>
<tr>
<td>Torrid zone</td>
<td>975</td>
</tr>
<tr>
<td>Temperate zone</td>
<td>757</td>
</tr>
<tr>
<td>Frigid zone</td>
<td>454</td>
</tr>
</tbody>
</table>

Now, if we remove from the Atlantic in tropical regions \(\frac{3}{7}\)

of the heat received from the sun, we remove 405 parts from every 975 received from the sun, and consequently only 570 parts per unit surface remain.

It has been already shown that the quantity of heat conveyed by the Gulf-stream from the equatorial regions into the temperate regions is equal to \(\frac{100}{412}\) of all the heat received by the Atlantic in temperate regions*. But according to the theory under consideration the quantity removed is double this, or equal to \(\frac{100}{200}\) of all the heat received from the sun. But the quantity received from the sun is equal to 757 parts per unit surface; add then to this \(\frac{100}{200}\) of 757, or 367, and we have 1124 parts of heat per unit surface as the amount possessed by the Atlantic in temperate regions. The Atlantic should in this case be much warmer in temperate regions than in tropical; for in temperate regions it possesses 1124 parts of heat per unit surface, whereas in tropical regions it possesses only 570 parts per unit surface. Of course the heat conveyed from tropical regions does not all remain in temperate regions; a very considerable portion of it must pass into the arctic regions. Let us, then, assume that one half goes to warm the Arctic Ocean, and the other half remains in the temperate regions. In this case 183·5 parts would remain, and consequently 757 + 183·5 = 940·5 parts would be the quantity possessed by the Atlantic in temperate regions, a quantity which still exceeds by no less than 370·5 parts the heat possessed by the Atlantic in tropical regions.

As one half of the amount of heat conveyed from the tropical regions is assumed to go into the Arctic Ocean, the quantity passing into that ocean would therefore be equal to what passes through the Straits of Florida, which amount we have already found to be equal to all the heat received from the sun by 6,873,800 square miles of the arctic regions†. But taking the volume of the Gulf-stream, as already stated, at one half our original estimate, the quantity of heat passing into the Arctic Ocean would therefore be equal to all the heat received by 3,436,900 square miles of the Arctic Ocean. The entire area covered by sea beyond the arctic circle is under 5,000,000 square miles; but taking the Arctic Ocean in round numbers at 5,000,000 square miles, the quantity of heat conveyed into it by currents to that received from the sun would therefore be as 3,436,900 to 5,000,000.

The amount received on the unit surface of the arctic regions we have seen to be 454 parts. The amount received from the currents would therefore be 312 parts. This gives 766 parts of heat per unit surface as the quantity possessed by the Arctic

† Ibid. p. 84.
Ocean. Then the Arctic Ocean also would possess more heat than the Atlantic in tropical regions; for the Atlantic in these regions possesses only 570 parts, whereas the Arctic Ocean possesses 766 parts. It is true that more rays are cut off in arctic regions than in tropical; but still, after making due allowance for this, the Arctic Ocean, if Dr. Carpenter's theory be correct, ought to be as warm as, if not warmer than, the Atlantic in tropical regions.

We may therefore conclude that there can be no such large quantity of warm water, in addition to that of the Gulf-stream, as Dr. Carpenter supposes, flowing into the North Atlantic from the equatorial regions; for there is not heat in those regions sufficient to supply such a current. We may also conclude that, at least in respect of the Atlantic, it is not correct that there is more warm water flowing from the equatorial regions into the southern hemisphere than into the northern; for a very large proportion of the heat conveyed by the Gulf-stream is derived from the southern hemisphere. In fact the great equatorial current, the feeder of the Gulf-stream, comes from the southern hemisphere.

The entire area of 7,700,000 square miles of sea in equatorial regions lying to the north of the equator would not be sufficient to supply the current passing through the Narrows of Bahama. Were the heat of the Gulf-stream all derived from the northern hemisphere, the following would then represent the relative quantities of heat per unit surface possessed by the Atlantic in the three zones, assuming that one half of the heat of the Gulf-stream passes into the arctic regions, and the other half remains to warm the temperate regions:

- From the Equator to the Tropic of Cancer . . . 773
- From the Tropic of Cancer to the Arctic Circle . . . 848
- From the Arctic Circle to the North Pole . . . 610

These figures show that, were it not that a very large proportion of the heat possessed by the Gulf-stream is derived from the southern hemisphere, the Atlantic, from the equator to the tropic of Cancer, would be as cold as from the tropic of Cancer to the North Pole.

The comparatively high temperature which prevails in the northern parts of the Atlantic and in the Arctic seas is therefore to a considerable extent due to heat derived from the southern hemisphere. And no doubt this transference of heat from the southern hemisphere to the northern by means of ocean-currents, as was mentioned on a former occasion *, is the cause

* Phil. Mag. vol. xxxix. p. 103,
why the mean temperature of the southern hemisphere is so much lower than that of the northern.

We shall now proceed to consider the objections which have been urged against the theory that ocean-currents are due to the impulse of the trade-winds.

[To be continued.]

Erratum.

In Part I. of this paper, vol. xxxix. p. 89, 8th line from bottom, for 9.83 read 9.08.

XXX. On Statical and Dynamical Ideas in Chemistry.—Part II. Chemical Substance and Chemical Functions. By EDMUND J. MILLS, D.Sc.*

In the preceding Part the history of the ideas connected with acid, alkali (base), and salt was concisely stated, and it was shown that while, on the one hand, those ideas are erroneous and self-contradictory when they designate something particular, so, on the other hand, the most consistent and general theory that has been stated with respect to them is that of Avogadro, who is their modern expounder in the sense of chemical polarity. These results were in harmony with the idea of motion, the criterion adopted in these papers. The practical result is that there is no such thing as an acid, base, or salt, though the use of the adjectives and qualitative nouns derived from these terms might probably be successfully defended. If any one deny this conclusion he is bound to give a satisfactory definition of an acid, for example—a task in which, as history clearly shows, success is unlikely to accrue.

Having thus pointed out the value of the idea of motion in the concrete sphere of external chemistry, I may now penetrate, or perhaps ascend, to the remoter regions of Chemical Substance and Chemical Functions, where the service of the same idea will prove available.

1. Chemical Substance.

We are accustomed, in the language of everyday chemistry, to say that such and such bodies or substances undergo certain operations; sulphur, hydric nitrate, aniline, &c. are spoken of as bodies or substances indifferently. In recording the facts of an analysis (even of a mechanical mixture), it is customary to say that so much substance contained or furnished so much of a product; and this product may be volatile matter or organic matter, which, in its turn, may become substance for analysis. A che-

* Communicated by the Author. For Part I. see Phil. Mag. 1869, vol. xxxvii. p. 461.
Dr. E. J. Mills on Chemical Substance

mical substance, however, is always understood to mean an object that is chemically homogeneous in its own class. But the name is refused to a mechanical mixture; and I have often heard it stated, in that sense, "this is not a chemical substance," or, more curtly, "this is not a substance." Chemical body and chemical matter are phrases that seldom occur, even in a particular signification. On the whole it appears that "substance," considered as a chemical term, is used in two ways:—(1) as a loose expression*, synonymous with "body" or "matter," themselves being then loose expressions; but more especially (2) as indicating a specific scientific distinction. This latter employment is worthy of attentive examination.

The elder chemistry, possessing few known objects for experiment, had regarded more their fundamental unity than their individual diverseness. Then, as at the present day, but few chemists were cultivators of philosophy, so that the general prevalence of that philosophical regard was rather an accident than a merit. Indeed it can often be traced, almost with certainty, to a belief in the primitive unity of human races, and the tree of life in the midst of the garden, doctrines whose origin is known not to have been philosophical. In modern times more definiteness has permeated the idea of substance. Qualitative analytical methods, gradually increasing in precaution and refinement, were able to demonstrate when a substance was pure; quantitative processes followed with tardy corroboration. From the latter sprang the law of definite proportions discovered by Higgins and Dalton, with its immediate consequence—the theory of chemical composition. In this manner an à priori control was acquired over quantitative analysis†. Constancy in composition was regarded as a proof of purity, and purity was inevitably attended with constancy in composition. Such was the first precise notion of a chemical substance. "Peculiar earths" might be discovered, and "peculiar elements" afterwards prepared from them; and so long as the latter were accounted as being merely means to an end (composition, namely), the notion of constancy could be universally maintained.

But what of the means themselves? Were the elements compound or absolutely simple? The prevalent tendency has been to affirm the latter alternative. Mercuric sulphide, for example, can be made by the direct addition of mercury to sulphur; but it did not appear that any two substances, on being placed in contact, produce sulphur. The polar theory, it is true, has represented sulphur and other elements as intrinsically dual, like

* Thus Naquet, *Principes de Chimie*, first edition, p. 1, says, "Ce qui constitue les corps s'appelle matière ou substance."
mercuric sulphide; but inasmuch as it has excluded, by its own fundamental condition, the existence in the free state of what (in sulphur) would correspond to the mercury and sulphur of the experiment, no chemist, probably, has ever considered the elements compound in the same sense as he has entertained that consideration in respect of other bodies. Hence it is that constancy of composition has been insensibly supplemented, perhaps supplanted, by another idea of universal applicability, namely that of homogeneousness.

The definition that a chemical substance is that which is chemically homogeneous in its own class may become more intelligible on illustration. Each element is, as has been stated, esteemed homogeneous; that is, the whole list of elementic discriminants fails to show that it consists of more than one thing, or that it can be made by putting two or more things together. Each amine is homogeneous; because the varied application of aminic discriminants, such as potassic hydrate, hydric chloride, platinic chloride, reveals one thing only. [An elementic discriminant such as an extremely high temperature, would of course remove any amine from its own class.] The definition is evidently in the main analytical. It moreover proves to be not unjust when tested by the idea of motion. For in the idea of homogeneousness there is no limit reserved; while the process and result of classification, depending partly on experiment, partly on convenience, are confessedly destitute of finality and absoluteness—indeed of any stational property.

Whatever theory may have been proposed as to the nature of substance in general, there does not appear to be any real obstacle in adding thereto the idea of homogeneousness. Spinosa, who defined substance as "that whose conception needs not the conception of another thing as necessary to its formation," Hobbes and Berkeley, whose subjective doctrine is well known, and Leibnitz, who stated substance to be "a being capable of action," have more in common than at first appears. I cannot find on examination any thing in their views or tendency excluding homogeneousness or even inconsistent with it. On the other hand, the idea of homogeneousness is preeminently chemical, no other science offering it so frequently, and none on so especially inductive a basis. When, therefore, to our idea of substance in general we add that of homogeneousness, we know what is chemical substance; and no further supplement is necessary.

The substantial in chemistry is, consequently, wholly unrelated to indivisibles; and it cannot be known or determined by theories of constitution or the canons of formulæ. For many years past chemists have been in the habit of using methods of symbolic representation which, while they undoubtedly express
numerical relations of the highest value and merit, are usually understood to mean much more than any experimenter has yet been able to adduce. Consider, for instance, the formula

\[
\begin{array}{c}
H \\ H \\ O \\
\end{array}
\]

I pass over the assertions that H, H, and O stand for atoms fixed in space or elsewhere*, one H being above the other, and the O being outside the two, and so forth. Almost every one takes that formula to mean, among other things, that water contains hydrogen and oxygen—probably because hydrogen and oxygen are obtainable from water. Yet when the hydrogen and oxygen united they underwent loss, and are consequently no more the antecedent hydrogen and oxygen than liquid chlorine is gaseous chlorine. Our symbolic system does not guard against this error. It is an evident and not an uncommon statement that we can only judge of a substance by its reactions—that is, by motion, But if so, the statical argument is abandoned, and we can neither continue to ascribe discrete parts to chemical substance nor implicate them in its formula.

The result of the preceding investigation may perhaps surprise the reader. It is now evident that, side by side with the great theory of limits, has been running a confluent stream, never mixing therewith and for the most part unnoticed. How long they may continue to flow together is a question of the greatest moment to theoretical chemistry. When the inconsistency between the atomic and homogeneous theory is more generally realized, fewer minds will be found to admit them both simultaneously.

2. Chemical Functions.

It has been shown that chemical substance cannot be regarded, and is not practically regarded, statically. Our contemplation of it is an act, the result of certain experimental acts; beyond this we know nothing; and the precise idea of it, as above arrived at, is therefore commensurate with a mode of motion.

When from chemical substance we descend to groups of chemical substances, modes of motion of subordinate generality require to be contemplated; and these are chemical functions. An inquiry into the nature of these is not without its value, and, as in the previous paper, turns at first upon the use of words.

The term *alcohol*, just like "acid," first implied a specific substance; but its meaning has grown generic with the advance of chemical discovery. The word is now applied to bodies gene-

* The atoms being regarded by all atomists as in some way fixed, though not by all as fixed in space, an extraspatial region becomes a necessity.
rally which are susceptible of easy oxidation in two or more definite stages, with the successive production of derivatives resembling acetylic hydride and hydrate; and with these prominent attributes a number of others of minor importance are usually associated. Such are, for instance, the power of yielding peculiar classes of oxides (ethers, mixed ethers, compound ethers, &c.). In order to render the name specific, a specific designation, as methyllic or amylic, is prefixed to it. But what is the logical result when we examine this operation? The word "alcohol" proves to be not the name of a thing, but the name of what a thing will do—to wit, yield a peculiar kind of derivatives. Hence it is the name of a function. In like manner, aldehyde, ketone, glycol, amine, amide, acid, base, salt, and similar designations may be shown to be pure designations of chemical functions. It must be borne in mind that I am here making an analysis of a practical use of part of chemical nomenclature as actually received by every one, just as in 1. the current ideas on chemical substances were accepted as a groundwork; and the result is exclusively dynamical in both cases. There are no parts in substance. The material image of an alcohol becomes more than ever an illusion.

There is another class of names* whose use is rapidly extending, and whose character is especially dualistic. Such are hydric sulphate, sodic chloride, &c. They fulfil the double duty of giving a specific and generic designation, and of indicating a certain mode of chemical decomposition. Their chemical meaning, therefore, is as assuredly dynamical as that of the names whose signification has been above investigated.

[Although not writing on the subject of nomenclature, I wish to express my decided opinion in favour of the Berzelian method. In the first place, it follows the immemorial usage of the Greek, Latin, and English languages to make it a rule to qualify by means of an adjective; secondly, it proceeds upon the plan universally adopted in botany; thirdly, it is available throughout descriptive chemistry, and is sufficiently supple for any legitimate inflexion. These are great advantages. On the other hand, such names as "potassium nitrate," "disodium tartrate," "hydrogen and sodium tartrate" (a rather ambiguous expression), &c. offend against systematic grammatical usage, have no counterpart in any other science, are excessively awkward to compound, and often are very inharmonious. Chemistry might profitably lose many of her oldest acquirements, but she ought not to give up her adjectives without a struggle.]

* Berzelius, Journ. de Phys. vol. lxxiii. p. 263.
XXXI. On the Magnetism of Electrodynamic Spirals.
By George Gore, F.R.S.*

I HAVE made some experiments on the influence of high temperatures upon the magnetic condition of electrodynamic spirals formed of iron, copper, and platinum, the heat being obtained by means of a voltaic battery.

Experiment 1.—Two horizontal spirals of wire, A and B, fig. 1, were employed. A was composed of a platinum wire 34.9 centims. long and 1.42 millim. thick, and B of a copper wire 34.9 centims. long and 2.59 millims. thick—each being coiled into a cylindrical helix about 3.8 centims. long and 1.6 centim. in diameter, with exactly the same number of turns in each; they were united by means of binding-screws, C. A magnetized steel needle, D, about 8 centims. long, was suspended in the direction of north and south (by means of a fibre about 35 or 40 centims. long), with its south pole between and equidistant from the ends of the two coils, which were about 3 centims. asunder; the needle was weighted at its centre by a little piece of lead. One of the wires was coiled in the direction of a right-handed, and the other of a left-handed screw, so that on passing the current through them the magnetism excited in their ends nearest to the needle was of the same kind; and the direction of the current was such that the poles of the wires were of the same kind as that of the nearest end of the needle. With ten Grove's cells as one series, the platinum plates of which were 16.5 centims. long and 7.7 centims. wide, the platinum wire became very hot, but not red-hot, whilst the copper wire remained cold; the needle remained equidistant between the two wire poles, being equally repelled by the hot platinum and cold copper.

* Communicated by the Author.
Experiment 2.—In this experiment the spirals were 1·5 centim. in diameter and 2·9 centims. long, and were placed parallel to each other; as in fig. 2, and were formed of platinum wire 82 millim. thick, and copper wire 2·05 millims. thick; but in other respects the arrangements were similar to those of No. 1 experiment. With the ten Grove's cells as one series the platinum wire was quite red-hot throughout, and the copper wire cold, and the pole of the needle remained equally repelled by each spiral as before; and with the cells as a double series of five, the platinum wire was bright red-hot, and the needle remained in the centre the same as if no current was passing. A bright red heat, therefore, did not sensibly increase or decrease the magnetizing influence of the platinum spiral; and the magnetism of that spiral was independent of change of temperature, and of the molecular state produced by change of temperature.

Experiment 3.—The arrangements were the same as in the last experiment, except that an iron wire was substituted for the platinum one, and ten large Smee's cells, 12 inches × 8 inches, substituted for the Grove's battery. With an iron wire 1·42 millim. thick the iron wire became heated to about 200° C., and repelled the needle much more powerfully than did the cold copper spiral. With an iron wire 95 millim. thick the iron spiral, though much hotter, still repelled the needle more strongly than the copper one; and also repelled it strongly after the current was stopped and both the wires were quite cold, evidently because it retained much of its induced magnetism. With a "No. 23" iron wire, 68 millim. thick, the iron spiral became red-hot, and repelled the pole of the needle with but little greater force than that of the copper spiral; on shifting the copper spiral to a distant part of the circuit and renewing the current, the iron spiral repelled the needle strongly. This experiment shows:—first, that an electric current of a given strength passing simultaneously through two similar spirals of wire, one of iron and one of copper, produces at moderate temperatures a greater degree of magnetism in the former than in the latter; secondly, that when the iron wire attains a red heat its total magnetic power is not much greater than that of the copper; thirdly, that the iron

at moderate temperatures possessed both the magnetism due to
the current itself and that induced by the current upon the mole-
cules of the iron, whilst the copper possessed only that which
belonged to the current itself; and, fourthly, that by a rise of the
temperature of the iron to redness the induced portion of the
magnetism in it decreased, whilst the magnetism due to the cur-
rent alone remained equal to that in the copper.

Experiment 4.—I now tried flat spirals of wire facing each
other about 4 centims. apart, with their axes in one horizontal
line (see fig. 3); the spirals were about 2·5 centims. in diameter,

Fig. 3.

and each contained four turns of wire. With 20 centims.
length of iron wire 95 millim. in diameter, and 20 centims. of
copper wire 2·07 millims. in diameter, and the ten large Smee’s
elements, the iron spiral became nearly red-hot; at the first in-
stant of passage of the current the iron repelled the similar pole
of the needle somewhat more strongly than the cold copper; but
as it became hot its excess of repulsion became less. I now sub-
stituted twelve Grove’s cells (of the size already mentioned) as a
double series of five for the Smee’s battery: the iron wire be-
came red-hot, whilst the copper one remained cold, and the needle
remained about equidistant between the two helixes. With a
flat helix of thin platinum wire substituted for the one of iron
the platinum one became nearly white-hot, and appeared to repel
the needle a little more than the copper; it is difficult, however,
to make the spirals perfectly uniform, and to exactly determine
the middle point between them.

Experiment 5.—In this experiment no needle was employed.
Two flat spirals (A and B, fig. 4), about 1·8 centim. in diameter,
composed of thin iron wire 15 centims. long, and each of the same thickness, were taken, and one of them freely suspended (by means of the two fibres C, C) vertically and facing the other at a distance of about 2 or 3 millims. apart. The lower ends of the spirals dipped into two small cups of mercury to enable the connections to be made with freedom of motion to the suspended one; and the similar poles of the spirals faced each other. The current from the twelve Grove’s cells in single series was passed through them; they immediately repelled each other and then melted. The same current was now passed through two similar ones 1·6 centim. in diameter, composed of thicker iron wires, each 12 centims. long and 0·95 millim. thick; they repelled each other most distinctly and strongly whilst warm, and also whilst quite red-hot, and the moveable one instantly fell back on disconnecting the battery.

As general results of these experiments on electro-spirals, it is shown, first, that a red heat diminishes only the induced magnetism in an electro-spiral of iron, and does not increase or decrease that due to the current alone; secondly, that a red-hot electro-spiral of iron is capable of inducing magnetism, but not of having magnetism induced in it; thirdly, that the production of heat by electricity in a wire of great resistance, whether of iron, copper, or platinum, is not attended by a diminished production of the magnetism due to the current in the heated part of that wire.

These results also support the view that the magnetism of an electro-spiral of copper or other non-magnetic or slightly magnetic metal is not due to a particular position or mode of motion of the particles of the metal, but is a direct result of, and inseparable from, the electric current itself; whilst that of a similar spiral of iron is partly due to the same cause, and is partly dependent upon a molecular condition which is destroyed by a high temperature.

The induced magnetism of iron is a much more complex phenomenon than the magnetism of an electric current, because it depends both upon temperature and upon the molecular structure of the iron; whereas the magnetism of an electric current
is entirely independent of temperature, and of molecular structure, and cohesive strength, except so far as they affect the quantity of the current, as is also shown by the magnetic character of an electric current circulating through an electrolyte, and also of the electric discharge in rarefied gases.

XXXII. Memoir on Internal Work in Gases.
By M. Achille Cazin.

[Concluded from p. 210.]

Part II.—Application of the Thermodynamical Formule to the Investigation of the Internal Work in Gases.

§ I. On the internal work performed in a gaseous mass when the reservoir which contains it is connected with a second, empty reservoir.

Amongst the problems which can be treated by means of the thermodynamical formulæ, and which relate to the experiments described in the first part, the following is the most simple:—

Problem I.—In a reservoir whose sides are impermeable to heat, there is 1 kilogramme of gas of which the volume and temperature are \( v_1 \), \( t_1 \) respectively; this reservoir is connected with a second, empty reservoir, with sides impermeable to heat, of \( v_2 \) capacity: to calculate the temperature \( t' \) and the pressure \( p' \) which are established when the exchanges of heat and motion have entirely ceased, and the internal work effected.

Whilst the efflux is going on, heat disappears in the first reservoir, with production of mechanical work; in the second reservoir \( vires vivæ \) are created which are equivalent to this work, and which are finally transformed into heat. No external work is either produced or spent; no amount of heat is either taken from or given to external bodies. If there were no attraction between the molecules of the gas, no internal work would be put in operation; the heat created would be equal to the heat which had disappeared, and finally the temperature would again become \( t_1 \). But if there is molecular attraction, it has at length been overcome, and the quantity of heat which disappeared exceeds that which has been created; the difference is equivalent to the internal work produced, and the state of the gas is

\[
\begin{align*}
\frac{1}{2} &< t_1, \\
\frac{1}{2} &+ v_2, \\
p' &.
\end{align*}
\]

The thermodynamical formulæ enable us to calculate this final state.
I shall denote by
\[ dQ \] the heat supplied to the body when it undergoes an elementary modification;
\[ KdT \] the increase of its sensible heat, \( K \) being its true specific heat, and \( T=273+\theta \) its absolute temperature (\( \theta \) in Centigrade degrees);
\[ A \) the calorific equivalent of the unit of work;
\[ dI \] the elementary internal work produced;
\[ dE \) the elementary external work produced.

The general relation is
\[ dQ = KdT + AdI + AdE. \tag{1} \]

The sum \( KdT + AdI \) is the differential of a function \( U \) which has been called total internal heat by M. Zeuner, virtual energy by M. Him. It is remarkable in this one thing, that its variations only depend on the initial condition and final condition of the body, and not at all on the manner in which the change of condition is effected. Its mechanical equivalent is still called internal energy.

For each body, there exists a certain relation,
\[ \phi(p, v, T) = 0, \tag{2} \]
in which it is sufficient to have two of the three quantities \( p, v, T \) in order that the condition of the body may be determined. It is this relation which is expressed approximately, for the gases called permanent, by the laws of Mariotte and Gay-Lussac,
\[ \frac{pv}{T} = M, \tag{3} \]
\( M \) being a constant which depends on the units adopted and on the nature of the gas.

Thus the quantity \( dU \) will be a function of two of the three variables \( p, v, T \) (of \( v, T \) for example, considered as independents); and when the body undergoes any final change we shall have
\[ U - U_0 = \phi(v_1, T_1) - \phi(v_0, T_0), \]
whatever may be the law of the change between the initial condition \( v_0, T_0 \) and the final condition \( v_1, T_1 \).

It follows from this that the variation of virtual energy \( U - U_0 \) remains the same between the same limits, whether the mode of change be reversible or non-reversible.

Consequently from the two fundamental theorems of thermodynamics, and from the experimental fact that for air a condition exists in which relation (3) is verified, the following relation, ap-
Applicable to changes of every kind, can be demonstrated*,

\[ dU = KdT + A \int \left( T \frac{dp}{dT} - p \right) dv, \ldots (4) \]

where \( \frac{dp}{dT} \) is the partial derivative of \( p \) with respect to \( T \), as deduced from equation (2).

In the present problem,

\[ dQ = 0 \text{ and } dE = 0. \]

Hence

\[ dU = 0; \]

and the integration between the conditions \( v_1, T_1; v', T' \) gives

\[ K(T_1 - T') = A \int_{v_1 T_1}^{v' T'} \left( T \frac{dp}{dT} - p \right) dv. \ldots (5) \]

Such is the formula from which \( T' \) may be known when the relation (2) is known. Each member of that equation expresses in calories the internal work effected in the operation. It may be easily seen that, if formula (3) be taken for that relation, we shall find

\[ T_1 - T' = 0, \]

which is not justified by experiment. I am going to apply formula (5) to carbonic acid, for which formula (2) may be calculated empirically within certain limits by means of the experimental data of M. Regnault.

Mr. Rankine has calculated, following M. Regnault's experiments on the heating of carbonic acid under a constant volume, an empirical formula which is very convenient for the calculation, and of which Messrs. Joule and Thomson have made use in their researches on the efflux of gases through narrow orifices†.

This is the formula:

\[ p = \frac{p_0 v_0 T}{T_0} - \frac{ap_0 v_0^2}{T_0 v^2}, \ldots (6) \]

\( p_0, v_0, T_0 \) are relative to one kilogramme of gas at the temperature of melting ice and under the pressure of one atmosphere. From this formula may be derived

\[ T \frac{dp}{dT} - p = - \frac{2ap_0 v_0^2}{T v^2}, \]

* P. de Saint-Robert, Principes de Thermodynamique, p. 82 (1865).
and by introducing this value in equation (5), we obtain

\[ T_1 - T' = \frac{2Aq_0v_0^2}{K} \left( \frac{1}{T_1v_1} - \frac{1}{T'v'} \right), \]

whence

\[ T' = \frac{T_1}{2} - N + \sqrt{\left( \frac{T_1}{2} - N \right)^2 + N'}, \]

\[ N = \frac{Aq_0v_0^2}{KT_1v_1}, \]

\[ N' = \frac{2Aq_0v_0^2}{Kv'}. \]

(7)

The calculation is, as may be seen, very easy.

Before applying formula (7) numerically, it is important to see how formula (6) satisfies the various experiments of M. Regnault on the compressibility and dilatation of carbonic acid.

Mr. Rankine took \( a = 1.9 \); but I found, when all the experimental data of M. Regnault were taken into consideration, that the value \( a = 1.6 \) was more suitable. The reader will be able to decide the question from the following reasoning.

In his memoir on the density of gases*, M. Regnault says that a glass globe of 9881086 litres at zero contained 19539 grms. (mean of five experiments) of carbonic acid gas at zero and under the pressure of one atmosphere; from this may be deduced the volume of one kilogramme of gas under normal circumstances,

\[ v_0 = 0.50571 \text{ cub. m.} \]

By means of the Table on page 236 of vol. xxvi. of the Mémoires de l'Académie des Sciences de Paris, we obtain

\[ v = 0.38307 \text{ cub. m.} \]

at zero, under the pressure of 1000 millims. of mercury. The specific volume at zero and under various pressures may afterwards be calculated by means of the formula on page 426, vol. xxi. of the same Mémoires, which gives the results of experiments made on carbonic acid at about 3°; at zero the same formula is admitted. Finally, in the memoir on the dilatation of gas†, we find (p. 112) that, under the pressure of 3589 millims. of mercury, the binomial of dilatation at constant volume from 0° to 100° is

\[ 1 + 100a = 1.38598, \]

† Mémo. de l'Acad. des Sciences de Paris, vol. xxi.
\[
1 + 100 \alpha = \begin{cases} 
1.37099 \text{ under the pressure of 760 millims.} \\
1.38455 \text{ \ldots 2520 \ldots} 
\end{cases}
\]

By means of these data I calculated the following Table, in which column \( p (1) \) gives the observed pressures, column \( p (2) \) gives the pressures calculated from formula (6), taking \( \alpha = 1.6 \); column \( p (3) \) gives the pressures calculated according to the same formula, taking \( \alpha = 1.9 \). It may be seen that the numbers of column \( p (3) \) all diverge in the same direction from the numbers of column \( p (1) \), whilst those of column \( p (2) \) diverge at first in one direction, then in the opposite; which justifies the use of \( \alpha = 1.6 \):

<table>
<thead>
<tr>
<th>v.</th>
<th>T.</th>
<th>( p (1) )</th>
<th>( p (2) )</th>
<th>( p(1) - p(2) )</th>
<th>( p (3) )</th>
<th>( p(1) - p(3) )</th>
</tr>
</thead>
<tbody>
<tr>
<td>cub. met.</td>
<td></td>
<td>millims.</td>
<td>millims.</td>
<td>millims.</td>
<td>millims.</td>
<td>millims.</td>
</tr>
<tr>
<td>0.6933</td>
<td>373</td>
<td>760</td>
<td>756</td>
<td>+ 4</td>
<td>755</td>
<td>+ 5</td>
</tr>
<tr>
<td>0.38307</td>
<td>273</td>
<td>1000</td>
<td>996</td>
<td>+ 4</td>
<td>996</td>
<td>+ 4</td>
</tr>
<tr>
<td>0.20779</td>
<td>373</td>
<td>2520</td>
<td>2508</td>
<td>+ 12</td>
<td>2508</td>
<td>+ 12</td>
</tr>
<tr>
<td>0.15008</td>
<td>273</td>
<td>2520</td>
<td>2510</td>
<td>+ 10</td>
<td>2510</td>
<td>+ 10</td>
</tr>
<tr>
<td>0.10437</td>
<td>273</td>
<td>3589</td>
<td>3578</td>
<td>+ 11</td>
<td>3578</td>
<td>+ 11</td>
</tr>
<tr>
<td>0.10437</td>
<td>373</td>
<td>4974</td>
<td>4955</td>
<td>+ 19</td>
<td>4955</td>
<td>+ 19</td>
</tr>
<tr>
<td>0.03831</td>
<td>273</td>
<td>9226</td>
<td>9257</td>
<td>- 31</td>
<td>9257</td>
<td>- 31</td>
</tr>
<tr>
<td>0.01915</td>
<td>273</td>
<td>16705</td>
<td>16961</td>
<td>- 256</td>
<td>16961</td>
<td>- 256</td>
</tr>
</tbody>
</table>

Between zero and 100°, one atmosphere and twelve atmospheres, the values of \( p (2) \) are sufficiently near; but at twenty-two atmospheres the difference \( p (1) - p (2) \) is large enough to make the formula of Mr. Rankine not very exact; but I thought it necessary to use it provisionally, with the modification relative to \( \alpha \) which I have indicated. In order to establish a more satisfactory empirical formula, some data relative to high temperatures would be necessary, which are totally wanting.

\textit{Numerical application of formula (7):—}

\[
v = 0.10871 \text{ cub. m.,} \\
v' = 0.52056 \text{ \ldots,}
\]

The ratio of these two numbers is that of the reservoirs of my ultimate apparatus.

I consider one kilogramme of carbonic acid at 10°, passing from volume \( v_1 \) to volume \( v' \), in the conditions of the problem already enunciated:

\[
A = \frac{1}{425},
\]

\[
\alpha = 1.6,
\]

\[
\rho_0 = 10334,
\]
This value of K is the specific heat at a constant volume and a high temperature. According to M. Regnault, the specific heat at constant pressure is 0.2396 towards 200°. If the ratio of the two specific heats were 1.29, as it seems to be at the ordinary temperature*, K would be equal to 0.18. But it is not proved that this ratio does not increase with the temperature. M. Hirn took 0.164 for the value of K. By taking 0.17 I do not think a very large error is made.

With these various data the formula (7) leads to

\[ T' = 280°, \]

whence

\[ T_1 - T' = 3°. \]

Having \( T' \) and \( v' \), the pressure \( p' \) in millimetres of mercury can be calculated by means of the formula (6), in which \( p_0 = 760 \) millims. It is found that

\[ p' = 753.15 \text{ millims.} \]

In the same manner, by introducing into this formula the values of \( T_1, v_1 \) we shall have, for the initial pressure,

\[ p_1 = 3572 \text{ millims. (4.7 atmospheres).} \]

Suppose that after the expansion the heat of the sides reestablishes the temperature \( T_1 \) in the gaseous mass. Then the pressure, which was \( p' \), will become \( p \); and this value may be calculated by introducing \( T_1, v' \) into formula (6); we obtain

\[ p = 761.30 \text{ millims.} \]

Thus one kilogramme of carbonic acid at 10° and under a pressure of 4.7 atmospheres expanding, without external work and without transmission of heat, to the pressure of about one atmosphere (753.15 millims.), undergoes a spontaneous fall of temperature of 3°. When it afterwards resumes its primitive temperature by the action of the sides, its pressure rises

\[ p - p' = 8.15 \text{ millims.} \]

The example just given furnishes some numbers quite comparable with those given by my experiments.

It may be observed that
\[ p_1v_1 = 388.31, \]
\[ p'v' = 392.06, \]
\[ p'v'' = 396.30. \]

Now the first and last of these products refer to the same temperature \( T_1 \); and we have
\[ \frac{p'v'}{p_1v_1} = 1.0206, \]
a number which accords very well with the experiments of M. Regnault on the compressibility of carbonic acid.

By comparing the initial condition and the final condition before the calorific action of the sides, we get
\[ p'v' > p_1v_1. \]

§ II. On the internal work which is accomplished in a gaseous mass when it flows into the atmosphere under a constant pressure.

Problem II.—A gas maintained under the constant pressure \( p_1 \), and at the constant temperature \( T_1 \), flows into a space the sides of which are impermeable to heat, where it is maintained at the constant pressure \( p' \): to calculate the final temperature \( T' \) which establishes itself beyond the orifice when the gaseous molecules have lost their velocities in producing heat.

This simple case refers to the experiments of Messrs. Joule and Thomson and to those of M. Hiran.

Conceive the gas to be contained in an indefinite cylinder (fig. 9) divided into two parts by the partition \( E \) in which is the orifice. In the permanent efflux there is on each side of the partition a certain space in which the molecules of the gas acquire velocity, and afterwards lose it in again forming heat. Let \( C \) and \( D \) be the two planes which bound this space, and \( \mu \) the gaseous mass contained in it. Suppose the cylinders to have a section equal to the unit of surface, and consider one kilogramme of gas under the pressure \( p_1 \) and at the temperature \( T_1 \); it will occupy a certain volume \( v_1 = AC \). During the efflux the mass of gas \( 1 + \mu \) passes from the condition \( AD \) to the condition \( CB \), and the part \( BD = v' \) contains one kilogramme of gas at the temperature \( T' \) and under the pressure \( p' \). As in the two positions of the mass \( 1 + \mu \) the part contained in the space \( CD \) remains in the same condition, we may say that one kilogramme of gas has passed from the volume \( AC \) to the volume \( CB \), and apply to this change the fundamental formula of thermodynamics (1).

We suppose that the sides of the cylinder are impermeable to heat; then \( dQ = 0 \); consequently if external work has been
effected, there must have been a variation of the virtual energy equivalent to this work and of contrary sign. Let us examine whether there is external work.

The motion of the plane A to the position C represents a work \( p_1v_1 \); that of the plane D passing in the same time to B represents a work \( p'v' \), taken with the opposite sign to that of the preceding. The work \( p_1v_1 \) is expended; the work \( p'v' \) is, on the contrary, produced; finally our gas has flowed out, producing the work \( p'v' - p_1v_1 \), if the value of this expression is positive; if it is negative, the gas has, on the contrary, expended this quantity of work. Without foreseeing the sign of this quantity, we shall only say that

\[
\int_{v_1}^{v_1'} dE = p'v' - p_1v_1.
\]

As to the variation of the virtual energy, we shall have the same expression as in the preceding section, so that the equation of the problem is

\[
K(T' - T_1) + A \int_{v_1T_1}^{v_1'T'} (T \frac{dp}{dT} - p) dv + A (p'v' - p_1v_1) = 0,
\]

from which we can deduce \( T' \) when we have the function (2). The second term of equation (8) represents internal work estimated in calories. I am now going to apply this equation to carbonic acid.

By combining equations (8) and (6) we obtain

\[
K(T' - T_1) + A p_v v_0 \left[ \frac{T' - T_1}{T_0} + 3av_0 \left( \frac{1}{T_1v_1} - \frac{1}{T'v'} \right) \right] = 0,
\]

whence may be deduced

\[
T' = \frac{T_1}{2} - N + \sqrt{\left(\frac{T_1}{2} - N\right)^2 + N'},
\]

\[
N = \frac{3\Lambda p_v v_0^2 T_0}{2T_1v_1(KT_0 + A p_v v_0)},
\]

\[
N' = \frac{3\Lambda p_v v_0^2 T_0}{v'(KT_0 + A p_v v_0)},
\]

formulas analogous to formulae (7).

When \( T' \) is calculated, its value and that of \( v' \) may be introduced into formula (6) and thus \( p' \) may be deduced; we shall then obtain the quantity \( p'v' - p_1v_1 \).

**Numerical application.**—By employing the same data as in
the preceding section, it is found that

\[ T' = 279^\circ 44, \]
\[ T_1 - T' = 3^\circ 56, \]
\[ p' = 751.63 \text{ millims.}, \]
\[ p'v' = 391.27. \]

We have already had
\[ p_1v_1 = 388.31. \]

Hence \( p'v' = p_1v_1 \) is positive, and there is a production of external work during the efflux. This work has been produced at the expense of the virtual energy of the gas; and by that may be explained why the fall of temperature \( T_1 - T' \) is greater than in the first problem.

**Discussion of the experiments of Messrs. Joule and Thomson and of M. Hirn.**

Messrs. Joule and Thomson have made their experiments under conditions in which the gas on leaving the orifice resumed the initial temperature \( T_1 \). From that moment the sides furnished to each unit of weight of the gas the heat \( C_p(T_1 - T') \), \( C_p \) being the specific heat at constant pressure. We shall easily pass from the preceding case to this latter.

The gas was in the condition \( p', v', T' \); it passes to the temperature \( T_1 \) and to the volume \( v' \) under the constant pressure \( p' \); hence it expands in producing external work \( p'(v'' - v') \); its virtual energy experiences an increase

\[ K(T_1 - T') + A \int_{v''}^{v'} \left( T \frac{dp}{dT} - p \right) dv. \]

The fundamental equation (1) gives therefore

\[ C_p(T_1 - T') = K(T_1 - T') + A \int_{v''}^{v'} \left( T \frac{dp}{dT} - p \right) dv + A p'(v'' - v'). \quad (1) \]

This equation may give \( v'' \). But by adding equations (10) and (8) together we get

\[ C_p(T_1 - T') = A \int_{v''}^{v'} \left( T \frac{dp}{dT} - p \right) dv + A (p''v'' - p_1v_1). \quad (11) \]

This formula reveals the final condition of the gas passing from the condition \( p_1 v_1 T_1 \) to the condition \( p'' v'' T'' \), with transmission of the external heat \( C_p(T_1 - T') \). Moreover it is immediately obtained by means of the fundamental equation (1).

In calculating it as I have done, there is the advantage of following gradually the operation under its two phases.

In order to apply these formulae to carbonic acid, it is only
necessary to combine equation (11) with formula (6), and the following simple formula is obtained:

\[ C_p(T_1 - T') = \frac{3\Lambda \sigma_0 \nu_0^2}{T_1} \left( \frac{1}{\nu_1} - \frac{1}{\nu''} \right), \quad \ldots \quad (12) \]

from which we can deduce \( \nu'' \).

I shall use equation (12) in order to verify the calculation made of \( T' \) by means of equations (9).

To this end I introduce in formula (6) the values \( T_1 = 283^\circ \) and \( p' = 751.63 \) millims., and from it deduce

\[ \nu'' = 0.527296 \text{ cub. m.}, \quad p'' = 396.33. \]

Having \( \nu'' \), we introduce its value into equation (12); and putting

\[ C_p = 0.216, \]

I find

\[ T_1 - T' = 3.56, \]

which is just the value deduced from formulæ (9).

The identity of the numbers evidently depends on the value of \( C_p \); but the value which I have chosen is rather too great, so that it leads us to take 3.56 as a minimum. The smallest value of \( C_p \) observed by M. Regnault is 0.187; this number leads to

\[ T_1 - T' = 4.11, \]

and we must regard this value as a maximum. Messrs. Joule and Thomson have made use of the formula (12) by expressing \( v_1 \) and \( v'' \) as functions of \( p_1 \) and \( p' \) by means of the law of Mariotte. In fact putting

\[ p_1 v_1 = T_1 \frac{p_0 v_0}{T_0}, \]
\[ p' v'' = T_1 \frac{p_0 v_0}{T_0}, \]

we have

\[ T_1 - T' = \frac{3\Lambda \sigma_0 T_0}{C_p T_1^2} (p_1 - p'). \quad \ldots \quad (13) \]

In this formula the pressures are expressed in units of weight on the unit of surface; if we wish to express them in atmospheres, we shall take

\[ T_1 - T' = \frac{3\Lambda \sigma_0 T_0 \times 10334}{C_p T_1^2} (n_1 - n') \text{ atmosphere}. \quad (14) \]

Such is the formula employed by the English physicists, p. 337 of their memoir*. Let us examine if it agrees with


There is a mistake in the printing of the last formula of page 337; the factor \( \frac{p_1 - p_2}{p_0} \) is omitted.
formula (12). Making, in this latter as in the preceding calculation, \( n_1 = 4.7 \) atmospheres and \( n' = 0.989 \), we find
\[
T_1 - T' = 3^\circ.46,
\]
which number is a little less than that deduced from formula (12); but the difference is so trifling that the approximation of formula (14) is justified.

All the preceding formulae follow the course of the phenomena; but they can never give very exact numbers, on account of the uncertainty which prevails as to the value of some of the constants, such as \( \alpha \), \( \Lambda \), \( K \), and \( C_p \). Thus Messrs. Joule and Thomson have taken, from Mr. Rankine,
\[
\log \frac{3a v_0}{C_p} = 2.5111438 \text{ and } T_0 = 274,
\]
instead of 2.4365685 and 273, which I have adopted.

Consequently formula (14) gives with these numbers
\[
T_1 - T' = 4^\circ.09.
\]

On page 336 of the memoir just quoted may be read that the cooling observed with a difference of pressure of 60.601 pounds on a square inch was 5°.049 at the temperature of 12°.844.

This excess of pressure is equivalent to \( n_1 - n' = 4.216 \) atmospheres, if we assume for one atmosphere a pressure of 14.373 pounds per square inch.

Formula (14) gives the preceding number with the constants of Mr. Rankine very well; but with those which have been used in the preceding calculations we obtain under the same circumstances 4³.65, a smaller number.

It is possible that in the experiments of Messrs. Joule and Thomson the thermometer may have been placed at too small a distance from the orifice for the \textit{vis viva} of the jet to be completely converted into heat. Let us see what in fact was the mode of operating.

In the large apparatus described in volumes cxliii. and cxliv. of the Transactions of the Royal Society, the gas contained in a gasometer is withdrawn by a pump and forced into a long serpentine tube surrounded by water. At the end of this serpentine tube is a wooden cylinder into which cotton is pressed, 2.72 inches in length and 1.5 inch in diameter; and beyond, a tube is adjusted which reducts the gas to the gasometer. The pump maintains the regular circulation, and keeps the pressures \( p_1 \) and \( p' \) constant on each side of the porous partition. The able physicists took the most minute precautions in order to obtain regular effects. A thermometer placed very near the partition received the gas after its expansion in the form of a multitude
of small jets, having certainly very little *vis viva*; and when the pressures were perfectly constant, this thermometer indicated a constant temperature considerably less than the external temperature.

The general effects correspond to the theory; but I do not think that the method is susceptible of very great accuracy. The influence of the sides and the porous partition on the thermometer was very great (as the experimenters observed) when there were variations of the pressure \( p_1 \). Besides, the gas was not completely dry, and the correction of the effect due to moisture not very accurate. Finally the position of the thermometer must have had an enormous influence. Very close to the partition the jets were still animated by a certain velocity, and the temperature must have increased very rapidly from the partition to a certain distance, from which it again became equal to the external temperature. It appears evident to me that the effect observed on the thermometer does not indicate the temperature which the gas would have if it returned to rest after the expansion without external calorific action. I think that the thermometer indicated too low a temperature, and that hydrogen would have been able to produce analogous effects to those of air under the same circumstances, as occurred in my own experiments. Unfortunately, particulars of the observations made on this gas are not given.

Neither is the method employed by M. Hirn* free from all objection. Aqueous vapour on leaving the boiler passes through a tube of 5 centims. diameter, where it is superheated; it passes through an orifice of 4 millims. into a cubical wooden box, which is enveloped by two other boxes. Thus the expanded vapour circulates in the spaces between the boxes before issuing into the atmosphere; and consequently the central cavity is conveniently sheltered from the cooling action of external bodies. In this cavity the thermometer is placed; it is perfectly sheltered by a partition against the direct shock of the jet of vapour, so that the molecules of vapour only impinge upon it after they have lost their velocities. But, on account of the magnitude of the orifice, the molecules of vapour have a great velocity on each side of the orifice, and there is a great fall in the temperature, due to the velocity, between the orifice and the partition. Hence the radiation of the sides is considerable, and the jet only returns to rest after having received heat from without; and therefore the final temperature observed on the thermometer is too high. This objection was made by M. Combes†.

* Exposition analytique et expérimentale de la Théorie mécanique de la chaleur, second edition, p. 177 (1865).
† Exposé des principes de la Théorie mécanique de la chaleur, par M. Combs, p. 238 (1867).
I do not think that the thermal effects at present in question can be studied very exactly by means of the thermometer. The inevitable action of the sides, alone, prevents entire exactness.

M. Hirn has given in one of his last memoirs some very simple formulæ, by means of which we can solve the problems before us*. I tried to apply them; but the results to which they lead differ from those which I have given.

Following the method invented by M. Hirn, and which is applied in the memoir quoted to the vapours of water, of bichloride of carbon, of sulphide of carbon, of alcohol, and of ether, I calculated a formula for carbonic acid,

\[ p = a \frac{T}{v} - b \left(\frac{v_0}{v}\right)^\beta \]  \hspace{1cm} (15)

But I have not found for the constants \( a, b, \beta \) values which made the formula agree sufficiently with the Table on page 272. If an agreement between them were established, that formula would take the place of the formula of Mr. Rankine, and, introduced into the preceding equations, would solve our problems.

For example, in the problem of § I., formula (5) would give

\[ T_1 - T' = \frac{A b v_0^\beta}{K (\beta - 1)} \left(\frac{1}{v_1^{\beta-1}} - \frac{1}{v^{\beta-1}}\right). \]

The calculations which I have made in the second problem indicate that it is very distinct from the first. In order that the mechanical and thermal effects might be the same in these two problems, it would be necessary that

\[ p' v' = p v_1; \]

and this equality seems to me impossible. The contradiction may very likely be only apparent between the results at which I have arrived and those of M. Hirn. It is not in the spirit of criticism that I refer to it here. Seeking to resolve those questions which have been already treated by my excellent friend, by means of a method differing from his, I meet with differences which may be explained by the inexactness of the numerical data. The importance of formula (15), which M. Hirn has used with advantage in the study of vapours, imposed on me the comparison of the two methods; and I have made it in the hope that it may contribute to the elucidation of a delicate question of thermodynamics.

§ III. On the internal work in a gas which undergoes expansion or compression without external calorific action, and of which the elastic force at each moment balances the pressure exerted on its surface.

Problem III.—A kilogramme of gas at temperature $T_1$ passes from volume $v_1$ to volume $v_2$ while overcoming external pressure equal at each moment to its elastic force, without there being either addition or loss of external heat: knowing $v_1$, $T_1$, and $v_2$, to calculate the final temperature $T_2$ and the internal work effected.

This operation belongs to the kind which are termed reversibles. It cannot be realized in practice; but a gaseous mass which pushes a piston in a cylinder, or which is compressed by the piston so quickly that the external heat may be neglected, comports itself very nearly as in the problem enunciated.

The quantity of external heat which the gas takes in changing its condition may be represented by

$$Q = \int T d\phi,$$

$\phi$ being a function (of two of the variables $p, v, T$) which Mr. Rankine has called the thermodynamic function.

If $v$ and $T$ be taken as the variables, we have*

$$\phi = K \ell T + A \int \frac{dp}{dT} dv,$$

$\frac{dp}{dT}$ being the partial derivative of $p$ deduced from relation (2).

In the present problem $Q = 0$; hence $\phi$ = constant, and consequently

$$K \ell \frac{T_2}{T_1} + A \int_{v_1 T_1}^{v_2 T_2} \frac{dp}{dT} dv = 0. \quad \quad \quad \quad (16)$$

This equation being combined with relation (2), $T_2$ may be calculated when $T_1$, $v_1$, $v_2$ are known. The initial and final pressures $p_1$, $p_2$ may be calculated by means of relation (2).

A relation between $p$ and $v$ may afterwards be established, which will serve to calculate the external work

$$\int_{v_1}^{v_2} p dv.$$

Finally, the internal will be

$$\int_{v_1 T_1}^{v_2 T_2} \left( T \frac{dp}{dT} - p \right) dv.$$

Hence the problem is solved.

* P. de Saint-Robert, Principes de Thermodynamique, p. 69 (1865).

For carbonic acid in particular, formula (6) must be used, and equation (16) will become
\[
\frac{\theta}{T_1} + \frac{A p_0 v_0^2}{K v_2 T_2^3} = \frac{A p_0 v_0^2}{K T_0} \frac{v_2}{v_1} + \frac{A p_0 v_0^2}{K v_1 T_1^3}
\]  \( (17) \)

Whence \( T_2 \) may be deduced by tentative methods.

The internal work effected will be
\[
2A p_0 v_0^2 \left( \frac{1}{T_1 v_1} - \frac{1}{T_1 v_2} \right).
\]

If there were no external work (as in § 1.) between the same volumes, initial and final, we should evidently have a final temperature \( T' > T_2 \); and we have seen in this section that the internal work was
\[
2A p_0 v_0^2 \left( \frac{1}{T_1 v_1} - \frac{1}{T_1 v_2} \right).
\]

Hence it would be greater than the preceding.

According to this, the internal work would not be a function of the volume alone, and there would be internal work expended during the cooling at constant volume, which ought to reduce the gas from the condition \( v_2 T' \) to \( v_2 T_2 \). In other terms, the molecular forces ought to create heat during the cooling.

Conversely, during the heating at constant volume there would be internal work produced.

It is probable that the apparent specific heat at constant volume decreases when the temperature rises. Let \( C_v \) be that specific heat; \( C_p \), the specific heat at constant pressure (determined by M. Regnault), then \( \frac{C_p}{C_v} = \gamma \).

The quantity \( C_p \) increases considerably with the temperature; hence it seems probable that \( \gamma \) increases rather rapidly with the temperature.

It is for experiment to confirm these previsions. By the method described by me in these Annales in 1862, \( \gamma \) might be investigated up to 100°. The apparatus described in the first part of this memoir would be very suitable. I hope to be able to make some trials in this direction.

The calculations indicated in this section would be of no utility at the present time, because I know of no experiments on the expansion and compression of carbonic acid. I content myself at the present moment with indicating them, reserving the application of them for a special study, and remark that my method permits us to find the relation which exists between the pressure and the volume of a gas which is compressed or expanded under the conditions stated in the enunciation of the problem.
Up to the present time a relation

$$pv^\beta = \text{const.} \quad \ldots \ldots \ldots \ldots$$ (18)

appears to be admitted, such as was established by Laplace and Poisson before the appearance of the thermodynamic theory. In the recent experiments which M. Hirn and myself have made on the expansion of aqueous vapour, the value of $\beta$ varied very little, so that we could accept it as constant by attributing the variations to experimental errors. Such is, I believe, the opinion of M. Hirn and M. Zeuner*. I have undertaken some new researches on carbonic acid which may throw some light on this matter.

§ IV. On the passage of a gas from a reservoir where it is compressed, into a reservoir which contains a certain quantity of the same gas rarefied.

The following problem, which relates to the experiments described in the first part of this memoir, can be solved by means of the relations established in the preceding sections.

Problem IV.—In a reservoir $A$, of volume $V_1$, is a certain weight of gas under the pressure $p_1$ and at the temperature $T_1$. In a second reservoir $B$, of volume $V_2$, is another weight of the same gas, under the pressure $p_2 < p_1$ and at the same temperature $T_1$. These two reservoirs put into communication with one another: to determine the condition of the gas when the disturbances have ceased, supposing the sides to be impermeable to heat.

Denote the weight of gas contained in reservoirs $A$ and $B$ by $m_1$, $m_2$ respectively. It is easy to deduce these values from relation (2) when $p_1$, $p_2$, and $T_1$ are known. In fact we may deduce from this relation the specific volumes $v_1$ and $v_2$, and we shall have

$$m_1 = \frac{V_1}{v_1}, \quad m_2 = \frac{V_2}{v_2}.$$

Conceive the reservoir $B$ to be of the form of a cylinder, and a piston without mass to be applied against the orifice of reservoir $A$, supposed to be at first closed. When this orifice is opened, the gas $m_1$ pushes the piston; some whirlings take place on each side of the orifice, whilst the gas $m_2$ receives a pressure which its elastic force balances at each moment. There is one moment when, these whirlings having ceased, the condition of the gas $m_1$ is $V_1 + x, p', T'$, and that of the gas $m_2$ is $V_2 - x, p', T''$. The problem thus stated does not differ, as to the final effect, from that with which we are occupied, although

* G. Zeuner, Ueber das Verhalten der überhitzten und der gemischten Wasserläufe (Civilingenieur, 13th année, 1867).
in the latter the mixture of the masses $m_1$, $m_2$ commences at the same time as the efflux.

The gas $m_1$ produces external work which is entirely consumed by the gas $m_2$; so that, if the virtual energy of $m_1$ has diminished by a quantity equivalent to this work, the virtual energy of $m_2$ has increased by the same quantity. The sum of these two energies has remained constant.

Denote the specific volume of the gas by $v$, the increase of virtual energy in the unit of weight by $\Delta U$; we shall have, for the algebraical increase of the energy of the gas $m_1$,

$$m_1 \Delta U_a = m_1 K (T' - T_1) + m_1 A \int_{v_1T_1}^{v'T_1} \left( \frac{dp}{dT} - p \right) dv,$$

then, for the gas $m_2$,

$$m_2 \Delta U_b = m_2 K (T'' - T_1) + m_2 A \int_{v_2T_1}^{v''T_1} \left( \frac{dp}{dT} - p \right) dv,$$

and finally

$$m_1 \Delta U_a + m_2 \Delta U_b = 0. \quad \ldots \ldots \quad (19)$$

After having calculated the integrals by means of the relation (2),

$$p = \phi(v, T),$$

and put

$$v_1 = \frac{V_1}{m_1}, \quad v' = \frac{V_1 + x}{m_1}, \quad v_2 = \frac{V_2}{m_2}, \quad v'' = \frac{V_2 - x}{m_2},$$

we shall have the first equation, between $x$, $T'$, $T''$.

If the relation between $p$ and $v$, which expresses the law of the compression of the gas $m_2$, be known (§ III.), we shall have

$$p' = \psi(p_2, v_2, v'') = \phi(T', v'), \quad \ldots \ldots \quad (20)$$

which is the second equation, between $x$ and $T''$.

Finally, we have similarly

$$\psi(p_2, v_2, v'') = \phi(T'', v'), \quad \ldots \ldots \quad (21)$$

the third equation, between $x$ and $T'$, which, united to the preceding ones, will enable us to determine the unknowns of the problem.

For carbonic acid, if for (2) formula (6) be taken, and for (20) the approximative formula

$$p' = p_2 \left( \frac{V_2}{V_2 - x} \right)^g,$$

equations (19), (20), and (21) will become

$$m_1 K (T' - T_1) + m_2 K (T'' - T_1)$$

$$+ 2 \Delta a p_0 v_0^2 \left[ \frac{m_1^2}{V_1 T_1} - \frac{m_1^2}{(V_1 + x) T'} + \frac{m_2^2}{V_2 T_1} - \frac{m_2^2}{(V_2 - x) T''} \right] = 0, \quad (19 \text{bis})$$
After the moment we are now considering, there is exchange of heat between the gases \( m_1 \) and \( m_2 \); the first receives heat, the second loses it; and the piston falls back a little. When the exchange is accomplished, there is a definitive state of equilibrium; the two masses \( m_1 \) and \( m_2 \) are at the same temperature \( \theta \); the specific volumes are equal,

\[
\frac{V_1 + y}{m_1} = \frac{V_2 - y}{m_2};
\]

and, lastly, the common pressure is \( p \).

From the last equality is deduced

\[
y = \frac{m_1 V_2 - m_2 V_1}{m_1 + m_2}.
\]  

Equation (19) alone is sufficient to calculate \( \theta \). As to \( p \), its value is deduced from relation (2).

For carbonic acid we shall have

\[
(m_1 + m_2)K(\theta - T_1) + 2\Delta p_r v^2 \left[ \frac{m_1^2}{T_1 V_1} - \frac{m_1^2}{\theta (V_1 + y)} + \frac{m_2^2}{T_1 V_2} - \frac{m_2^2}{\theta (V_2 - y)} \right] = 0,
\]

an equation which, if combined with (22), gives

\[
\theta = \frac{T_1}{2} - N + \sqrt{\left( \frac{T_1}{2} - N \right)^2 + N'},
\]

\[
N = \frac{\Delta p_r v_0^2 (m_1^2 V_2 + m_2^2 V_1)}{K T_1 V_1 V_2 (m_1 + m_2)},
\]

\[
N' = \frac{2\Delta p_r v_0^2}{K (V_1 + V_2)};
\]

and, from (6), the final pressure will be

\[
p = \frac{p_0 v_0}{T_0} \cdot \frac{\theta (m_1 + m_2)}{V_1 + V_2} - \frac{\Delta p_r v_0^2 (m_1 + m_2)^2}{\theta (V_1 + V_2)^2}.
\]  

It may be remarked that by making \( m_2 = 0 \), \( m_1 = 1 \) in these two formula, we recur to the equations (6) and (7) of § I. For \( V_1 \) becomes the specific volume \( v_1 \), and \( V_1 + V_2 \) is the final specific volume \( v' \).

I shall give some numerical examples of equations (23) and (24) by taking, as in the preceding examples, the data which relate to my experiments.
Example I.—I consider, as in § I., a kilogramme of carbonic acid gas contained in the apparatus at ordinary temperature; a pump causes the gas to pass from reservoir B into reservoir A up to the pressure of 3.8 atmospheres.

In order to know the weight \( m_1 \) of gas which is in reservoir A, it will suffice to calculate by formula (6) the specific volume \( v_1 \) under a pressure of 3.8 atmospheres and at \( T_1 = 283^\circ \); we find

\[
v_1 = 0.13514 \text{ cub. m.}
\]

I have previously supposed

\[
V_1 = 0.10871 \text{ cub. m.,}
\]
\[
V_2 = 0.41185 \text{ ,}
\]

numbers which are in the ratio of the capacities of my apparatus; then

\[
m_1 = \frac{V_1}{v_1} = 0.80442 \text{ kilogramme.}
\]

The weight \( m_2 \) of gas left in B is simply

\[
m_2 = 1 - m_1 = 0.19558 \text{ kilogramme.}
\]

It is also easy to calculate the pressure \( p_2 \) of the mass \( m_2 \).

In fact its specific volume

\[
v_2 = \frac{V_2}{m_2} = 2.10582 \text{ cub. m.}
\]

Consequently the pressure is calculated by formula (6); and

\[
p_2 = 0.24 \text{ atmosphere,}
\]

a number which differs very little from that of Series IX.

Introducing the values of \( V_1, V_2, m_1, m_2 \) into formula (23), we find

\[
\theta = 281^\circ.25,
\]
\[
T_1 - \theta = 1^\circ.75;
\]

then, with formula (24),

\[
p' = 756.68 \text{ millims.,}
\]
\[
p - p' = 4.62 \text{ ,}
\]

because

\[
p = 761.30 \text{ (p. 273, § I.).}
\]

Example II.—Let us put \( p_1 = 2.425 \) atmospheres as in Series X. Following the same process, it is found that

\[
v_1 = 0.21338 \text{ cub. m.,}
\]
\[
m_1 = 0.50947,
\]
\[
m_2 = 0.49053,
\]
\[
v_2 = 0.83960,
\]
Example III.—Replace the reservoir B of 0.41185 cub. m. by another of 0.7385 cub. m., as in the experiments. Then the mass \( m_1 + m_2 \) of the gas which fills all the apparatus under the ordinary pressure will be

\[
m_1 + m_2 = \frac{0.73850 + 0.10871}{0.41185 + 0.10871} = 1.62787 \text{ kilogramme.}
\]

Take, as in the first example,

\[
m_1 = 0.80442 \text{ kilogramme;}
\]

then the reservoir A contains the gas under the pressure of 3.8 atmospheres.

Then

\[
m_2 = 1.62787 - 0.80442 = 0.82345,
\]

\[
v_2 = 0.89684 \text{ cub. m.,}
\]

\[
p_2 = 0.58 \text{ atmosphere},
\]

\[
\theta = 281^\circ.74,
\]

\[
T_1 - \theta = 1^\circ.26,
\]

\[
p' = 757.88 \text{ millims.,}
\]

\[
p - p' = 3.42 \text{ millims.}
\]

These values agree with the observations made in § X. of the First Part.

In order to facilitate the comparison, I have collected all the numbers calculated into one Table:

<table>
<thead>
<tr>
<th>( V_1 )</th>
<th>( V_2 )</th>
<th>( p_1 )</th>
<th>( p_2 )</th>
<th>( T_1 - T' )</th>
<th>( p - p' ), in sulphuric acid.</th>
</tr>
</thead>
<tbody>
<tr>
<td>cub. m.</td>
<td>cub. m.</td>
<td>atmos.</td>
<td>atmos.</td>
<td>millims.</td>
<td></td>
</tr>
<tr>
<td>0.10871</td>
<td>0.41185</td>
<td>4.7</td>
<td>0.00</td>
<td>3.00</td>
<td>65</td>
</tr>
<tr>
<td>0.10871</td>
<td>0.41185</td>
<td>3.8</td>
<td>0.24</td>
<td>1.75</td>
<td>37</td>
</tr>
<tr>
<td>0.10871</td>
<td>0.41185</td>
<td>2.425</td>
<td>0.62</td>
<td>0.44</td>
<td>9</td>
</tr>
<tr>
<td>0.10871</td>
<td>0.73850</td>
<td>3.8</td>
<td>0.58</td>
<td>1.26</td>
<td>27</td>
</tr>
</tbody>
</table>

The calculations show what is the share of internal work in the changes which the curve XX undergoes when the pressure \( p_1 \) and the dimensions of reservoir B are changed. Since they indicate exactly the direction and, up to a certain point, the magnitude of these changes, just as they have been observed, they furnish a new proof of this internal work.

1. GENERAL Principle stated.—The object of this paper is to state in a more general and comprehensive form than has hitherto been done to my knowledge, a thermodynamic and hydrodynamic principle of which many particular cases are well known and understood. That principle may be stated as follows:

In a steady stream of any fluid, the abstraction of heat at and near places of minimum pressure, and the addition of heat at and near places of maximum pressure, tend to produce acceleration; the addition of heat at and near places of minimum pressure, and the abstraction of heat at and near places of maximum pressure tend to produce retardation; and in a circulating stream the quantity of energy of flow gained or lost in each complete circuit is equal to the quantity of energy lost or gained in the form of heat; and in the absence of friction, the ratios borne by that quantity to the heat added and the heat abstracted (of which it is the difference) are regulated by the absolute temperatures at which heat is added and abstracted, agreeably to the second law of thermodynamics.

2. Equation of the Flow of a steady Stream without Friction. —Let a steady stream of any fluid, whether liquid, vaporous, or gaseous, flow in a suitable smooth passage or channel without friction. At a given point in the stream let \( v \) be the velocity, \( U \) the potential energy of attractive forces exerted on unity of mass of the fluid, \( s \) the bulkiness, or volume of unity of mass, and \( p \) the pressure. Then by a well-known equation of hydrodynamics we have

\[
vdv + dU + sdp = 0, \quad \cdots \cdots \cdots (1)
\]

or, in the integral form,

\[
\frac{v^2}{2} + U + \int sdp = \text{constant}; \quad \cdots \cdots \cdots (1A)
\]

that is to say, what each unit of mass gains in energy of flow (denoted by \( \frac{v^2}{2} \)) it loses in energy of head, as the quantity \( U + \int sdp \) may be called. When the attractive force considered is gravitation near the earth’s surface, we have \( U = g(z) \), \( z \) being the height above some fixed horizontal surface.

3. Thermodynamic Acceleration and Retardation.—Let it be supposed that a stream comes from a place where the pressure is \( p_1 \), and the potential energy of attraction \( U_1 \), flows through a

* Communicated by the Author, having been read to the British Association at Liverpool (Section A), September 1870.
place where the pressure is \( p_0 \), and the potential energy of attraction \( U_0 \), and finally arrives at a place where the quantities \( p \) and \( U \) have their original values \( p_1 \) and \( U_1 \). Let the fluid be in the condition called *adiabatic*—that is, let it neither receive nor give out heat. Then the relation between \( p \) and \( s \) is defined by the constancy of the *thermodynamic function*

\[
\phi = J c \text{ hyp. log } \tau + \chi(\tau) + \int \frac{dp}{d\tau} ds; \quad \ldots \ldots (2)
\]

in which \( J \) is the dynamical equivalent of a unit of heat, \( c \) the real specific heat of the fluid, \( \tau \) the absolute temperature, \( \chi(\tau) \) a function of \( \tau \) which is null for substances capable of approximating indefinitely to the perfectly gaseous state, and will be omitted throughout the rest of this paper; and in the integral \( \frac{dp}{d\tau} \) is taken on the supposition that \( s \) is constant. Then at the place where the pressure is \( p_0 \) the energy of flow in each unit of mass is expressed by

\[
\frac{v_0^2}{2} = \frac{v_1^2}{2} + U_1 - U_0 + \int_{p_0}^{p_1} s dp, \quad \ldots \ldots (3)
\]

the integral being taken subject to the condition that the thermodynamic function \( \phi \) has a certain constant value.

By the time that the stream has arrived at the place where \( U \) and \( p \) return to their original values, \( v \) also has returned to its original value \( v_1 \); and here there is no thermodynamic acceleration or retardation.

But next suppose that at the place where the pressure is \( p_0 \) each unit of mass has a certain quantity of heat either added to or abstracted from it, so as to change the thermodynamic function from \( \phi \) to \( \phi' \). That quantity of heat is expressed in dynamical units by

\[
\int_{\phi}^{\phi'} \tau_0 d\phi. \quad \ldots \ldots \ldots (4)
\]

Let the return to the original pressure take place with this altered value of the thermodynamic function. Then throughout this second division of the stream each value \( p \) of the pressure will have corresponding to it a value \( s' \) of the bulkiness suited to the new value of the thermodynamic function, and different from the value \( s \) corresponding to the same pressure in the first division of the stream. The relation between the change in the value of \( \phi \) and the change in the values of \( s \) is given by the equation

\[
\phi - \phi' = J c \text{ hyp. log } \frac{\tau}{\tau'} + \int_{s'}^{s} \frac{dp}{d\tau} ds. \quad \ldots \ldots (5)
\]
At the end of the process the stream, on arriving at the second place where the pressure is \( p_1 \), will now no longer return to the same velocity, but its energy of flow will be

\[
\frac{v_2^2}{2} = \frac{v_0^2}{2} - U_1 + U_0 - \int_{p_0}^{p_1} s' dp = \frac{v_1^2}{2} + \int_{p_0}^{p_1} (s - s') dp, \tag{6}
\]

and there will have been on the whole a change of energy of flow to the following amount,

\[
\frac{v_2^2 - v_1^2}{2} = \int_{p_0}^{p_1} (s - s') dp; \tag{7}
\]

which is a gain or a loss, corresponding to an acceleration or a retardation, according as it is positive or negative, its sign being the same with that of the product \( (p_1 - p_0)(\phi - \phi') \).

4. Circulating Stream.—Let the place at which the stream arrives and where the pressure is \( p_1 \) be the same with that from which it sets out; and on the return of each particle to that place let a quantity of heat be abstracted or added, as the case may be, so as to restore the thermodynamic function to its original value \( \phi \). That quantity of heat is expressed by

\[
\int_{\phi}^{\phi'} \tau_1 d\phi. \tag{8}
\]

Then in the course of each complete circuit made by a unit of mass of the fluid in that stream there is a change in the energy of flow to the amount expressed by equation (7); and accordingly as that change is a gain or a loss, there is on the whole a disappearance or a production of heat to an equivalent amount, expressed by

\[
\int_{\phi}^{\phi'} (\tau_1 - \tau_0) d\phi = \int_{p_0}^{p_1} (s - s') dp. \tag{9}
\]

5. Examples.—Amongst particular cases of the thermodynamic acceleration and retardation of streams the following may be specified.

Acceleration by the addition of heat at and near a place of maximum pressure:—the draught of a furnace; and the production of disturbances in the atmosphere in regions where the ground is hotter than the air.

Retardation by the abstraction of heat at and near a place of maximum pressure:—the dying away of atmospheric disturbances in regions where the ground is colder than the air.

Acceleration by the abstraction of heat at and near a place of minimum pressure:—the injector for feeding boilers, in which a jet of steam, being liquefied by the abstraction of heat, is enabled not only to force its way back into the boiler, but to sweep a
current of additional water along with it; also, to a certain extent, the ejector-condenser.

6. Retardation by Conduction.—The conduction of heat from the parts of a stream where the pressure and temperature are highest to the parts of the same stream where the pressure and temperature are lowest, produces, according to the foregoing principles, a gradual and permanent retardation of the stream, independently of the agency of friction; and this is accompanied by the production of heat to an amount equivalent to the lost energy of flow.

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

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The Rev. J. M. Heath, in his paper "On Thermodynamics," published in the Philosophical Magazine for September, states, with reference to certain dynamical principles, that I have admitted, by my silence, that those principles were ignored by the earliest original investigators in the science. This is a misconception of the meaning of the statement made by me in my letter published in your August Number, which was as follows:—"This very principle" (that is, the principle that the work done by a force in overcoming attractions and repulsions cannot take effect in accelerating molecular motions) "has been most carefully kept in view by every author of original researches in thermodynamics, and by every writer on the subject who has understood those researches." I did not keep silence respecting the earliest original investigators; for my statement obviously comprehends every original investigator, early or late. The only writers as to whom I kept silence were those who had not understood the original researches.

Mr. Heath then refers to the more modern writers having "altered their creed and language" in reference to the principle just referred to. As regards this, I have to state that, so far as my knowledge of the writings of original investigators and of those who have understood the results of their researches extends, there has been no such alteration in the course of the twenty-one years which have elapsed since the mathematical principles of thermodynamics were first set forth in a complete and systematic form. The notation and the methods of demonstration have been simplified, and the principles have been tested by new experiments and applied to new problems in theory and practice;
but as regards the fundamental principles themselves there has been no alteration.

It is, of course, to be understood that I do not, in making the preceding statement, contradict any allegation of Mr. Heath's as to matters of fact; for doubtless his information as to the history and state of thermodynamics must have been derived from writings with which I am unacquainted.

I have only to add the following remark as to Mr. Heath's explanation of how he conceives that the capacity of a vessel containing moving particles not exerting sensible attractions or repulsions might be diminished without accelerating the motion of the particles. He supposes the piston to be moved inwards during the intervals between the impulses of the moving particles upon it, and to be at rest at the instant of each of those impulses. According to this supposition there would, of course, be no addition to the energy of the particles; but at the same time there would be no work done in moving the piston, for it would meet with no resistance to its inward motion, and all the energy expended by the external force in setting the mass of the piston in motion after each impulse of a particle would be obtained back again in the act of stopping it before the impulse of the next particle.

As to the proposition that when work is done in moving the piston inwards against the reactions due to the motions of the particles alone, the additional energy due to the acceleration of the particles is exactly equivalent to that work, it might be treated simply as a particular case of the general principle of the conservation of energy. For elementary purposes, however, it may be desirable to use a special demonstration, such as the following. Let the piston be moving inwards with the uniform velocity $+u$. At a given instant let $-v$ be the normal component of the velocity, relatively to the vessel, with which a set of particles are moving towards the piston. The normal velocity of those particles relatively to the piston is $-(v + u)$. They strike the piston and rebound with the velocity $+v + u$ relatively to it, so that after the collision their velocity relatively to the vessel has become $v + 2u$ instead of $v$, which it was before.

Let $m$ be the aggregate mass of the particles which act on the piston in one second. Their total change of velocity is $2(v + u)$; therefore the outward pressure exerted by them on the piston, and the inward pressure exerted by the external force which pushes the piston inwards, are each equal to $2m(v + u)$. The piston moves through the distance $u$ in a second; therefore the work done in driving the piston inwards in each second is

$$2mu(v + u) = 2m(uv + u^2).$$

(1)
Dr. W. J. M. Rankine on Thermodynamics.

The aggregate energy of the mass \( m \) of particles before the collision is \( \frac{mv^2}{2} \), and after the collision \( \frac{m(v + 2u)^2}{2} \); therefore the increase of the energy of the moving particles in one second is

\[
\frac{m}{2} \left( (v + 2u)^2 - v^2 \right) = 2m(uv + u^2), \quad \ldots \quad (2)
\]

being exactly equal to the work done in pushing the piston inwards.

For the sake of simplicity, the preceding demonstration has been applied to elastic particles striking a piston and rebounding; but the same principle can be proved for any mode of internal motion of matter in a confined space; and when the particles exert attractions and repulsions, it can be proved for the work done by that term in the value of the external pressure which is equal and opposite to the outward pressure due to the motions of the particles—a term which, according to the second law of thermodynamics, is found by performing the operation \( \frac{d}{dT} \) upon the external pressure, \( \tau \) being the absolute temperature. For example, when a mass of saturated vapour occupying the space \( S \), at the pressure \( p \), and absolute temperature \( \tau \), is compressed into the liquid state, occupying then the volume \( s \), the temperature and pressure during the compression being maintained constant by abstracting the heat produced, the principle just mentioned gives for the amount of that heat in dynamical units

\[
\tau \frac{dp}{dT} (S - s),
\]

a result which is known to be exactly confirmed by experiments on various fluids.

For detailed information, however, on this and other points I must again refer to Professor Tait's treatise on Thermodynamics, and to the original papers referred to in that treatise.

I am, Gentlemen,

Your most obedient Servant,

W. J. Macquorn Rankine.

Glasgow, September 10, 1870.
XXXV. On an Object-glass Spectral apparatus.
By Sigmund Merz*.
[With a Plate.]

In Gilbert's Annalen der Physik for 1823, Fraunhofer gives a description of his observations upon the spectra of the light of the fixed stars. He therein says, "I have recently constructed a large instrument arranged solely for this purpose; it is provided with an object-glass the aperture of which is 4 inches French measure (=4.26 English). The flint-glass prism of this instrument has an angle of 37° 40', and is of the same diameter as the object-glass."

These are, without doubt, the earliest experiments on stellar spectra. More than twenty years elapsed before further investigations on this subject were entered upon, first by Lamont, then by Donati, Secchi, Janssen, Huggins, Lockyer, and Zöllner, all of whom, however, had abandoned the method employed by Fraunhofer, and commenced their observations by means of an arrangement adapted to the eyepiece.

Father Respighi in Rome has recently endeavoured to reintroduce the method of observation employed by Fraunhofer. His first experiments gave such brilliant results that Father Secchi wrote to me thereupon as follows:—"Professor Respighi relates wonders to me about the prism you sent to him," and gave me the commission to construct an object-glass spectral apparatus for the refractor of the observatory of the Collegium Romanum.

Now, as the apparatus in question has been for some time forwarded to its destination, and as Father Secchi has given an account thereof in the Comptes Rendus of November 22, 1869, it may not be considered out of place to make mention of it in this periodical as well.

Fig. 1, Plate III. represents the arrangement as mounted for fitting on to the cell of the object-glass. Fig. 2 represents the apparatus with the prism removed. Fig. 3 shows the prism itself†. The refracting angle of this prism is 12°. It is worked out of the purest and most colourless flint-glass, so that the loss of light in traversing it may be taken to be =0. Its aperture is 6 inches French measure (=6.39 English). The setting is provided with the requisite arrangements for adjustment.

Now, in spite of this prism reducing the effective aperture of the 9-inch refractor at Rome (=9.59 English) by more than half, the illumination far exceeds that of the refractor with

† Fig. 3 is of the size of the figure in the original plate. Figs. 1 and 2 are considerably reduced.
the full aperture of 9 inches when an eyepiece-spectroscope with
direct vision is employed. (Secchi says, "the aperture of the re-
fractor becomes thus reduced by more than half its surface, yet
notwithstanding the light is so intense that it greatly exceeds
that which one obtains with interposed direct-vision prisms near
the eyepiece. The dispersion is so considerable that it is at
least six times that which I have obtained with the most pow-
erful spectroscopic eyepiece, and even that of the spectroscope
without slit, with cylindrical lens, which I originally employed.")

The price of this object-glass spectral apparatus amounted to
525 Bavarian florins, equal to £45.

With a certain loss of light there could, however, be given to
this object-glass spectral apparatus for analyzing stellar light a
disposition for observing by direct vision. Within these few days
an object-glass spectral prism with direct vision has been com-
pleted, composed of crown- and flint-glass, which has completely
borne out what was anticipated from it. This arrangement has
an aperture of 34 French lines (=3.02 English inches); and its
dispersive power is equivalent to that of a flint-glass prism the
refracting angle of which is 26°.

XXXVI. On the Action of Low Temperatures on Supersaturated
Saline Solutions. By Charles Tomlinson, F.R.S.*

TWO years ago I had the honour of bringing before the
Chemical Section of the British Association an account of
an experiment in which it was shown that crystals of magnesic
sulphate, sodic sulphate, and one or two other salts do not neces-
sarily act as nuclei to their supersaturated solutions. My object
was to show, by an extreme case, that if a body which is usually
a most powerful nucleus be made chemically clean, the solution
adheres to it as a whole, and there is no separation of salt.

I propose to-day to bring before the Section another case, which
seems to me as extreme as the former one. It is intended to
illustrate this position, namely that, in the absence of a nucleus,
highly supersaturated solutions reduced to the zero of Fahren-
heit's scale, or from that to -10°, solidify into unstable hy-
drates rather than crystallize; and on exposing such solidified
solutions to a temperature of about 32°, they rapidly liquefy into
clear, bright, supersaturated solutions without any separation of
salt. This effect may be produced any number of times, pro-
vided the solution be preserved from the action of nuclei or
 carriers of nuclei, such as the air. The only precaution to this

* Communicated by the Author, having been read before the Chemical
Section of the British Association at Liverpool, September 20, 1870.
end is to use clean filtered solutions in clean tubes, kept plugged with cotton-wool.

For example, the sulphates of zinc and magnesia in atomic proportions, with a small quantity of water, are heated in a flask, boiled and filtered into test-tubes in which they are again heated nearly to boiling, and then plugged and set aside to cool. When cold, one of the tubes is put into a freezing-mixture at about \(-10^\circ\); and in about ten minutes or so large tetrahedral crystals beautifully formed begin to grow, as it were, from the side of the tube, and they go on increasing until the whole of the solution is transformed into a solid mass. If the tube be now transferred to snow and water at \(32^\circ\), the solid melts rapidly and the solution becomes clear as before. If, however, the cotton-wool be removed for a moment, either when the solution is solid or liquid, it crystallizes, in the one case during the melting, and in the other immediately.

The solidification of the solution must not be regarded as case of freezing, since no ice is formed; it is rather a case of abnormal crystallization of the saline molecules in combination with the water, and capable of existing only at this low temperature under the defined conditions. I call it a case of abnormal crystallization, because most of the solutions that were tried behaved in the same manner; that is, they formed tetrahedral crystals at about \(0^\circ\) and melted rapidly at \(32^\circ\).

Take another example. A supersaturated solution of the double sulphate of copper and magnesia was reduced to about \(-4^\circ\) F., when tetrahedral crystals formed on the surface, the solid angles growing downwards until the whole of the solution became solid. The deep blue colour had disappeared, and the solid presented different shades of very light blue. When the tube was put into snow and water at \(32^\circ\), the solid retreated from the sides of the tube, and the clear solution appeared intensely blue in contrast with the nearly white solid.

Sulphate of zinc and potash-alum in atomic proportions formed tetrahedral crystals at \(4^\circ\). The triple salt was afterwards crystallized in an open dish, and 200 grains of it, boiled with five drachms of water, formed a clear solution, which was filtered into a clean tube, plugged, and left to repose during six days. It was then placed in a freezing-mixture at \(0^\circ\), when a white powder, probably of a basic sulphate of alumina and anhydrous sulphate of zinc, was thrown down, and on this grew a brilliant white foliace resembling ivy, having an exquisite effect. This ivy-leaf pattern seems to me to result from deformations of the tetrahedral crystals, as I believe was also the case with a supersaturated solution of the double sulphate of copper and nickel, which at \(0^\circ\) formed beautiful feather-shaped crystals. A supersaturated
solution of sulphate of iron gave tetrahedral crystals, while the sulphate of zinc and ammonia at $4^\circ$ formed beautiful feathery crystals. The solution of the last-named double salt consisted of 80 grains of the salt, previously crystallized in an open dish and dissolved in $\frac{3}{8}$ of an ounce of water. In this case one of the salts, although anhydrous, did not interfere with the formation of a supersaturated solution; but sulphate of potash, from its comparatively small solubility, does so interfere in all the cases I have tried.

I think there can be little doubt that the tetrahedral crystals referred to are hydrated compounds of the salts in solution; and when such salts form supersaturated solutions, they exist, as I believe, in the anhydrous form. When such solutions are considerably reduced in temperature, the water of the solution cannot crystallize and form ice, because the saline molecules are too numerous to be excluded from the aqueous molecules; but the saline molecules, being brought by the low temperature within the range of their mutual attractions, form crystalline masses enclosing the water which suspends and surrounds them.

I have not been able to determine the amount of hydration of these tetrahedral crystals. The problem is a difficult one, how to examine crystals which apparently owe their existence to a low temperature and the absence of nuclei. Perhaps some member of the Section may be able to give me a hint on the subject. I may mention that in one case, soon after the tetrahedral crystals had begun to form in the middle of the tube and attached to its side, the tube was taken out of the freezing-mixture and held in air at about $50^\circ$. The crystals split up, and threw down to the bottom of the tube a quantity of anhydrous powder, which immediately began to combine with water and to rise in temperature, so that, on putting the tube into snow and water, it immediately acquired a thick coating of ice, except at the bottom, where, to the height of about a quarter of an inch, the tube was quite free from ice.

When a supersaturated solution of sulphate of soda is put into a freezing-mixture, it throws down octahedral crystals of the anhydrous salt, which take up water and form the abnormal seven-atom salt. This cannot be exposed to the air without heating and fixing three additional equivalents of water. Highly supersaturated solutions begin to throw down the anhydrous salt at various temperatures below $60^\circ$. But if undisturbed, it may happen that such a solution will resist a temperature of $18^\circ$ or $20^\circ$ F. without any apparent change. During the last winter I had a globular flask full of a solution (3 salt to 1 water), and a portion of it occupied the neck. It was placed over night on the window-ledge, and next morning the register-thermometer outside showed a minimum of $18^\circ$. There was no

deposit of crystals in the flask; but on gently shaking it the clear transparent solution immediately became opaque from the multitude of octahedral crystals that at once started into existence. These quickly subsided; and on the deposit a fine crop of the seven-atom salt soon began to form, consisting of flat plates or prisms with oblique summits from which heat-currents ascended, while the liquor above, consisting of a solution of the anhydrous salt, became quite clear.

It is perhaps useless to speculate on the condition of this solution, reduced, as it was, perhaps $30^\circ$ below the point at which it usually deposits anhydrous crystals. Did they start into existence in a moment? or were they present in the solution in some molecular condition in which they were not visible because they had the same index of refraction as the solution, until by shaking they threw off, as it were, the water external to the molecular groups and assumed an independent existence and their own peculiar refractive density and so became visible? When water is cooled many degrees below its freezing-point, is it a supersaturated solution of ice? and is the ice really present, but not visible, from the same cause? But the salt, it may be said, cannot be present and invisible, or it would fall by its weight. But the viscosity of the solution may prevent this; and is there not a condition of close adhesion within the body of a liquid, less than that required for solution, greater than that required for the independent existence of a crystal, by which a heavier solid is suspended and is not visible until, by some slight mechanical disturbance, that close adhesion becomes less, and the solid then assumes a more independent existence and subsides by its weight?

The double salt salt formed by mixing the sulphates of zinc and magnesia in atomic proportions forms with boiling water a solution which, when filtered into a clean tube and reduced to about $20^\circ$ F., behaves very similarly to supersaturated solutions of Glauber's salt at higher temperatures; that is, the double salt forms at the bottom of the tube acicular crystals of a lower degree of hydration, leaving the solution above still supersaturated with the double salt. On removing the cotton-wool, crystallization of the double salt sets in from the surface; and when this salt reaches the mass at the bottom, the latter becomes of an opaque white from splitting up into minute crystals and appropriating an additional quantity of water of crystallization. If before the solution crystallizes it be poured off and the modified crystals be pressed between folds of filtering-paper, they immediately begin to get warm from the fixation of water, and their constitution becomes changed.

This modified salt of zinc and magnesia, like the seven-atom
on Supersaturated Saline Solutions.

sulphate of soda, is of course quite different from the tetrahedral crystals already noticed, being much more permanent, even at atmospheric temperatures, and not changing so long as it is covered by the solution. But, like the seven-atom sulphate of soda, it cannot exist if exposed to the air; for it immediately fixes an additional quantity of water and becomes the normal salt.

The sulphate of zinc and soda, as also the sulphate of zinc and cadmium, also form supersaturated solutions which at about 10° F. form compounds resembling the tetrahedral crystals, of a silky texture, which at about 50° rapidly melt, leaving the solutions clear and bright as before. At a lower temperature the sulphate of zinc and soda forms crystals of a peculiar character, which will be noticed in another paper.

By varying the strength of the solutions and the temperature to which they are reduced, as well as the time of exposure to cold, varied results may be obtained. Rüdorff* showed some years ago that, by employing saturated solutions of single salts and reducing them to low temperatures, ice is formed together with a hydrated compound of the salt in question. In my experiments with supersaturated solutions in close vessels chemically clean, the conditions are of course quite different, and no ice is formed. In some cases the solutions became viscid like syrup; but they never froze. For example, sulphate of soda and potash-alum in atomic proportions formed in an evaporating-dish an amorphous mass containing 55 per cent. of water of crystallization. A portion of this wetted with a few drops of water can by gradual heating be raised to the boiling-point without depositing the anhydrous salt. When filtered into tubes it may be kept for hours at about 0° F., when it becomes very viscid without any deposit of salt. Under the influence of a nucleus the solution crystallizes, the soda-salt apparently separating from the alum in the process.

In some cases the results vary, unless the double salt is first formed before a supersaturated solution is made with it. For example, 246.3 grains of sulphate of magnesia in large crystals and 322 grains of sulphate of soda, dry but effloresced, and ½ oz. of water were heated to boiling and filtered into two tubes. At about 0° the sulphate of soda crystallized separately in long lines, after which the magnesia-salt attached itself to the sides and grew upwards like a vegetable. The tubes were again heated to near boiling, and the solution was poured into a small evaporating-dish and placed on hot sand. Crystalline scales formed on the surface, consisting of a central boss surrounded by a flat ring with radial markings directed to the centre, and this by an-

* Jahresber. der Chemie, 1862, p. 20.

X 2
Mr. A. S. Davis on a Theory of Nebule and Comets.

other flat ring similarly marked. Twenty grains of the double salt heated in a porcelain crucible became 7½, thus giving 62½ per cent. of water of crystallization. One ounce of this salt with half an ounce of water was boiled and filtered into two tubes: when cold, the tubes were reduced to 0°, when, after some time, the cup-shaped cavity at the bottom of the tube became lined with an anhydrous powder, from which grew small, sharp, acicular crystals. After this the capillary curve of liquid at the surface became solid and shot down acicular crystals. Suddenly the sulphate of soda separated at the surface and shot downwards in the well-known crystalline lines.

But amidst all these variations the main conclusion from this inquiry is left undisturbed, namely, that highly supersaturated solutions, chiefly (but not entirely) of double salts, when preserved from the action of nuclei and reduced to low temperatures, form compounds of various degrees of hydration, which can exist only at such temperatures and in close vessels chemically clean.

Highgate, N.,
September 9, 1870.


To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

There is one point in the theory of nebuleæ and comets, published in the June Number of your Magazine, which may appear unsatisfactory. In that theory I suppose that nebuleæ are formed by masses of gas coming into collision and forming, where they may become mixed, a chemical union, with evolution of light and heat.

It may be objected that two masses of gas on becoming mixed when in a cold and highly rarefied condition do not generally combine, or do not at least combine with an energy sufficient to produce light. To this objection I would answer, that though the masses of gas on approaching each other are in a cold and highly rarefied state, yet when they meet, the parts which come into collision become so much condensed and heated that they are put into a condition very favourable to their chemical union. This I will endeavour to show.

When two masses of gas come into collision, the portions which first meet will act only to a very small extent as a cushion or buffer to the remaining portions; their velocity is so great that the increased pressure produced by the condensation at the colliding surfaces has not time to transmit itself into the interior por-
tions of the masses before those portions themselves come close up to the colliding surfaces. Now the pressure due to the sudden condensation produced by the collision cannot transmit itself with a greater velocity than that with which sound would travel through the gases. Hence, if the masses collide with a velocity much greater than that with which they would transmit sound, the internal portions of the masses would not be brought gradually to rest, but would move on with undiminished velocity until they were brought to a violent check by meeting with a partition of gases in an exceedingly condensed state. The sudden condensation thus produced, together with the heat evolved, would put the gases into a condition highly favourable to chemical union.

Now in the formation of nebulae I think that the velocity of the colliding masses would be very much greater than the velocity of sound, and therefore that the above-mentioned effects would be actually produced. Suppose, for example, that each of the masses of gas contained as much matter as the sun, and that their relative velocity was simply that due to their mutual gravitation from a great distance: their relative velocity when their centres of gravity were at a distance equal to the earth’s distance from the sun would be twice the orbital velocity of the earth, or 36 miles per second, a velocity very much greater than the velocity of sound in any gas whatever.

A. S. Davis.

Roundhay Vicarage,
September 13, 1870.

XXXVIII. Notices respecting New Books.

Researches on Diamagnetism and Magnecrystallic Action, including the question of Diamagnetic Polarity. By John Tyndall, LL.D., F.R.S. London, 1870.

In this volume are reprinted Professor Tyndall’s researches into the subjects mentioned in the title, together with abstracts of lectures on these or kindred subjects delivered by him in the Royal Institution, and several letters, essays, and reviews relating to similar topics. In connexion with his own investigations into Diamagnetic Polarity, Professor Tyndall has printed some extracts from papers and letters by Faraday, Sir William Thomson, and Weber, in order to make the theoretical discussion of the question more complete. The papers here collected date from 1850 and subsequent years.

As nearly the whole contents of the volume, with the exception of the abstracts of Royal Institution lectures, have already appeared in the pages of the Philosophical Magazine, any detailed comment upon it would be out of place here, and we therefore content ourselves with having thus announced its publication.
XXXIX. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 225.]

June 16, 1870.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communication was read:

“Note on the Spectra of Erbia and some other Earths.” By William Huggins, L.L.D., F.R.S.

Bahr and Bunsen have shown* that erbia, rendered incandescent in a Bunsen’s gas-flame, gives a spectrum of bright lines in addition to a brilliant continuous spectrum. As they were unable to discover the bright lines in the flame beyond the limits of the solid erbia, they suggest that the light which is dispersed by the prism into bright lines is emitted by the solid erbia, which substance therefore appears to stand alone, as a remarkable exception, among solid bodies. Bahr and Bunsen found the spectrum of bright lines to coincide very nearly with the absorption spectrum of some compounds of erbium.

A few weeks since, when in Ireland, I made the observation that the spectrum of the ordinary lime-light contains bright lines†. Dr. Emerson Reynolds, Director of the Laboratory of the Royal Dublin Society, kindly undertook to make experiments to ascertain from the position of the lines if they were due to the cylinder of lime, or to impurities contained in it.

Upon my return to town I made the following experiments; shortly after commencing them I received from Dr. Reynolds the account of his experiments, which, with his permission, I have added to this note.

Erbia.—A few months since, I received, through the kindness of Dr. Roscoe, F.R.S., a few grains of nitrate of erbia, which he had procured from a trustworthy source. I followed Bunsen’s method of placing it with syrupy phosphoric acid upon a platinum wire. The erbia, obtained by this method in a finely divided state, was then submitted to the heat of the oxyhydrogen blowpipe.

In all the experiments described in this paper hydrogen alone was first turned on, and the effect of the heat of the flame on the substance under examination observed with the spectroscope. Oxygen was then admitted slowly, and the effect of the increased heat carefully noted.

With the flame of hydrogen alone, the lines represented in the map which accompanies Bahr and Bunsen’s paper were seen; but the

* Liebig’s Annalen, Bd. lxi. (1866) S. 1.
† Dr. W. Allen Miller informs me that in 1845 he noticed a bright line in the spectrum of the diffused light of the oxyhydrogen jet reflected from a sheet of paper.
lines were more distinct when a small proportion of oxygen was admitted. With the full proportion of oxygen, the light from the glowing erbia was more intense, but the lines were not so well seen. Even with the intense heat of the oxyhydrogen flame I was unable to trace the lines beyond the limits of the solid erbia, though the line of sodium could be seen for some distance from the erbia. I found, however, that the lines appeared more distinct, in consequence, probably, of their being brighter relatively to the parts of the continuous spectrum where they occur, when the slit was directed from the side upon the gas immediately in front of the glowing part of the erbia.

The spectrum of bright lines obtained by means of the oxyhydrogen flame agreed more completely with the absorption spectrum represented by Bahr and Bunsen (No. 2 in their diagram) than the spectrum of bright lines figured by those observers (No. 3). The most important differences occurred in the band in the red, which showed two points of greatest brightness, thus forming a double line with a little outstanding light, and the line in the green at 65 of the scale, which was double, precisely as the corresponding absorption-line is represented in spectrum No. 2 of the diagram.

**Lime.**—The experiments were made with the cylinders of lime prepared for use with the oxyhydrogen blowpipe, and also with pieces of pure caustic lime; but there was no sensible difference presented in the spectroscope.

The bright lines consisted of a double line in the green, and several bands in the orange and red, which were found to form a spectrum identical with that which is produced when chloride of calcium is heated in the flame of a Bunsen's burner.

When the spectroscope was directed to a point in the flame a little above the incandescent portion of the lime, the lines appeared beyond the bright continuous spectrum, showing that they are not produced by the white-hot solid lime, but by the luminous vapour into which a portion of the lime has been converted by the heat of the flame.

**Magnesia.**—The commercial heavy oxide of magnesium was made into a paste with distilled water, and formed into a small pellet upon the end of a platinum wire. The pellet of magnesia was slowly dried, and then placed in the oxyhydrogen flame. I was surprised to see a spectrum of bright lines precisely similar to that which is produced by lime. Chloride of magnesium, when introduced into the Bunsen flame, gave a similar spectrum. I record these results as the oxide and chloride were those sold as pure. I found afterwards that a very small trace of lime may be detected in magnesia by means of the oxyhydrogen flame.

I then took metallic magnesium, which I had found by the spectroscope to be nearly pure, and formed from it magnesia and chloride of magnesium.

When this magnesia, formed into a small ball upon a wire, was subjected to the oxyhydrogen flame, two bright bands were seen in the green. One of these was found to be coincident with the triple
line of Fraunhofer’s b, which distinguishes magnesium, and the other with a group of bright lines which is seen between b and F, nearly in the position of the brightest double line of nitrogen, when metallic magnesium is burnt in air.

The chloride formed from magnesium, when introduced into the Bunsen flame, gave the same bands; but the more refrangible band was exceedingly faint.

When an induction-spark was taken from a wire covered with cotton-wool soaked with a solution of the chloride, the lines at b and the more refrangible group were seen. If the heating-power of the spark be increased by the introduction of a Leyden jar, the band between b and F becomes scarcely distinguishable, while the lines peculiar to metallic magnesium are much more intense. When a spark was taken between electrodes of the same specimen of magnesium from which the chloride was formed, no trace of this band was detected.

Baryta.—When pure caustic baryta is subjected to the heat of the oxyhydrogen flame, a brilliant spectrum is seen identical with the well-known spectrum which presents itself when chloride of barium is heated in the Bunsen flame. Baryta furnishes a larger quantity of vapour than lime and magnesia, and therefore the lines could be traced to a greater distance from the solid baryta.

Strontia.—Pure strontia was fused into a large bead upon a platinum wire. When this bead was heated by the oxyhydrogen flame, the same spectrum of bright lines presented itself as is seen when chloride of strontium is placed in the flame of a Bunsen’s burner.

Zirconia.—One of the small pellets of zirconia prepared in France for use with the oxyhydrogen blowpipe was found to give no trace of bright lines. This great fixity of zirconia as compared with lime is in agreement with the inalterability of the substance under the action of the oxyhydrogen flame.

Alumina.—Pure alumina treated in the same way as the magnesia gave a continuous spectrum only, without any trace of bright lines.

Glucina.—Glucina gave a bright line in the red, which I found to be due to potassium. Glucina, therefore, appears not to form vapour of any kind under the heat of the oxyhydrogen blowpipe.

Titanic acid gave a continuous spectrum without lines.

Oxide of uranium a continuous spectrum without lines.

Tungstic acid a continuous spectrum without bright lines.

Molybdic acid a continuous spectrum without bright lines.

Silica (precipitated) a continuous spectrum without bright lines.

Oxide of cerium a continuous spectrum without bright lines.

The question presents itself as to the nature of the vapour to which the bright lines are due in the case of the earths lime, magnesia, strontia, and baryta. Is it the oxide volatilized? or is it the vapour of the metal reduced by the heat in the presence of the hydrogen of the flame? The experiments show that the luminous vapour is the same as that produced by the exposure of the chlorides of the metals to the heat of the Bunsen gas-flame. The character common to these spectra, of bands of some width, in most cases gradually shading
of Erbia and some other Earths.

off at the sides, is different from that which distinguishes the spectra of these metals when used as electrodes in the metallic state.*

Roscoe and Clifton have investigated the different spectra presented by calcium, strontium, and barium; and they "suggest that, at the lower temperature of the flame or weak spark, the spectrum observed is produced by the glowing vapour of some compound, probably the oxide, of the difficultly reducible metal; whereas at the enormously high temperature of the intense electric spark these compounds are split up, and thus the true spectrum of the metal is obtained. In none of the spectra of the more reducible alkaline metals (potassium, sodium, lithium) can any deviation or disappearance of the maximum of light be noticed on change of temperature".+ 

As the experiments recorded in this paper show that the same spectra are produced by the exposure of the oxides to the oxyhydrogen flame, Roscoe and Clifton's suggestion, that these spectra are due to the volatilization of the compound of the metal with oxygen, is doubtless correct.

The similar character of the spectrum of the bright lines seen when erbia is rendered incandescent would seem to suggest whether this earth may not be volatile in a small degree, as is the case with lime, magnesia, and some other earths. The peculiarity, however, of the bright lines of erbia, observed by Bahr and Bunsen, that they could not be seen in the flame beyond the limits of the solid erbia, deserves attention. My own experiments to detect the lines in the Bunsen gas-flame, even when a very thin wire was used, so as to allow the erbia to attain nearly the heat of the flame, were unsuccessful. The bright line in the green appears, indeed, to rise to a very small extent beyond the continuous spectrum, but I was unable to assure myself whether this appearance might not be an effect of irradiation.

It is perhaps worthy of remark that the chlorides of sodium, potassium, lithium, caesium, and rubidium give spectra of defined lines which are not altered in character by the introduction of a Leyden jar, and which, in the case of sodium, potassium, and lithium, we know to resemble the spectra obtained when electrodes of the metals are used. Now all these metals belong to the monad group; it appeared therefore interesting to observe the behaviour of the other metal belonging to this group.

Chloride of silver when introduced into the Bunsen flame gave no lines. The chloride was then mixed with alumina (which had been found to give a continuous spectrum only) and exposed to the oxyhydrogen flame; but no lines were visible. When, however, the moistened chloride was placed on cotton and subjected to the induction-spark without a jar, the true metallic spectrum was seen, as when silver electrodes are used.

* For the spectra of metallic strontium, barium, and calcium, see Phil. Trans. 1864, p. 148, and Plates I. and II. Both forms of the spectra of these substances are represented by Thalen in his 'Spektralanalyse.'

+ Roscoe's Spectrum Analysis, p. 175, and Proc. Lit. & Phil. Soc. Manchester, April 1, 1862. See also A. Mitscherlich, 'Ueber die Spectren der Verbindungen,' S. 10.
The behaviour of silver, therefore, is similar to that of the other metals of the monad group. Now the difference in basic relations which is known to exist between the oxides of the monatomic and polyatomic metals is in accordance with the distinction which the spectroscope shows to exist in the behaviour of the chlorides: the chlorides of the polyatomic metals would be more likely to split up in the presence of water into oxides and hydrochloric acid.

In the case of some of the oxides and chlorides, one or more of the lines appeared to agree with corresponding lines in the metallic spectra; it may therefore be, that under some circumstances, as in the case of magnesium burning in air, the metallic vapour and the volatilized oxide may be simultaneously present.

Dr. Reynolds's Experiments.

"After you observed the occurrence of two bright lines in the spectrum of the light emitted by incandescent lime, you recollect we identified these as belonging to calcium. At the time we supposed that these lines were produced by the ignition of the vapour of some volatile calcium compound probably present as an impurity in the samples of lime used in the experiments. If this explanation were found to be true for lime, the bright lines seen in the spectrum of erbia might possibly be accounted for in a similar manner. In order to examine the matter fully, I arranged the experiments described below.

"I selected two oxides for comparison with erbia, viz. lime and magnesia. As it seemed desirable to prepare these oxides in precisely the same manner as the erbia, some calcium and magnesium nitrates were made chemically pure to ordinary tests, and then used in the preparation of the respective oxides.

"The oxyhydrogen flame was employed as the chief source of heat. The hydrogen was made from zinc and sulphuric acid in the usual way, and the oxygen from potassium chlorate. As both gases are certain to be contaminated with traces of acids, I took the precaution of passing each gas through a long tube filled with fragments of solid potassium hydrate. If this plan were not adopted, the traces of acid which would find their way into the hydrogen or oxyhydrogen flame might produce volatile compounds with the earths, and so lead to mistakes.

"1. Experiments with Magnesia.—A loop of stout platinum wire was moistened with syrupy phosphoric acid, and some magnesium nitrate made to adhere. The nitrate was then heated in the hydrogen flame, and a residue of magnesia obtained. No lines were observed in the spectrum of the light emitted by the incandescent earth; and when the latter was intensely heated in the oxyhydrogen jet, only a continuous spectrum was seen*.

* "Since writing the above, I have succeeded in observing the bright lines described by Mr. Huggins as occurring in the spectrum of the flame surrounding the incandescent magnesia. In the earlier experiments I probably admitted too much oxygen to the mixed gas-flame in the first instance."
"2. Experiments with Lime.—A platinum wire of the same thickness as the last was moistened with the phosphoric acid, some calcium nitrate was then taken up in the loop, and heated in the hydrogen flame until a residue of lime was obtained. At the outset the calcium-spectrum was observed; but the light speedily gave only a continuous spectrum. The lime and loop of wire were kept well enveloped in the hydrogen flame for nearly half an hour, in order to ensure the complete decomposition of the nitrate. During this time no lines could be detected on the background of the continuous spectrum, or in the spectrum of the flame surrounding the lime. More hydrogen was now turned on and oxygen slowly admitted, the light being examined with the spectroscope during the time. When the proportion of oxygen had reached a certain point, faint traces of the two brightest Ca lines appeared on the bright background; and the intensity of these lines increased with the amount of oxygen admitted, up to a definite extent. When a certain proportion of oxygen was exceeded, the lines became less distinct. The best results were obtained when the hydrogen was decidedly in excess of the oxygen in the flame—that is to say, more than in the proportion of 2:1.

When the slit of the spectroscope was pointed in such a way that only the light from the flame surrounding the incandescent lime entered the instrument, all the Ca lines and bands were observed with great ease without a continuous spectrum. On looking at the mantle of flame with the naked eye, it was easy to perceive a reddish tinge. I next maintained the small fragment of lime at the highest temperature its supporting wire was capable of resisting for three hours; at the end of this time the Ca lines were as strongly marked as before, and the lime on the wire had very appreciably diminished in amount. The same results were obtained when no phosphoric acid was employed to attach the calcium nitrate to the wire in the first instance.

Again, a piece of well-burned quicklime, of very small size, was heated alone on a platinum wire for more than an hour; and the bright Ca lines were seen during the whole time.

From the results of these experiments we must draw the conclusions:—(1) that when lime is sufficiently heated the light which it emits is derived in part from the incandescent solid, and partly from ignited vapour; (2) that lime is either volatile as such, or that in the first instance it suffers reduction by the excess of hydrogen in the flame, the luminous vapour of calcium then giving its own peculiar spectrum.

"3. Experiments with Erbia.—The specimen of erbium nitrate which you kindly gave me was attached to a platinum loop with syrupy phosphoric acid as usual, and decomposition of the salt effected in the plain hydrogen flame. After heating for a short time in this way, the chief green line of erbia became visible, but seen upon the continuous spectrum. Oxygen was now turned slowly into the flame. As the temperature rose, two of the other bright lines of the earth were seen. The best observations were made when the oxyhydrogen flame had hydrogen in excess, and the erbia was kept in such a position that it was very strongly ignited. The
erbia lines were most distinctly seen when the slit of the spectroscope took in the light from the extreme edge of the incandescent solid. When the bright lines were best observed, the continuous spectrum was relatively faint. Again, when the slit was made to cut the edge of the ignited bead of the earth, the strong green line of erbia was seen to extend to a very small but appreciable distance above or below (as the case might be) the continuous spectrum. I could only observe this for the strong line. I failed to get any trace of lines in the spectrum of the flame beyond the incandescent erbia.

"The erbia was next heated in the oxyhydrogen flame to the maximum temperature that the wire would bear for three and a half hours; but the green line was seen to be just as strongly marked at the end as at the beginning of the experiment. The bulk of the erbia was so much reduced by this treatment, that I have now scarcely a trace left.

"From the results of these experiments, I think we must conclude:—(1) that the light emitted by incandescent erbia is derived chiefly from the ignited solid, but that the bright lines observed in its spectrum have as their source a luminous vapour of extremely low tension at even the highest temperature of the oxyhydrogen flame; (2) that this interrupted spectrum belongs either to erbium or to its oxide.

"If these conclusions are true, it follows that erbia is not an exception to the ordinary law.

"It would appear that in these experiments three substances have been employed, varying in their degree of volatility. At the temperature of the oxyhydrogen flame magnesia appears to be less volatile than lime; but I am in doubt what relative volatility to assign to erbia, since its spectrum of bright lines can be seen when the earth is heated in the plain hydrogen flame, and yet at the much higher temperature of the oxyhydrogen jet the volume of luminous vapour does not appear to materially increase.

"Finally, we have yet to learn whether or not in all these cases reduction of the oxide precedes volatilization; if reduction takes place, the luminous vapour must be that of the metal. The settlement of this question would no doubt be very difficult. But I rather incline to the view that the vapour whose spectrum is obtained on igniting these earths is that of the metal; for I find that the bright lines are most easily observed when hydrogen is present in excess in the oxyhydrogen flame. Moreover the actual amount of matter volatilized on very prolonged heating is really very small; and this circumstance appears to favour the view that a slow surface-reduction is in progress."
GEOLOGICAL SOCIETY.

[Continued from p. 227.]

March 23rd, 1870.—Warington W. Smyth, Esq., M.A., F.R.S.,
Vice-President, in the Chair.

The following communications were read:—

1. Professor Huxley communicated a letter received by him from
Dr. Emanuel Bunzel, of Vienna, giving a short account, illustrated
with figures, of the posterior portion of a skull obtained by Professor
Suess from a coal-mine of Upper Cretaceous (Gosau) age. Dr. Bun-
zel stated that at the first glance this skull appeared to possess
Reptilian characters, but that the convexity of the occiput, and its
gentle passage into the roof of the skull, the presence of a transverse
ridge in the occipital region, the absence of sutures, the globular
form of the condyle, and some other peculiarities prevent the animal
to which this skull belonged from being referred to any known order
of Reptiles. The author compared this fragment of a skull with that
of a bird, and suggested the establishment of a new order of fossil
Reptiles (Ornithocephala), closely related to Prof. Huxley’s Ornitho-
scelida. He proposed to refer his fossil to a new genus, which he
named Struthiosaurus.

of Trinidad.” By R. J. Lechmere Guppy, Esq., F.L.S., F.G.S.

The author described the rocks of the “Caribbean Group” as con-
sisting of gneiss, gneissose, talcose, and micaceous slates, and crystal-
line and compact limestones, and remarked upon the probable distrib-
ution of rocks of the same series on the continent of South America.
In Trinidad the uppermost member of the series is a compact dark
blue limestone, which contains obscure but very abundant fossils;
in the subjacent clay-slates and quartz rocks calcareous strings and
bands containing more distinct traces of organisms occur. The
author believed that he had detected an Eozoön (which he called
E. caribbeum), a Favourites (named F. fenestralis), a Coral, and frag-
ments of Echinoderms. He considered it probable that the Caribbean
series was pre-Silurian.

3. “On the Palæontology of the Junction-beds of the Lower and
Middle Lias in Gloucestershire.” By Ralph Tate, Esq., A.L.S.,
F.G.S.

The object of this paper was to show that the attachment of the
zone of Ammonites raricostatus to the Lower Lias and that of A.
Jamesoni to the Middle Lias harmonizes with the distribution of the
organic remains: 50 species were catalogued from the united zones
of A. oxynotus and A. raricostatus, 8 of which pass up into the
Middle Lias, whilst 13 occur in the lower horizons; 115 species were
enumerated as occurring in the zone of Ammonites Jamesoni, 60 of
which pass to higher zones, whilst 11 made their first appearance in
the Lower Lias,—the number of species common to the contiguous
zones being 14.

The author inferred that, as the conditions of depth and deposit of
the upper part of the Lower Lias are repeated in the lower part
of the Middle Lias, accompanied by a total change in the fauna, a
break in the stratigraphical succession existed between the Lower
and Middle Lias. This view is supported by the fact of the nume-
rical decrease of species in passing up through the several stages
of the Lower Lias, and that of the introduction of many new generic
types with the zone of *Ammonites Jamesoni*. Many new species were
described.

By T. H. Cockburn Hood, Esq., F.G.S.
In this paper the author described the general features of the
locality from which he has obtained bones of *Plesiosaurus, Ichthy-
osaurus*, and *Teleosaurus*. The bones were not obtained in situ, but
from large boulders and blocks scattered in the ravines of the Wa-
para and its tributaries.

5. R. H. Scott, Esq., F.G.S., communicated an extract from a
letter addressed to him by M. Coumbary, Director of the Imperial
Observatory of Constantinople, containing an account received from
M. L. Carabello of the reported fall of a large meteorite near Mor-
zouk, in the district of Fezzan, in lat. 20° N., and long. 12° E. of
Paris. It fell on the evening of the 25th December last, in the form
of a great globe of fire, measuring nearly a metre in diameter; on
touching the earth it threw off strong sparks with a noise like the
report of a pistol, and exhaled a peculiar odour. It fell near a group
of Arabs, who were so much frightened by it, that they "immediately
discharged their guns at this incomprehensible monster."

XLI. *Intelligence and Miscellaneous Articles.*

**EXPERIMENTAL RESEARCH ON THE INFLUENCE OF HEAT ON
ELECTROMOTIVE FORCE.**  *AY DR. L. BLEEKRODE.*

*This question has already been the object of the researches of
several physicists. Until the present time, only three cases have
been considered—(1) when metals are placed in contact with acids,
(2) saline solutions with one another, (3) metals with their saline
solutions.

Faraday commenced this investigation in 1840*. He heated one
of the branches of a U-tube containing an acid in which two identi-
cal metallic wires were immersed. The current was strong when
the metal was attackable, weak when gold or platinum was used:
in the latter case Faraday considered the current thermoelectric.

Wild† experimented on liquids in contact with one another. He
still considered the current to be thermoelectric, but he tried in vain
to reproduce with liquids the phenomena of Peltier.

Lindig‡ placed in contact two electrodes of the same metal with
a solution of that metal; he confined himself to zinc and copper,
because he proposed to investigate whether the electromotive force
of Daniell’s battery varies with the temperature.

* Philosophical Transactions, 1840.
Dr. Bleekrode has varied these experiments considerably. He immersed two electrodes of the same metal in two vessels containing a salt of this metal, united by a siphon. One of these vessels was maintained at nearly & constant temperature, whilst the other, placed in a hot air-bath, was gradually heated. The electrodes were connected by a circuit, including a coil whose resistance was such to render imperceptible the variations produced by heat in the conductivity of the liquid; and in the second place a reflecting galvanometer, the deflections of which never exceed 30°, measured directly the intensities. As the resistance of the circuit remained sensibly constant, these deflections were also proportional to the electromotive forces.

The memoir by Dr. Bleekrode is very rich in numerical results. These results are collected in twenty-one Tables, of which the majority are represented by curves. Some of these curves do not differ much from the straight line, so that the electromotive force is proportional to the difference of the temperatures; this is what takes place, for instance, with copper in sulphate or nitrate of copper, and with amalgamated zinc in sulphate or chloride of zinc; but it is rarely like this.

In general the current goes through the siphon from the cold vessel to the heated vessel, except with silver immersed in acetate or nitrate of silver, when it is in a contrary direction.

The most curious cases are those in which the current changes sign. Of these the author distinguishes three. When amalgamated zinc is immersed in a solution of double cyanide of zinc and potassium, the current is at first positive or passes from cold to hot; then it changes direction when the difference of temperatures exceeds 30°; it is the same at 53° for silver in double cyanide of silver and potassium. Lead in nitrate of lead is still more singular: the couple is at first negative (the current passes from hot to cold); then it changes direction when the difference of temperatures exceeds 21°, and again changes and becomes negative when the difference reaches 51°.

Too much importance must not be attached to the numerical determinations of electromotive forces, because the composition of the liquids changes under the action of the current, and soon produces other currents which are superposed on the first and produce heterogeneity of the solutions in which the electrodes are immersed. This phenomenon is particularly remarkable with silver immersed in nitrate of silver. Although in this case the current passes from hot to cold, it produces in the heated vessel some free acid at the same time that it deposits some metallic silver, partly black and partly crystallized under an arborescent form.—Poggendorff’s Annalen, vol. cxxxviii. p. 571; Annales de Chimie, April 1870.

ON TESTS FOR THE PERFECTION AND PARALLELISM OF PLANE SURFACES OF GLASS. BY WOLCOTT GIBBS, M.D., RUMFORD PROFESSOR IN HARVARD UNIVERSITY.

When a plano-convex lens of long radius of curvature is placed
Intelligence and Miscellaneous Articles.

upon a plane surface of glass and the system is illuminated by an obliquely incident beam of monochromatic light, as, for example, by a sodium-flame, the well-known phenomenon of Newton's rings is observed with remarkable distinctness and perfection of definition. The symmetry of the rings will depend, in part, on the perfection of figure of the lens, in part on that of the plane surface. An extremely minute deviation from a perfect plane will produce a marked distortion of the circular figure of the ring nearest the centre. That this distortion is or is not due to the lens may be determined by rotating the lens round its optical axis normal to the plane. No change of figure will be seen if the lens is perfect in form and the inequality is in the plane surface only. Different parts of the plane surface may of course be tested in succession, by moving the lens from point to point; and, if necessary, the rings may be observed with a telescope.

Prof. Rood, of New York, has suggested for the observation of Newton's rings a method which permits of the employment of a lens of comparatively small radius of curvature and a microscope. In his arrangement the lens and plate of glass are placed upon the stage of the microscope, the light from beneath being cut off; and monochromatic light is then thrown down upon the system by means of a plate of glass with parallel surfaces inclined to the axis of the microscope at a convenient angle, and placed between the objective and the plano-convex lens. In this manner the rings are seen with great distinctness and beauty, and the arrangement is particularly compact and convenient.

The interference bands of Talbot afford a method not merely of observing with great precision the inequalities of surface and want of parallelism of the faces of plates of glass, but also of photographing these defects and obtaining a permanent chart of the glass which may be of material assistance in correcting its figure. It is only necessary for this purpose to place the glass to be examined near to the object-glass of the collimator and perpendicular to its axis, so as to intercept that half of the bundle of parallel rays which falls upon the first surface of the first prism nearest its refracting edge. If the plate has perfectly plane and parallel surfaces, the interference bands will be sharply defined and parallel in the whole field of view. The slightest inequality of surface or inclination of the faces will produce curvature or distortion of the bands; and if the eyepiece of the observing-telescope be removed, the image may be received on a sensitive plate and photographed. The number of prisms to be employed in a particular case will depend upon the thickness of the plate of glass examined, and, in general terms, upon its dispersive power. For a piece of French plate glass four millimetres in thickness, two bisulphide-of-carbon prisms of $60^\circ$ must be used to produce a sufficient separation of the interference bands to enable them to be seen distinctly. More prisms must be used for thicker plates; and in this way a limit is soon reached at which the method ceases to be applicable.—Silliman's American Journal, July 1870.
By Professor F. Zöllner*.

[With a Plate.]

§ 1.

Among the characteristic forms of the protuberances† which the spectroscope used with wide slit now enables every one to examine, a considerable number convince the observer at once that we have here to do with enormous eruptions of incandescent hydrogen.

Without stepping beyond the range of known analogies, and therefore of conditions explanatory of cosmical phenomena, it is scarcely possible to find a cause for these eruptive protuberances other than that of a difference of pressure between the gases in

* Translated from a separate impression, communicated by the author, from the Proceedings of the Royal Saxon Society of Sciences, June 2, 1870.
† The protuberances may be classed in two characteristic divisions, according to their shapes, viz. into the vapour- or cloud-forms, and the eruptive forms. The predominance of the one or of the other type appears to depend partly upon local conditions of the solar surface and partly upon periodic variation; so that at one time one type may prevail, whilst at another time the other type may be most strongly developed. It is easy to see why the cloudy prominences so closely correspond in form to terrestrial clouds and vapours, when we remember that the forms of our clouds depend not upon the suspended vesicles of water, but upon the mode in which different masses of heated and moving air are distributed. The vesicles in terrestrial clouds form only the material by means of which this difference in the masses of air is rendered visible. The clouds of the solar protuberances are rendered visible by the light emitted by the masses of glowing hydrogen.

the interior and those on the surface of the sun. The possibility
of such a difference of pressure, however, necessitates the exist-
ence of a zone of separation between the interior and exterior
masses of hydrogen, the latter of which has been shown to form
an essential portion of the solar atmosphere.

The supposition of such a layer of separation is so forcibly im-
pressed on the mind on the first sight of such an eruptive pro-
tuberance, that it even suggests itself to observers who, like
Respighi, suppose that electricity may be the cause of these solar
volcanic phenomena.

If, however, we admit only the simpler and therefore more
probable hypothesis of the difference of pressure, we have before
us a phenomenon which, by the application of the mechanical
theories of heat and of gases, may be made to yield us most im-
portant conclusions concerning the temperature and physical
constitution of the sun. The object of the present communica-
tion is to exhibit the fertility of this mode of attacking the
question.

The mechanical theory proves for perfect gases:—
(1) The law of Mariotte and Gay-Lussac.
(2) The constant relation of specific heat with constant volume
and with constant pressure.

These constants, determined by means of well-known methods
for a given gas, must therefore, from the point of view of the
mechanical theory of gases, be considered unalterable, in the
same way as the atomic weight of the elements; and they cer-
tainly must not be placed in the category of other empirical con-
stants, such as the conductive power of bodies for heat, or the
coefficients of expansion of solid or liquid bodies. These con-
stants only apply within the limits for which they have been
ascertained by experiment, and altogether lose their significance
when applied far beyond these limits.

Upon this assumption I consider the eruptive protuberances
as a phenomenon of the issue of a gas from one space to another,
in such a way that the pressure during the issue in both spaces
is supposed to be constant, and so that no absorption or evolu-
tion of heat occurs.

Let $A$ signify the heat-equivalent of the unit of work;
$v$ the initial velocity of the gas in the plane of the outlet;
$g$ the intensity of gravity on the sun;
$\kappa$ the relation of the specific heat of the gas with constant
pressure and constant volume;
$c$ the specific heat of the gas with constant volume reduced
to an equal weight of water;
$t_i$ the absolute temperature of the gas in the interior space
whence the gas issues;
Physical Constitution of the Sun. 315

t_a the absolute temperature of the issuing gas in the plane of the outlet;

\( p_i \) the pressure of the gas in the interior;

\( p_a \) the pressure in the plane of the outlet.

According to the mechanical theory of heat, and upon the above-mentioned hypotheses, the following relation holds good between these nine quantities*:

\[
\frac{v^2}{2g} = k \left( t_i - t_a \right), \quad \ldots \ldots \ldots (1)
\]

\[
\frac{t_i}{t_a} = \left( \frac{p_i}{p_a} \right)^{\frac{k-1}{k}} \quad \ldots \ldots \ldots (2)
\]

Further, let

\( a_i \) signify the mean height of the barometer in metres of mercury;

\( \rho \) the density of the gas under consideration at the temperature of melting ice, and under the pressure of the column \( a_i \) on the earth's surface;

\( \sigma \) the density of the gas in the interior space under the pressure \( p_i \) and at the absolute temperature \( t_i \);

\( \alpha \) the coefficient of expansion of the gas for 1° C.

According to Mariotte and Gay-Lussac's law we have, therefore, the following relation,

\[
\sigma = \frac{\rho}{a_i \alpha} \cdot \frac{p_i}{t_i} \quad \ldots \ldots \ldots (3)
\]

The pressure \( p_a \) in the plane of the outlet may, according to our assumptions, be considered to be equal to that exerted by the solar atmosphere at the level of the layer of separation, or at the lowest point of the atmosphere.

Let \( p_a \) signify the pressure at the lowest point of the atmosphere;

\( h \) a given height above this point;

\( p_h \) the pressure at this height;

\( t \) the absolute temperature of this atmosphere assumed to be constant throughout in the absence of knowledge of the laws of temperature;

\( g \) the gravity of the sun at the bottom of its atmosphere;

\( r \) the radius of the layer of separation;

\( \rho_1 \) the specific gravity of mercury at the temperature of melting ice;

\( g_1 \) the intensity of gravity at the earth's surface;

\( a_1 \) the mean height of the barometer;

\( \rho \) the density of the gas forming the atmosphere at the temperature of melting ice and under the action of \( g_1 \) and \( a_1 \).

* Zeuner, Grundzüge der mechanischen Wärmetheorie, 2te Aufl. 1866, p. 165.
We then have the following relation:

$$\log \frac{p_a}{p_h} = \frac{pgrh}{\rho_1 g_1 a_1 l(r + h)}.$$

In order to connect this equation with the three previous ones, a twofold hypothesis must be made:

1. That the chief constituent of the solar atmosphere exerting the pressure $p_a$ consists of the same gas which issues from the interior of the sun during the activity of the eruption.

2. That the absolute temperature $t$ of the atmosphere may be supposed mainly to correspond with the absolute temperature $t_a$ at the level of the point from which the gas issues.

I consider that the first of these assumptions is sufficiently proved by observation to allow of its use for the purposes of the present memoir, inasmuch as the discovery of the chromosphere has proved that the whole surface of the sun is surrounded by a very considerable atmosphere of hydrogen gas. The admissibility of the second assumption I deduce from the fact that in general the intensity of the light of the base of all the eruptive prominences is not essentially different from that of the chromosphere.

If it be remembered that the constant mean temperature $t$ in formula (4) (which, owing to our ignorance of the law of decrease of temperature, has been substituted for the temperatures sinking to the elevation $h$) * must nearly coincide with that of the lowest portions of the atmosphere, it will be seen that this temperature must nearly approach that of the outer surface of the dividing layer.

According to the first supposition, the value $\rho$ in the fourth formula will become identical with the corresponding value in formula (3); and according to the second supposition we have $t = t_a$.

§ 2.

Having in the foregoing stated the theoretical basis and the most essential hypotheses upon which the phenomena under consideration are to be treated, it will now be advisable to look to

* With reference to the increasing density of the air as the lowest layer is approached, the temperature expressed in formula (4) must, independently of the particular law of diminution of temperature, always agree with that of a layer lying deeper than $\frac{h}{2}$. This difference, which, as a simple calculation shows, is generally very considerable, appears to me to have been lost sight of in barometric measurements of heights where the mean temperature of both stations has been used; and this circumstance may suffice to explain simply many periodic phenomena which have been lately insisted on.
a simplification and alteration of the above formula rendering
them more suitable for the question under consideration.

If \( H \) represent the height to which a body possessing the
initial velocity \( v \) can be thrown up vertically from the sun's sur-
face, we have

\[
v^2 = 2gH \frac{r}{r + H}, \text{ or } \frac{v^2}{2g} = \frac{rH}{r + H}
\]

This value, substituted for \( \frac{v^2}{2g} \) in equation (1), gives

\[
t_i = \frac{rHA}{\kappa c(r + H)} + t_a;
\]

or when \( \frac{rHA}{\kappa c(r + H)} = a \) and, according to our hypothesis, \( t_a = t \), we
have for equation (1),

\[
t_i = a + t. \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (I)
\]

If, further, we place:

\[
\frac{\kappa - 1}{\kappa} = \frac{1}{q}, \quad \frac{p}{a_\alpha} = b, \quad \frac{g}{g_1\rho_1} = m,
\]

the equations (2), (3), and (4) will read as follows:

\[
\frac{t_i}{t} = \left( \frac{p_i}{p_a} \right)^{\frac{1}{q}}, \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (II)
\]

\[
\sigma = b \frac{p_i}{t_i}, \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (III)
\]

\[
p_a = p_h e^{b_m \frac{rh}{(r+h)t}} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (IV)
\]

By elimination the following equation is obtained:

\[
\sigma = \frac{bp_h}{a + t} \left( \frac{a + t}{t} \right)^q e^{b_m \frac{rh}{(r+h)t}} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (V)
\]

This equation therefore expresses the density \( \sigma \) of the com-
pressed gas as a function of the three values \( p_h, h, \) and \( t \). If,
therefore, three of these four magnitudes can be ascertained by
observation, or if certain limits can be assigned to their values,
the fourth can be determined. In fact it is possible, partly by
spectroscopic and partly by other means of observation, to deter-
mine certain limiting values for \( \sigma, p_h, \) and \( h \); so that a limit
also can be obtained for the value of \( t \)—that is, for the tempera-
ture of the outer atmosphere of hydrogen in the neighbourhood
of the glowing liquid layer of separation. This value, substituted
in equation (I.), then gives, when \( H \) is known, a value for the in-
ternal temperature \( t_i \); and in the same way the values of \( p_i \) and
\( p_a \) can be obtained from equations (III.) and (IV.).
§ 3.

I now begin the discussion of numerical values with formula (I). The lowest value which can be assigned to \( t \) is clearly 0. Hence we obtain for the internal temperature \( t_i \) the minimum value

\[
t_i = a = \frac{rHA}{\kappa c(r+H)} \quad \ldots \ldots \ldots \ldots \ldots \ldots \ldots \ldots \quad (5)
\]

Considering that the density of the solar atmosphere is reduced to an almost infinitely small quantity at a very moderate distance from the sun’s surface, and that the resistance thereby offered is very slight, we may for the sake of simplicity make the value of \( H \) equal to the mean height of the eruptive protuberances. A more exact discussion of the conditions under which this may be done will be given hereafter.

Protuberances are not unfrequently observed having an elevation of 3 minutes; but in order to be near the mark of a mean value, I will take \( H \) to be 1.5 minute.

The Heat-equivalent \( A \) I take to be equal to \( \frac{1}{424} \) expressed in metrical and centesimal units. The product \( \kappa c \) is taken to equal 3.409 for hydrogen, from the latest researches of Regnault.*

The value of \( \kappa \) for hydrogen is, according to Dulong, 1.411†.

A somewhat more detailed discussion is needed to obtain the numerical value of \( r \). According to the preceding, this signifies the radius of the layer of separation through which the eruptions break out. The question is, does this value correspond with the radius of the sun, or, in other words, is this radius identical with that of the solar disk as we see it, or not?

The latest researches of Frankland and Lockyer, St.-Claire Deville, and Wullner have shown that the discontinuous spectrum of hydrogen and other gases can be changed into a continuous bright one, the lines of the discontinuous spectrum undergoing a characteristic series of changes when the pressure is gradually increased, which consist essentially in the widening of the lines (as in the line \( H_b \)), and in a corresponding diminution of the distinctness of their boundaries. These changes render it possible to come to some conclusion respecting the amount of the pressure exerted at the given point; and Frankland and Lockyer have already drawn conclusions on this subject, since they say that “at the lower surface of the chromosphere itself the pressure is very far below the pressure of the earth’s atmosphere”‡.

From Wüllner's researches* we may, I believe, conclude that the pressure at the base of the chromosphere or at the outer edge of the luminous solar disk is equal to that of a column of mercury at the earth's surface from 50 millims. to 500 millims, in height†.

It is thus clear that it is not necessary, in order to explain the presence of the dark lines in the solar spectrum, to assume that the continuous spectrum is produced by the incandescence of a solid or liquid body; for we may with equal right consider that the continuous spectrum is produced by the glowing of a powerfully compressed gas.

This has indeed been experimentally proved by Wüllner for the sodium-lines, inasmuch as he remarks, "Under a pressure of 1230 millims. the maximum of light at Hα becomes less distinct, the whole spectrum is most dazzling, and the sodium-lines are seen as beautiful dark lines‡, so that the light of glowing hydrogen is intense enough to produce in an atmosphere of sodium vapour a Fraunhofer-line—a proof that the light of a glowing solid is not requisite for this purpose."

Hence it follows that the radius of the visible solar disk need not be necessarily identical with that of the supposed layer of separation, but that this latter may probably be assumed to lie below the point at which the hydrogen gas under compression evolves a continuous spectrum. The probability of these considerations is much increased by the phenomena of sun-spots. However different even now may be the theoretical speculations as to the nature of the spots, almost all observers agree in admitting that the umbra lies at a lower level than the surrounding parts§. The depth at which the umbra lies has been ascertained, parly by direct (De La Rue, Stewart, Loewy), partly by indirect observations (Faye), to be about 8" |

If, therefore, we consider the umbra the products of a local cooling floating on the surface of a glowing liquid like islands on a glowing ocean, and the penumbra to be condensation-clouds which surround these islands or cooler spaces at a certain

‡ In consequence of the high temperature, the sodium of the glass is volatilized. At 1000 millims. pressure the sodium-lines are seen bright (loc. cit. p. 345).
§ Spörer, on the contrary, says, "we regard the spots as cloud-like forms floating above the bright surface of the sun. The penumbra is nothing more than a collection of small spots, through the interstices of which the bright surface above which the spot is situated can be seen" (Pogg. Ann. vol. cxxviii. (1866), p. 270).
|| Faye, by calculations from Carrington's observations, finds that this depth corresponds to 0.005–0.009 of the sun's radius (Comptes Rendus, vol. lxi. pp. 1082–1090).
elevation*, it appears that the simplest supposition is that the liquid surface required by this theory of the sun-spots is identical with that out of which the protuberances burst forth. The radius of this surface \( r \) will be \( r = R - 8'' \) when \( R \) signifies the sun's radius expressed in seconds; or taking \( R \) at the mean distance of the sun to be 16', we have \( r = 15' 52'' \). According to Hansen, the mean solar parallax is 8''915; hence we have \( r = 680,930,000 \) metres, or \( 8'' = 5,722,500 \) metres. In order, therefore, to be able to obtain a numerical value for the minimum temperature in the space from which an eruption of the height of 1.5 minute breaks out, we have only to substitute the following values in equation (5):

\[
r = 680,930,000, \quad H = 64,370,000, \quad A = \frac{1}{\sqrt{2}}, \quad \kappa = 3.409.
\]

Hence we find \( t_i = 40690^\circ \).

If we give \( H \) double the above value, or suppose an eruption of 3 minutes (as not unfrequently occurs) to take place, we have a minimum temperature \( t_i = 74910^\circ \).

We may, however, now inquire whether we are justified in taking the extreme heights of observed protuberances as values for \( H \) in our formula, \( H \) signifying the height to which a body thrown out from the sun's surface would rise without resistance. If we have really to do with rising masses of glowing hydrogen, as is indeed sufficiently proved, this rise may take place, according to Archimedes's principles, like air heated and made specifically lighter than the neighbouring parts. It is, however, clear that the two conditions of motion will produce a very different effect as regards the time in which the moving masses will reach a given height. Without going more specially into these conditions, it is plain that the time which a protuberance needs in order to rise to a given elevation \( H \), by virtue of the principle of Archimedes, will under all circumstances be greater than that needed to rise to the same height \( H \) when moving under the influence of a certain initial velocity and encountering no resistance.

An exact observation of the length of time which a rising prominence takes to reach a given elevation will give a means of deciding whether or not the elevation is reached by the action of the first-mentioned cause; and unless this is proved to be likely, this elevation cannot be used as an integral part of the above formula.

According to the hypothesis which we have made, the outlet whence the protuberance emerges from the glowing layer of

* I have mentioned this theory, five years ago, in my 'Photometrical Researches,' p. 245, and also in the Vierteljahresschrift d. Astron. Ges. Jahrgang iv., II. 3. p. 172; and it is my intention to develop it in a special memoir.
liquid is situated at a depth of \( h = 8'' \) below the visible edge of the solar disk. \( H \) signifies the elevation of a protuberance reckoned from the plane of the outlet.

Let \( \tau \) signify the time which the prominence requires to rise from the outlet to the height \( H \);

\( \tau_1 \) the time which the protuberance needs to rise from the height \( h \) (that is, from the visible limit of the photosphere) to the height \( H \);

\( v \) the velocity at the outlet;

\( v_1 \) the velocity at the height \( h \).

Assuming the truth of the first hypothesis, and neglecting the diminution of the intensity of gravity (\( g \)), we obtain the following equations:

\[
\tau = \sqrt{\frac{2H}{g}}, \quad \tau_1 = \sqrt{\frac{2(H - h)}{g}},
\]

\[
v = \sqrt{2gH}, \quad v_1 = \sqrt{2g(H - h)}.
\]

If we now take

\( H = 64,370,000 \) metres, \( h = 5,722,600 \) metres, \( g = 274.3 \) metres

we have

\( \tau = 11 \) minutes 25 seconds,

\( \tau_1 = 10 \) minutes 54 seconds,

\( v = 187,900 \) metres = 25.32 German geog. miles,

\( v_1 = 179,400 \) metres = 27.17 German geog. miles.

If, therefore, we observe in a protuberance an ascensional velocity equal to the above, we are entitled to employ in our equation the elevation which is reached in the above times. I have often observed such a velocity in a protuberance, and have drawn a representation (Plate II.) of a protuberance in which the rate of ascension agrees well with the calculated velocities.

As regards the enormous initial velocities of these eruptive movements, Lockyer, by his beautiful observations of the alteration of the refrangibility of light, came by direct observations to numbers of the same order. During the short period during which these observations have been made, Lockyer* has ascertained that the maximum values of the rate of motion, horizontal and vertical, of a current of hydrogen in the chromosphere reach 40 and 120 miles per second. The above values expressed in English miles are

\( v = 123.1 \) miles, and \( v_1 = 117.7 \) miles;

and therefore they agree with Lockyer's observations.

According to the mechanical theory of heat, such velocities in the case of hydrogen necessitate a difference of temperature amounting to 40690° C. The temperatures themselves may be approximately determined if we can succeed in obtaining any limiting value for \( t \), the temperature of the outer atmosphere of hydrogen. This temperature, as has been already shown, may be taken to be nearly identical with that in the neighbourhood of the outlet.

§ 4.

A limiting value for \( t \) may be obtained from equation (V.),

\[
\sigma = \frac{bp_h}{a+i} \left( \frac{a+i}{t} \right)^{\frac{\sigma}{b_m}} \left( \frac{r_h}{(r+h)t} \right).
\]

In this the density \( \sigma \) of the enclosed mass of gas is expressed as a function of the three values \( p_h, h, \) and \( t \). I shall now show that the value of \( \sigma \) cannot exceed a certain limit; and thus the value of \( t \) is also ascertained within a given limit, inasmuch as limits to the values \( p_h \) and \( h \) have been already determined. It has been already pointed out that the explanation of the eruptive protuberances presupposes the existence of a layer of separation, dividing the space out of which the eruptions break forth from that into which they empty themselves. It is only by the existence of such a division that the required difference in pressure is rendered possible.

Respecting the physical constitution of this layer, the further assumption is necessary that it is in some other state than the gaseous. It may be either solid or liquid. In consequence of the high temperature the solid state is excluded; and we must therefore conclude that the layer of division consists of an incandescent liquid.

Respecting the mass of hydrogen enclosed by this liquid layer, two suppositions appear at first sight possible:

1. The whole interior of the sun is filled with glowing hydrogen, and our luminary would appear like a great bubble of hydrogen surrounded by an incandescent atmosphere.

2. The masses of hydrogen which are thrown out in these volcanic outbursts are local aggregations contained in hollow spaces formed near the surface of an incandescent liquid mass, and these burst through their outer shell when the increased pressure of the material in the interior reaches a certain point.

According to the first assumption, a state of stable equilibrium will only occur when the specific gravity of the liquid dividing layer is smaller than that of the gaseous layer which lies immediately underneath it. As, however, the density of a gaseous globe, whose particles obey the laws of Newton and Mariotte,
increases from the surface towards the centre, the specific gravity of the layer of division must necessarily be smaller than that of the mean specific gravity of the sun. If we assume that the highest limit of specific gravity of this layer is the mean specific gravity of the sun, we shall have to assume that all the deeper-lying layers, and therefore the still deeper-lying gaseous layer, have the same temperature. But then the interior of the sun would not consist of a gas, but of an incompressible liquid. All these deductions are, it will be seen, necessary consequences of the supposition that the specific gravity $\sigma$ of the compressed gases issuing in these eruptive discharges reaches its maximum limit, viz. that of the mean specific gravity of the sun.

In this case, however, the first supposition changes into the second, according to which the sun consists of an incompressible liquid, near the surface of which local aggregations of masses of glowing hydrogen occur; and these burst forth when a sufficient difference of pressure presents itself, giving rise to the phenomena of eruptive protuberances.

However small these spaces may in certain cases be supposed to be, the specific gravity of the enclosed mass of gas cannot be taken to be greater than that of the surrounding liquid; otherwise the compressed gas would sink into the interior of the sun.

The specific gravity of the sun is, according to the newest results, $1.46$. If we insert this value for $\sigma$ in equation (V.), and for $a$ the number $40690$, and for $h$ the value $8^\prime$ in metres, we obtain the following limiting values for $p_h=0.500$ metre and $p_h=0.050$ metre:

for $p_h=0.500$, $t=29500^\circ$; and for $p_h=0.050$, $t=26000^\circ$

or a mean value of $t=27700^\circ$ as the absolute temperature of the solar atmosphere.

If equation (5) be differentiated according to $t$, the differential quotient $\frac{d\sigma}{dt}$ becomes negative—that is, $\sigma$ diminishes for increasing values of $t$. Hence it follows that the above values of $t$ are minimum values.

With the mean value of $t$ for the temperature of the sun's atmosphere the value for $p_h$ of $0.180$ metre is obtained. These values are used in the following calculations.

It may be remarked that the high values obtained for these temperatures are about eight times that given by Bunsen* for that of the oxyhydrogen-flame, and that iron must exist in the solar atmosphere in a permanently gaseous state.

From formula (I.) the value of the internal temperature $t_i$ is found, when $t=27700^\circ$, to be $t_i=68400^\circ$. If we substitute

these values in equation (II.), we get \( \frac{p_i}{p_a} = 22.1 \); that is, the
pressure in the interior of the space from which the protuberances issue is 22.1 times as great as the pressure on the surface of the liquid dividing-layer. If we next substitute the value of \( t \) in formula (IV.), and take, as before, the value of \( h \) to be 8", we have \( \frac{p_a}{p_h} \) 766000 for the relation between the pressure at the
liquid surface of the sun to the pressure at the elevation \( h \), where
the hydrostatic spectrum begins to be continuous owing to the in-
crease of pressure. If we substitute for \( p_h \) the above value of
0.180 metre of mercury, we have for \( p_a = 184000 \) atmospheres,
and for \( p_i = 4,070,000 \) atmospheres.

If we next calculate the depth in the liquid body of the sun
having a specific gravity of 1.46, at which, in consequence of the
hydrostatic pressure, the maximum pressure of \( p_i \) is attained, we
find that this is reached at a depth of 139 geographical miles
from the surface—that is, at a depth of 1.46 second, or one 658th
part of the sun's semidiameter. Even if, not considering the
liquid condition, and under the assumption of a much greater
thickness of the atmosphere of hydrogen, we calculate the depth
at which the atmospheric pressure becomes equal to the internal
pressure \( p_i \), it appears that when the temperature is as high as
68400° the point is reached at a depth of only 27" under the
visible surface of the sun, or at about \( \frac{1}{35} \) of its visible semi-
diameter.

This shows how rapidly the pressure must increase towards
the interior of the sun's body; and it renders plausible the sup-
position that in the interior of the sun, in spite of the high tem-
peratures which there exist, the permanent gases such as hydro-
gen can only exist in the condition of glowing liquids.

\section*{§ 5.}

A very singular result is obtained if we assume an atmosphere
of nitrogen or oxygen of the same weight and temperature as
was the case with the hydrogen atmosphere which we have just
considered, and then calculate the pressure which will occur in
that atmosphere at the elevation at which the hydrogen spectrum
begins to be continuous. If we suppose that the pressure on
these three atmospheres of hydrogen, nitrogen, and oxygen is
the same at a depth of 8" below the visible edge of the sun's
disk or at the level of the supposed layer of division, and that
this common pressure is \( p_a = 184000 \) atmospheres, which, ac-
cording to the foregoing, corresponds to the assumed value of \( p_h \),
we obtain, when the temperature \( t = 27700° \), for the pressure of
these three atmospheres on the visible surface of the sun, the following values:

\[
\begin{align*}
\text{Hydrogen} & \quad p_h = 180 \text{ millims.}, \\
\text{Nitrogen} & \quad p_n = 323 \frac{1}{10^{78}} \text{ millims.}, \\
\text{Oxygen} & \quad p_o = 124 \frac{1}{10^{38}} \\
\end{align*}
\]

Hence it follows that, under the above conditions, the quantities of the two latter gases at the point in question, where the hydrogen spectrum becomes continuous, may be considered perfectly inappreciable in comparison with the quantity of hydrogen. This would still be the case if we were to assume that the weight of each of the two atmospheres is many million times as great; although, considering that nitrogen is fourteen times and oxygen sixteen times as heavy as hydrogen, it would only take \(\frac{1}{14}\) and \(\frac{1}{16}\) the quantities in order to make the pressure on these atmospheres at the lowest level agree with that on the hydrogen. The maximum value of the pressure at the lowest point of these atmospheres must, in accordance with our previous hypothesis, here be assumed to be equal to the mean density of the sun; and it is easy by means of formula (III.) and the known specific gravity of oxygen and nitrogen to calculate how great the weight of these atmospheres must be in order to reach this maximum of density. It is then found that the weight of the oxygen atmosphere is only 0.56, and that of nitrogen 0.64, when that of the hydrogen atmosphere is taken as the unit.

If, therefore, we assume the presence of all these three gases on the sun's surface, and if the influence of atmospheric motions remain unconsidered, we shall find that the rays of light reaching our eyes from the layer of hydrogen yielding a continuous spectrum, had on their way to pass through so small a number of glowing particles of nitrogen and oxygen that the absorption thereby produced is infinitely small, and that therefore the presence of oxygen and nitrogen in the solar spectrum cannot be ascertained, as we know is the case, by the appearance of dark lines corresponding to these elements.

Although the motion of gases will act in the direction of diminishing these great differences, still the existence of the chromosphere shows the small effect of this motion, in consequence of the great intensity of gravitation and the enormous elevation of the gaseous mass (see formula 4).

In order to explain by means of the above relation the absence in the solar spectrum of the lines of two elements so widely distributed on the earth as oxygen and nitrogen, we must bear in
mind the very small power of emission which permanent gases exhibit compared with that of vaporized solid bodies. If we consider the power of emission of very small quantities of different gases at the same temperature for rays of the same refrangibility*, we find that the above-mentioned experiment of Wüllner affords a good illustration of the extraordinary difference in the power of emission, and therefore, according to Kirchhoff's law, the power of absorption of different gases at the same temperature. In this experiment the small quantity of the sodium-vapour which is volatilized in the Geissler's tube gives out more light than hydrogen compressed under a pressure of 1000 millims. of mercury. It is only by bearing in mind this circumstance that we can explain why, if on this ground the lines of nitrogen and oxygen are absent, we yet see those of other elements whose atomic weights and therefore densities are much greater than those of oxygen and nitrogen. From these considerations the following conclusions may either be immediately drawn, or may be shown to follow by a train of reasoning which I propose more fully to develope elsewhere.

1. From the absence of certain lines in the spectrum of a self-luminous star the absence of the corresponding elementary body cannot be affirmed.

2. The absorbent layer in which the reversal of the spectrum occurs is different for each element. This point lies nearer to the centre of the star the greater the density and the smaller the power of emission of the element in question.

3. In the case of different stars this layer lies, under otherwise similar circumstances, nearer to the centre the greater the intensity of gravitation is.

4. The separation of the layers of reversion of the several elements, both from one another and from the centre of the star, increase with rise of temperature.

5. The spectra of different stars are, under otherwise similar circumstances, richer in lines the lower the temperature is and the greater the mass of the star.

6. The great difference in intensity exhibited in the lines of the solar spectrum and the spectra of other fixed stars not only depends upon the differences in the power of absorption, but also upon the different depths of the layers in which the reversal of the several spectra occurs.

In conclusion, I may be allowed to make some remarks on the application to the heavenly bodies of the results of experiments made upon rarefied gases. It has lately been suggested by Lecoq

* A perfect transparency of the mass of gas for the emitted ray is here assumed—an assumption which more nearly approaches the truth as the masses of the gases under comparison become smaller.
de Boisbaudran*, with regard to Wühlner's experiments on the variation of the spectra with pressure and temperature, that these results must be applied only with the greatest care to the phenomena of the pressure-variations of the solar atmosphere, inasmuch as these alterations in the spectra are influenced far more by changes of temperature than by changes of pressure. Even if this suggestion should be established by future experiments, the results developed in the foregoing memoir would only be very slightly affected; for the constitution of the function (formula V.) which has served us for the determination of the temperature of the atmosphere is such that the pressure \( p_h \), at which the hydrogen spectrum becomes continuous, may be altered within very wide limits without necessitating a corresponding alteration in the temperature. Thus, if we employ the two extremes of the assumed pressures 1 : 10, we find that the corresponding values for the temperatures stand in the ratio of 1 to 1.15 only.

Nevertheless it must be admitted that the exact elimination of the influences which pressure and temperature exert on the constitution of the spectrum is to be regarded as a problem the solution of which is of the greatest importance for Stellar Physics.

Perhaps by the application of the well-known laws of the heat of the current and of Gay-Lussac's law, it may prove possible so to regulate the pressure of the gas by alteration of the level of the mercurial column, that the increase of pressure produced by the elevation of temperature of a powerful discharge may be compensated for by a reduction of pressure before the discharge. In this way the pressure could be kept constant, and without knowing the temperature itself the effects of the changes of temperature on the spectra could be readily observed. In this case the loss of heat by conduction and radiation experienced during the short period of the discharge is neglected, and the quantity of heat evolved in the circuit is considered to be approximately proportional to the temperature of the incandescent gaseous mass. If this mass is known, it would be possible to calculate a superior limit for the absolute temperature of the incandescent gas, provided the duration of the discharge and the quantity of heat developed during such discharge were determined.

XLII. On a Salt that is invisible in its Mother-liquor.
By Charles Tomlinson, F.R.S.*

Many years ago Sir David Brewster devised a simple but accurate method for determining the refractive power of solid fragments without the trouble of grinding and polishing them. For this purpose a broken chip of the solid, so irregular that no object could be seen through it, was put into a fluid of the same refractive power, in which case the incident rays would suffer no refraction in passing from the fluid into the solid or from the solid into the fluid; consequently objects could be seen distinctly through the broken irregular chip. Thus a bit of crown glass, of very irregular shape, so as to appear almost opaque, became nearly invisible when put into Canada balsam, and so transparent that a printed page could be easily read through it. By mixing fluids of different refractive powers it is not difficult to obtain a compound of the same refractive density as that of the solid we wish to test. Oil of cassia mixed with oil of olives in different proportions may be used for determining the refractive powers of all solids from 5.077 (that of oil of cassia) to 3.113 (that of oil of olives).

I am not aware whether this valuable suggestion has ever been adopted by persons who deal in or work up precious stones. If a rough topaz or other rough stone be put into Canada balsam, oil of sassafras, or other fluid of nearly the same refractive density, and be turned round so that the rays of light may pass through in every direction, the slightest flaws or cracks are readily detected. Even when the refractive density of the stone exceeds that of any fluid, as in the case of diamond, jasper, spinelle, ruby, and some others, yet by immersing them in oil of cassia or terchloride of antimony flaws and imperfections not visible and not suspected start into view. Even when examined in water, flaws are more perceptible than when seen in air. By this method also precious stones may be distinguished from pastes.

I do not remember any case recorded by chemists in which a salt has the same refractive density as the liquor in which it is formed, and is consequently invisible in it. Such a case occurred to me last winter while examining the action of low temperatures on supersaturated solutions, chiefly of double salts. The zinc and sodic sulphates were mixed in atomic proportions, dissolved in a very small quantity of water, only just enough to prevent the anhydrous salt from being thrown down during the boiling; the boiling solution was filtered into clean test-tubes, and was protected from the action of nuclei by plugging the tubes with

* Communicated by the Author, having been read before the Chemical Section of the British Association at Liverpool, September 20, 1870.
cotton-wool. When cold the tubes were put into a freezing-mixture at 10° F., and afterwards into one at 0° F., apparently without any effect. The tubes were set aside with the cotton-wool undisturbed, and they remained at rest during a week. On again examining them the cotton-wool was removed; but there was no sign of crystallization until, on closing one of the tubes with the thumb and inverting it, a large mass of crystals became visible in consequence of the draining off of the mother-liquor, now only a saturated solution. Air entered some cavities in the crystals, and on turning back the tube so as to allow the mother-liquor once more to envelope the crystals, these air-filled cavities stood out with most perfect definition, while the crystals themselves again became invisible. This experiment very much impressed me with the value of Sir David Brewster’s suggestions; and I cannot fancy a better test in the hands of an intelligent lapidary for detecting flaws and cavities in precious stones before deciding on their value or proceeding to cut and polish them.

On repeating the experiment with the double salt, I found that at about 0° F. the solution became solid, but so transparent that no casual observer would suspect it to be so. One of the tubes that was more than three-fourths full had a few scattered needles at the surface, showing that crystallization had set in. On passing down a platinum spatula the solution was found to be pulpy, so much so that the tube could be inverted without any loss of liquid. By repose the pulpy salt became crystalline, and the mother-liquor, of the same refractive density, separated.

This sodium-zincic sulphate, as obtained in an open evaporating-dish, contains only four equivalents of water. In a closed tube, if left to repose during some weeks, it assumes a different state of hydration; and as it does so it acquires a different index of refraction as compared with that of the mother-liquor, and so becomes visible.

Highgate, N.,
September 8, 1870.

XLIII. On the Geodesic Lines on an Oblate Spheroid.
By Professor Cayley, F.R.S.*

THE theory of the geodesic lines on an oblate spheroid of any excentricity whatever was investigated by Legendre†; and the general course of them is well known, viz. each geodesic line undulates between two parallels equidistant from the equator (being thus either a closed curve, or a curve of indefinite length,

* Communicated by the Author.
Prof. Cayley on the Geodesic Lines

according to the distance between the two parallels): at a point of contact with the parallel, the curve is, of course, at right angles to the meridian; say this is \( V \), a vertex of the geodesic line, and let the meridian through \( V \) meet the equator in \( A \); the geodesic line proceeds from \( V \) to meet the equator in a point \( N \), the node, where \( AN \) is at most \( = 90^\circ \); and the undulations are obtained by the repetition of this portion \( VN \) of the geodesic line alternately on each side of the equator and of the meridian.

I consider in the present paper the series of geodesic lines which cut at right angles a given meridian \( AC \), or, say, a series of geodesic normals. It may be remarked that as \( V \) passes from the position \( A \) on the equator to the pole \( C \), the angular distance \( AN \) increases from a certain determinate value (equal, as will appear, to \( \frac{C}{A} 90^\circ \), if \( C, A \) are the polar and equatorial axes respectively) up to the value \( 90^\circ \); and it thus appears that, attending only to their course after they first meet the equator, the geodesic normals have an envelope resembling in its general appearance the evolute of an ellipse (see fig. 1 and also fig. 2),

the centre hereof being the point \( B \) at the distance \( BA = 90^\circ \), and the axes coinciding in direction with the equator \( BA \) and meridian \( BC \): this is in fact a real geodesic evolute of the meridian \( CA \). The point \( \alpha \) is, it is clear, the intersection of the equator by the geodesic line for which \( V \) is consecutive to the point \( A \) (so that \( \angle B OA = (1 - \frac{C}{A})90^\circ \); and the point \( \gamma \) is the in
tersection of the meridian C B by the geodesic line for which V is consecutive to the point C; and its position will be in this way presently determined. I was anxious, with a view to the con-
struction of a drawing and a model, to obtain some numerical results in relation to a spheroid of considerable excentricity, and I selected that for which \( \frac{C}{A} = \frac{1}{2} \) (polar axis = \( \frac{1}{2} \) equatorial).

Before proceeding further, I remark that Legendre's expression "reduced latitude" is used in what is not, I think, the ordinary sense; and I propose to substitute the term "parametric latitude:" viz., in figure 3, referring the point P on the ellipse by means of the ordinate M P Q to a point Q on the circle, radius O K (= O A, fig. 1), and drawing the normal P T, then we have for the point P the three latitudes,

\[
\lambda = \angle P T K, \quad \text{normal latitude,}
\]

\[
\lambda'' = \angle P O K, \quad \text{central latitude,}
\]

\[
\nu = \angle Q O K, \quad \text{parametric latitude;}
\]

viz. \( \nu \) is the parameter most convenient for the expression of the values of the coordinates \( x, y \) \( (x = A \cos \nu, \ y = C \sin \nu) \) of a point P on the ellipse. The relations between the three latitudes are

\[
\tan \lambda'' = \frac{C}{A} \tan \nu = \frac{C^2}{A^2} \tan \lambda,
\]

so that \( \lambda'', \nu, \lambda \) are in the order of increasing magnitude. I use in like manner \( l, l', l'' \) in regard to the vertex V. The course of a geodesic line is determined by the equation

\[
\cos \lambda' \sin \alpha = \text{const.,}
\]

where \( \lambda' \) is the reduced latitude of any point P on the geodesic line, and \( \alpha \) is at this point the azimuth of the geodesic line, or its inclination to the meridian. Hence, if \( l' \) be the parametric latitude of the vertex V, the equation is

\[
\cos \lambda' \sin \alpha = \cos l'
\]

(whence also, when \( \lambda' = 0, \alpha = 90^\circ - l' \); that is, the geodesic line cuts the equator at an angle \( = l' \), the parametric latitude of the vertex). The equation in question, \( \cos \lambda' \sin \alpha = \cos l' \), leads at once to Legendre's other equations: viz. taking, as above, A, C for the equatorial and polar semiaxes respectively, and \( \delta \) for the excentricity, \( \delta = \sqrt{1 - \frac{C^2}{A^2}} \); and to determine the position of P.
on the meridian, using (instead of the parametric latitude $\lambda'$) the angle $\phi$ determined by the equation

$$\cos \phi = \frac{\sin \lambda'}{\sin \beta},$$

and writing, moreover, $s$ to denote the geodesic distance $VP$, and $\Lambda$ to denote the longitude of $P$ measured from the meridian $CA$ which passes through the vertex $V$, these are

$$ds = d\phi \sqrt{C^2 + A^2 \delta^2 \sin^2 \beta' \cos^2 \phi},$$

$$d\Lambda = \frac{\cos \beta' \, d\phi \sqrt{C^2 + A^2 \delta^2 \sin^2 \beta' \cos^2 \phi}}{1 - \sin^2 \beta' \cos^2 \phi};$$

which differential expressions are to be integrated from $\phi = 0$; and the equations then determine $\lambda'$, $s$, and $\Lambda$, all in terms of the angle $\phi$, that is, virtually $s$ and $\Lambda$, the length and longitude, in terms of the parametric latitude $\lambda'$.

Writing, with Legendre,

$$c^2 = \frac{A^2 \delta^2 \sin^2 \beta'}{C^2 + A^2 \delta^2 \sin^2 \beta'}, \quad = \delta^2 \sin^2 \beta,$$

$$b^2 = 1 - c^2, \quad = \frac{C^2}{C^2 + A^2 \delta^2 \sin^2 \beta'}, \quad = 1 - \delta^2 \sin^2 \beta;$$

also

$$n = \tan^2 \beta', \quad M = \frac{C}{b \Lambda \cos \beta'} = \frac{C}{\Lambda \cos \beta'},$$

then the formulæ become

$$ds = \frac{C}{b} \, d\phi \sqrt{1 - c^2 \sin^2 \phi},$$

$$d\Lambda = M \frac{d\phi \sqrt{1 - c^2 \sin^2 \phi}}{1 + n \sin^2 \phi}.$$

Hence integrating from $\phi = 0$, and using the notations $F, E, \Pi$ of elliptic functions, we have

$$s = \frac{C}{b} \, E(c, \phi),$$

$$\Lambda = \frac{M}{n} \left\{ (n + c^2) \Pi(n, c, \phi) - c^2 F(c, \phi) \right\};$$

viz. these belong to any point $P$ whatever on the geodesic line, parametric latitude of vertex $= \beta'$; and if we write herein $\phi = 90^\circ$, then they will refer to the node $N$, or point of intersection with the equator.
The position of the point $\alpha$ is at once obtained by writing $l' = 0$: viz. this gives $c = 0$, $b = 1$, $M = \frac{C}{A}$, $n = 0$: the differential expressions are $ds = C d\phi$, $d\Lambda = \frac{C}{A} \frac{\pi}{2} d\phi$. Or integrating from $\phi = 0$ to $\phi = \frac{\pi}{2}$, we have $s = \Lambda \cdot \frac{C}{A} \frac{\pi}{2}$, $\Lambda = \frac{C}{A} \cdot \frac{\pi}{2}$, agreeing with each other, and giving longitude of $\alpha = \frac{C}{A} \cdot \frac{\pi}{2}$; or, what is the same thing, $\angle \alpha OB = \left(1 - \frac{C}{A}\right) \frac{\pi}{2}$.

Writing in the formulæ $l' = 90^\circ$, we have $c = \delta$, $b = \frac{C}{A}$, $\frac{M}{n} = 0$; whence $d\Lambda = 0$, or $\Lambda = \text{const.} = \frac{\pi}{2}$, since the geodesic line here coincides with the meridian $C B$; and moreover $s = \Lambda \cdot \frac{C}{A} \frac{\pi}{2}$, $\Lambda = \frac{C}{A} \cdot \frac{\pi}{2}$, giving longitude of $\alpha = \frac{C}{A} \cdot \frac{\pi}{2}$; or, what is the same thing, $\angle \alpha OB = \left(1 - \frac{C}{A}\right) \frac{\pi}{2}$.

To find it we must consider a position of $V$ consecutive to $C$, say, $l' = \frac{\pi}{2} - \epsilon$, where $\epsilon$ is indefinitely small; $n$ is thus indefinitely large, and the integral $\Pi(n, c, \phi)$ is not conveniently dealt with. But it may be replaced by an expression depending on $\Pi\left(\frac{c^2}{n}, c, \phi\right)$, where $\frac{c^2}{n}$ is indefinitely small; viz. (Legendre, *Fonct. Ellip.* vol. i. p. 69) we have

$$\Pi(n, c, \phi) = F(c, \phi) + \frac{1}{\sqrt{\alpha}} \tan^{-1} \frac{\sqrt{\alpha} \tan \phi}{\sqrt{1 - c^2 \sin^2 \phi}} - \Pi\left(\frac{c^2}{n}, c, \phi\right)$$

where

$$\alpha = (1 + n) \left(1 + \frac{c^2}{n}\right).$$

We thus have

$$\Lambda = \frac{M}{n} \left\{ nF(c, \phi) + \frac{\sqrt{n} \sqrt{c^2 + n}}{\sqrt{1 + n}} \tan^{-1} \frac{\sqrt{\alpha} \tan \phi}{\sqrt{1 - c^2 \sin^2 \phi}} - (c^2 + n) \Pi\left(\frac{c^2}{n}, c, \phi\right) \right\},$$

where, $\frac{c^2}{n}$ being small,
\[ \Pi \left( \frac{c^2}{n}, c, \phi \right) = \int \frac{d\phi}{ \left( 1 + \frac{c^2}{n} \sin^2 \phi \right) \sqrt{1 - c^2 \sin^2 \phi} } \]

\[ = \frac{\left( 1 - \frac{c^2}{n} \sin^2 \phi \right) d\phi}{\sqrt{1 - c^2 \sin^2 \phi}} = \left( 1 - \frac{1}{n} \right) F(c, \phi) + \frac{1}{n} E(c, \phi). \]

And expanding also the $\tan^{-1}$ term, we thus have

\[ \Lambda = \frac{M}{n} \left\{ nF(c, \phi) + \frac{\sqrt{n} \sqrt{c^2 + n} \left[ \frac{\pi}{2} - \sqrt{1 - c^2 \sin^2 \phi} \right]}{\tan \phi \sqrt{(1+n)(c^2+n)}} \right\} \]

\[ - (c^2 + n) \left\{ \left( 1 - \frac{1}{n} \right) F(c, \phi) + \frac{1}{n} E(c, \phi) \right\} \]

\[ = \frac{M}{n} \left\{ (b^2 + \frac{c^2}{n}) F(c, \phi) - \left( 1 + \frac{c^2}{n} \right) E(c, \phi) \right\} \]

\[ + \frac{\sqrt{n} \sqrt{c^2 + n} \pi}{2} \frac{n}{n+1} \cot \phi \sqrt{1 - c^2 \sin^2 \phi} \right\}, \]

which, in the term in \{\} neglecting negative powers of \( n \), becomes

\[ \Lambda = \frac{M}{n} \left\{ \sqrt{\frac{\pi}{2}} + b^2 F(c, \phi) - E(c, \phi) - \cot \phi \sqrt{1 - c^2 \sin^2 \phi} \right\}. \]

We may moreover write \( c = \delta, b = \frac{C}{A}, \phi = 90^\circ - \lambda', n = \frac{1}{\epsilon^2}, M = \epsilon, \)

and therefore \( \frac{M}{n} = \epsilon \), so that the formula is

\[ \Lambda = \epsilon \left\{ \frac{\pi}{2} - \epsilon \left\{ \tan \lambda' \sqrt{1 - c^2 \cos^2 \lambda'} + E(c, 90^\circ - \lambda') - b^2 F(c, 90^\circ - \lambda') \right\} \}

\[ = \frac{\pi}{2} - \epsilon \left\{ \tan \lambda' \sqrt{1 - c^2 \cos^2 \lambda'} + E(c, 90^\circ - \lambda') - b^2 F(c, 90^\circ - \lambda') \right\}, \]

where I retain \( c, b \) as standing for \( \sqrt{1 - \frac{C^2}{A^2}} \) respectively.

Writing herein \( \lambda' = 0 \), we have

\[ \Lambda = \frac{\pi}{2} - \epsilon \left( E(c) - b^2 F(c) \right), \]

where the coefficient \( E(c) - b^2 F(c) \) is

\[ = \int_0^{\frac{\pi}{2}} d\theta \left( \sqrt{1 - c^2 \sin^2 \theta} - \frac{1 - c^2}{\sqrt{1 - c^2 \sin^2 \theta}} \right) \]

\[ = c^2 \int_0^{\frac{\pi}{2}} \frac{\cos^2 \theta d\theta}{\sqrt{1 - c^2 \sin^2 \theta}} \]
consequently positive; that is, $\Lambda$, the longitude of the node, is less than $90^\circ$, as it should be. Hence in order that $\Lambda$ may be $=90^\circ$, we must have $\lambda'$ negative, say, $\lambda' = -\mu'$, where $\mu'$ is positive; and, observing that we may under the signs $E, F$ write $90^\circ - \mu'$ instead of $90^\circ + \mu'$, we thus have

$$\frac{\pi}{2} = \frac{\pi}{2} + \varepsilon \left\{ \sqrt{1 - c^2 \cos^2 \mu'} \tan \mu' - E(c, 90^\circ - \mu') + b^2 F(c, 90^\circ - \mu') \right\};$$

that is, we must have

$$\tan \mu' \sqrt{1 - c^2 \cos^2 \mu'} = E(c, 90^\circ - \mu') - b^2 F(c, 90^\circ - \mu');$$

viz. $\mu'$ is here the parametric latitude (south) of the intersection of the meridian $CB$ with the consecutive geodesic line—that is, of the point $\gamma$. As $\mu'$ increases from 0 to $90^\circ$, the left-hand side increases from 0 to $\infty$; and the right-hand side, beginning from a positive value and either attaining a maximum or not, ultimately decreases to 0; there is consequently a real root, which is easily found by trial.

Thus $\frac{C}{\Lambda} = \frac{1}{2}, \ C = \frac{1}{2} \sqrt{3}$ (the angle of modulus $= 60^\circ$), $b = \frac{1}{3}$; or the equation is

$$\tan \mu' \sqrt{1 - \frac{3}{4} \cos^2 \mu'} = E(90^\circ - \mu') - \frac{1}{4} F(90^\circ - \mu').$$

Using Legendre’s Table IX., we have

<table>
<thead>
<tr>
<th>$\mu'$</th>
<th>$90^\circ - \mu'$</th>
<th>$E$</th>
<th>$F$</th>
<th>$E - \frac{1}{4} F$</th>
<th>$\tan \mu' \sqrt{1 - \frac{3}{4} \cos^2 \mu'}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$0$</td>
<td>$90^\circ$</td>
<td>1\cdot21105</td>
<td>2\cdot15651</td>
<td>$-6719$</td>
<td>$0$</td>
</tr>
<tr>
<td>$10$</td>
<td>$80^\circ$</td>
<td>1\cdot12248</td>
<td>1\cdot81252</td>
<td>$-6693$</td>
<td>$6530$</td>
</tr>
<tr>
<td>$20$</td>
<td>$70^\circ$</td>
<td>1\cdot02663</td>
<td>1\cdot49441</td>
<td>$-6165$</td>
<td>$3819$</td>
</tr>
<tr>
<td>$30$</td>
<td>$60^\circ$</td>
<td>$-91839$</td>
<td>1\cdot21253</td>
<td>$-6153$</td>
<td>$5542$</td>
</tr>
<tr>
<td>$40$</td>
<td>$50^\circ$</td>
<td>$-79538$</td>
<td>$-96465$</td>
<td>$-6278$</td>
<td>$6278$</td>
</tr>
</tbody>
</table>

so that we see the required value is between $30^\circ$ and $40^\circ$; and a rough interpolation gives the value $\mu' = 37^\circ 40'$. But repeating the calculation with the values $37^\circ$ and $38^\circ$, we have

<table>
<thead>
<tr>
<th>$\mu'$</th>
<th>$90^\circ - \mu'$</th>
<th>$E$</th>
<th>$F$</th>
<th>$E - \frac{1}{4} F$</th>
<th>$\tan \mu' \sqrt{1 - \frac{3}{4} \cos^2 \mu'}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$37^\circ$</td>
<td>$53^\circ$</td>
<td>$-833879$</td>
<td>1\cdot035870</td>
<td>$-57419$</td>
<td>$54425$</td>
</tr>
<tr>
<td>$38^\circ$</td>
<td>$52^\circ$</td>
<td>$-821197$</td>
<td>1\cdot011849</td>
<td>$-56823$</td>
<td>$57108$</td>
</tr>
</tbody>
</table>

whence, interpolating, $\mu' = 37^\circ 55'$. The semiaxes of the geodesic evolute, measured according to their longitude and parametric latitude respectively, are thus $Bx,$
long. of \( \alpha = 45° \); By, param. lat. = 37° 55′. But measuring them according to their geodesic distance, the equatorial radius \( A \) being taken = 1, we have

\[
Bx = \frac{\pi}{4} \quad = 0.78540,
\]

\[
B\gamma = \left( \frac{C}{b} = 1 \right) \{ E_1 - E(52° 5') \} = 1.21106 - 0.82225 = 0.38881.
\]

Reverting to the general formulæ for \( s, \Lambda \), but writing therein \( \Lambda = 1 \), and therefore \( C = \sqrt{1 - \delta^2} \); writing also \( \phi = 90° \) (that is, making the formulæ to refer to the node \( N \) of the geodesic line), we have

\[
s = \frac{\sqrt{1 - \delta^2}}{\sqrt{1 - \frac{\delta^2}{\cos^2 l}}} \frac{E_1}{c},
\]

\[
\Lambda = \frac{\sqrt{1 - \delta^2}}{n \cos l} \left\{ (n + c^2) \Pi_1(n, c) - c^2 F_1 c \right\};
\]

but for the calculation of the second of these formulæ by means of Legendre's Tables it is necessary to express \( \Pi_1(n, c) \) in terms of the functions \( E, F \).

The proper formula is given in \textit{Fonct. Ellipt.}, vol. i. p. 137; viz. this is

\[
\frac{\Delta(b, \theta)}{\sin \theta \cos \theta} \Pi_1(n, c) = \frac{1}{2} \pi + \frac{\sin \theta}{\cos \theta} \Delta(b, \theta) \frac{E_1}{c} + F_1 c F(b, \theta),
\]

where \( \Delta(b, \theta) = \sqrt{1 - b^2 \sin^2 \theta} \). \( \theta \) is an angle given by the equation \( \cot \theta = \sqrt{n} \); we have \( n = \tan^2 l' \); consequently \( \theta = 90° - l' \). Substituting this value, except that for shortness I retain \( E(b, \theta), F(b, \theta) \) in place of \( E(b, 90° - l'), F(b, 90° - l') \), we have

\[
\Delta(b, \theta) = \sqrt{1 - b^2 \cos^2 l}' = \sqrt{1 - (1 - \frac{\delta^2}{\sin^2 l}) \cos^2 l}' = \sin l;
\]

and thence

\[
\tan \theta \Delta(b, \theta) = \cot l \sin l = \frac{\cos l}{\sqrt{1 - \delta^2}};
\]

whence

\[
\Pi_1(n, c) = \frac{\sin l' \cos l'}{\sin l} \left\{ \frac{1}{2} \pi + F_1 c \left[ \frac{\cos l}{\sqrt{1 - \delta^2}} + F(b, \theta) - E(b, \theta) \right] - E_1 c F(b, \theta) \right\}.
\]

But

\[
(n + c^2) \Pi_1(n, c) - c^2 F_1 c = \sin^2 l \left\{ \sec^2 l' \Pi_1(n, c) - \delta^2 F_1 c \right\};
\]

Hence

\[
(n + c^2) \Pi_1(n, c) - c^2 F_1 c = \sin^2 l \left\{ \sec^2 l' \Pi_1(n, c) - \delta^2 F_1 c \right\};
\]
and multiplying this by

$$\frac{\sqrt{1-\delta^2}}{n \cos \theta} = \frac{\sqrt{1-\delta^2} \cos \theta}{\tan^2 \theta \cos^2 \theta}$$

the exterior factor is

$$\frac{\sqrt{1-\delta^2} \cos \theta \tan^2 \theta}{\tan^2 \theta} = \frac{\cos \theta}{\sqrt{1-\delta^2}}$$

and we have

$$\Lambda = \frac{\cos \theta}{\sqrt{1-\delta^2}} \{ \sec^2 \theta \Pi_i(n, c) - \delta^2 Fc \},$$

which is the formula I used in the calculations. It would, however, have been better to reduce a step further; viz. we have

$$\sec^2 \theta \Pi_i(n, c) = \tan \theta \cos \theta \{ \}$$

$$= \frac{\sqrt{1-\delta^2}}{\cos \theta} \left\{ \frac{1}{2} \pi + Fc \left[ \cos \frac{\theta}{\sqrt{1-\delta^2}} + F(b, \theta) - E(b, \theta) \right] - E(c)F(b, \theta) \right\}$$

$$= \frac{\sqrt{1-\delta^2}}{\cos \theta} \left\{ \frac{1}{2} \Pi + Fc [ F(b, \theta) - E(b, \theta) ] - E(c)F(b, \theta) \right\} + Fc,$n

and thence

$$\sec^2 \theta \Pi_i(n, c) - \delta^2 Fc$$

$$= \frac{\sqrt{1-\delta^2}}{\cos \theta} \left\{ \frac{1}{2} \Pi + Fc [ \sqrt{1-\delta^2} \cos \theta + F(b, \theta) - E(b, \theta) ] - E(c)F(b, \theta) \right\} ;$$

or, finally,

$$\Lambda = \frac{1}{2} \pi + FcF(b, \theta) - FcE(b, \theta) - E(c)F(b, \theta) + \sqrt{1-\delta^2} \cos \theta \tan \theta.$$

It is easy with this expression of $\Lambda$ to obtain the results already found for the extreme values $\theta = 0^\circ, \theta = 90^\circ$.

As Legendre's Tables have for argument, not the modulus $c$, but the angle of the modulus, say $\chi$ (that is, $\sin \chi = c = \delta \sin \theta$), it is convenient to replace $\sqrt{1-\delta^2} \sin^2 \theta$ by its value $\cos \chi$; and the formulae thus are

$$s = \frac{\sqrt{1-\delta^2}}{\cos \chi}E(c),$$

$$\Lambda = \frac{1}{2} \pi + Fc[ \sqrt{1-\delta^2} \cos \theta + F(b, \theta) - E(b, \theta) ] - E(c)F(b, \theta),$$

where

$$C = \sin \chi = \delta \sin \theta, \quad \tan \theta = \sqrt{1-\delta^2} \tan \theta, \quad \theta = 90^\circ - \theta.$$

And in the case intended to be numerically discussed, $\delta = \frac{1}{2} \sqrt{3}$, $\sqrt{1-\delta^2} = \frac{1}{2}$. I take $\theta$ as the argument, giving it the values $0^\circ, 10^\circ, \ldots 90^\circ$, and perform the calculation as shown in the Table.
| \(x\) | \(90° - x\) | \(\cos x\) | \(E, \log\) | \(E, \text{nat.}\) | \(E, \log\) | \(E, \text{nat.}\) | \(|\cos x + 1 - \cos x|, \text{nat.}\) | \(\log E, \text{nat.}\) | \(\text{Addlog} E, \text{nat.}\) | \((1)\) | \((2)\) | \(\frac{\pi}{2} + (1) - (2)\) |
|-----|-----------|----------|--------|--------------|--------|--------------|-------------------|----------------|-----------------|------|-------|-------------|
| 10.00 | 90.00 | 0.00 | 18.86 | 1.0686 | 1.0376 | 1.0523 | 0.1827 | 3.8820 | 4.4446 | -2.2007 | -50.0833 | 2.1323 | -80.2222 | 55.8205 | 6.1058 | 90.29 |
| 20.71 | 69.29 | 0.97 | 16.53 | 1.6010 | 1.4633 | 1.6441 | 0.9206 | 1.0244 | 2.5769 | -0.7273 | 3.3681 | 2.1779 | -55.2538 | 48.51 | 23.0753 | 1.0713 | 0.9506 |
| 30.59 | 60.41 | 1.00 | 14.17 | 1.7990 | 1.3857 | 1.6621 | 0.9521 | 1.3347 | 1.7287 | 0.9835 | 0.9267 | 0.7655 | 2.0522 | 1.0678 | -92.5753 | 2.2623 | 1.8333 | -9.1666 |
| 40.50 | 50.50 | 1.03 | 12.15 | 1.8991 | 1.3232 | 1.6941 | 0.9214 | 1.3403 | 1.5486 | 0.8191 | -6.391 | 1.9641 | -0.8808 | 1.2192 | 1.0110 | 5.0629 | 6.8190 | -9.990 |
| 50.52 | 40.48 | 1.06 | 10.64 | 1.9923 | 1.2777 | 1.7180 | 0.6793 | 2.3266 | 1.6667 | 1.6667 | -4.635 | 1.8562 | 1.9620 | 1.1717 | 1.1168 | 5.9328 | 2.1224 | -1.0612 |
| 60.53 | 30.47 | 1.09 | 9.23 | 1.9202 | 1.2161 | 1.7508 | 0.5165 | 1.5292 | 1.8446 | 1.4992 | -3.157 | 1.7219 | 1.8205 | -0.6615 | 1.2250 | 7.1351 | 7.2479 | -1.2339 |
| 70.58 | 20.42 | 1.13 | 7.81 | 2.1154 | 1.2266 | 1.7509 | 0.4371 | 0.9327 | 2.9697 | 1.2591 | -1.973 | 1.5429 | 1.6337 | -4.3033 | 1.3378 | 7.3639 | 2.3410 | -1.7056 |
| 80.70 | 10.30 | 1.16 | 6.49 | 2.1463 | 1.2147 | 1.7431 | 0.4433 | 2.6463 | 1.9781 | 0.951 | 1.2423 | 1.3268 | -2.123 | 1.4536 | 8.3854 | 2.4240 | -2.120 |

\(x, 90° - x\) in degrees and decimals of a degree, to correspond with Legendre's Tables.
where the columns marked with an * show respectively the longitude of the node, and the length (or distance of node from vertex), for the geodesic lines belonging to the different values of the argument \(\theta\).

The remarks which follow have reference to the stereographic projection of the figure on the plane of the equator, the centre of projection being the pole (say the South Pole) of the spheroid. It is to be remarked that if a point \(P\) of the spheroid is projected as above, by means of an ordinate into the point \(Q\) of the sphere radius \(OK(=OA)\), then projecting stereographically as to the spheroid and the sphere from the south poles thereof respectively, the points \(P\) and \(Q\) have the same projection. And it is hence easy to show that an azimuth \(\alpha\) at a point of the meridian (parametric latitude \(\lambda'\), normal latitude \(\lambda\), and therefore \(\tan \lambda' = \frac{C}{A} \tan \lambda\)) is projected into an angle \((\alpha)\) such that

\[
\tan (\alpha) = \frac{\sin \lambda'}{\sin \lambda} \tan \alpha.
\]

In fact in Fig. 3, if we take therein \(OK, OC\) for the axes of \(x, z\) respectively, and the axis of \(y\) at right angles to the plane of the paper, and if we have at \(P\) on the surface of the spheroid an element of length \(PR\) at the inclination \(\alpha\) to the meridian \(PK\), then if \(x, y, z\) are the coordinates of \(P\), and \(x + \delta x, y + \delta y, z + \delta z\) those of \(R\), we have

\[
\begin{align*}
\delta x &= \rho \cos \alpha \sin \lambda, \\
\delta z &= -\rho \cos \alpha \cos \lambda, \\
\delta y &= \rho \sin \alpha,
\end{align*}
\]

and thence

\[
\tan \alpha = \frac{\delta y}{\sqrt{\delta x^2 + \delta z^2}}.
\]

Now, if the meridian and the points \(P, R\) are referred by lines parallel to \(Oz\) to the surface of the sphere radius \(OA\), the only difference is that the ordinates \(z\) are increased in the ratio \(C : A\); so that if the projected angle be \((\alpha)\), we have

\[
\tan (\alpha) = \frac{\delta y}{\sqrt{\delta x^2 + \frac{A^2}{C^2} \delta z^2}};
\]

and then projecting the sphere stereographically from its south pole, the angle in the projection is \(= (\alpha)\). And according to the foregoing remark, the angle \((\alpha)\) thus obtained is also the projec-
We have thus
\[
\tan \left( \frac{\alpha}{\tan \alpha} \right) = \frac{\sqrt{\delta z^2 + \delta z^2}}{\sqrt{dz^2 + \frac{A^2}{C^2} \delta z^2}} = \frac{\sqrt{\sin^2 \lambda + \cos^2 \lambda}}{\sqrt{\sin^2 \lambda + \frac{A^2}{C^2} \cos^2 \lambda}}
\]
\[
= \sqrt{\frac{1 + \cot^2 \lambda}{1 + \cot^2 \lambda'}} = \frac{\sin \lambda'}{\sin \lambda},
\]
which is the required relation.

The foregoing equations,
\[
\cos \lambda' \sin \alpha = \cos \lambda', \quad \tan \lambda' = \frac{C}{A} \tan \lambda,
\]
\[
\tan \left( \frac{\alpha}{\tan \alpha} \right) = \frac{\sin \lambda'}{\sin \lambda} \tan \alpha,
\]
determine in the stereographic projection the inclination (\(\alpha\)) to the radius, or projection of the meridian, of the geodesic line (parametric latitude of vertex = \(\lambda'\)) at the point the parametric latitude of which is = \(\lambda'\); viz. they enable the construction (in the projection) of the direction of the successive elements of the geodesic line. There would be no difficulty in performing the construction geometrically; but it would, I think, be more convenient to calculate (\(\alpha\)) numerically for a given value of \(\lambda'\) and for the successive values of \(\lambda'\). Observe that for \(\lambda' = 0\) we have (as above) \(90^\circ - \alpha = \lambda'\), and then \(\frac{\sin \lambda'}{\sin \lambda} = \frac{\tan \lambda'}{\tan \lambda} = \frac{C}{A}\), consequently \(\tan \left( \frac{\alpha}{\tan \alpha} \right) = \frac{C}{A} \cot \lambda'\); but we have also \(\cot \lambda' = \frac{A}{C} \cot \lambda\), so that this equation becomes \(\tan \left( \frac{\alpha}{\tan \alpha} \right) = \cot \lambda\); or we have \(90^\circ - \alpha = \lambda\); viz. in the projection, the geodesic line cuts the equator at an angle \(\lambda = \) the normal latitude of the vertex of the geodesic line.

The preceding formulæ and results have enabled me to construct a drawing, on a large scale, of the stereographic projection of the geodesic lines for the spheroid, polar axis = \(\frac{1}{2}\) equatorial axis.

XLIV. Reply to Mr. Templeton's "Remarks suggested by Mr. Douglas's Account of a New Optometer," By J. C. Douglas, Government Science Teacher, Assistant Superintendent East-India Government Telegraph Department*,

Unfortunately Mr. Templeton has misunderstood my description of the optometer principle; his proof that the indications of an instrument on this principle are fallacious is therefore not applicable. I did not give exact measurements, as such were not necessary to a description of the principle; I

* Communicated by the Author.
Remarks on a New Optometer. 341

merely indicated a principle on which an instrument might be constructed for accurately defining the region of accommodation. I did this after innumerable experiments had convinced me of the existence of the phenomena described, and careful consideration had led me to the explanation of the same. After I had arrived at my results I referred to Brewster’s Journal; I found there the experiment of Le Cat, to which, I presume, Mr. Templeton refers when he states, “the case is simply an extreme example of the experiment described half a century ago in Brewster’s Journal.” In my former paper I have referred to Brewster’s Journal (vol. iv. p. 89); but the experiment there described is not that described by me, as comparison will prove; it is not applied to any useful purpose; and, as I have already insisted, the explanation given is not the true one*.

The first production of multiple images by two or more orifices is due to Father Scheiner (1685); the application of Father Scheiner’s experiment to optometry to Porterfield (1759)†. Helmholtz recommends as a means of finding the far point the use of a point of light, a small opaque body being passed close to the eye, and the light moved to or from the eye until the opaque body is no longer seen in transit. I have suggested the use of two or more luminous points; and instead of moving these luminous points, I proposed to move the opaque body to or from the eye until it is seen single. The proposed method has several advantages: it is easily applied, more strictly defines the distance required, particularly in the case of an inexperienced observer; for the several shadows are very distinct, and their coalescence is therefore easily traced, easier than the gradual disappearance in Helmholtz’s experiments; and the multiplication of shadows may be applied to purposes other than optometrical; for example, if several stars appear to be one only, in consequence of the encroachment on each other of their circles of confusion, by passing a small object close to the cornea, the number of shadows seen will indicate the number of luminous points.

Mr. Templeton has referred to hypotheses of lens-adjustment, the most difficult portion of physiological optics. Rather than repeat what has been written elsewhere, I would refer Mr. Templeton to the chapters treating of this subject and the history of its development in Professor Helmholtz’s work referred to already. The treatment is very full, and the several theories proposed are tested by experiment. Mr. Templeton’s experiment with the ruler may be explained on the same principle as the formation of images by a pinhole, the frowning of myopes, &c.

Mr. Templeton states, “physicists have assumed, on quite in-* Another refutation of the explanation referred to appeared in the Philosophical Magazine for June last, in a paper by Mr. J. L. Tupper.
† Helmholtz, Physiologische Optik. Leipzig, 1867.
sufficient evidence, that the eye is simply a camera obscura, with a lens to form a picture, a retinal surface to receive a picture, and an adjusting-power for distance.” That inaccuracies exist in works on “physics” is not to be denied; but text-books on experimental physics are scarcely the sources from which to obtain authoritative statements on difficult points in physiological optics. The comprehensive work of Professor Helmholtz*, the works of Donders, C. Stellwag, von Carion, &c., would be truer representatives of the opinions of those who are building up this branch of knowledge. I do not find such physicists consider the eye any more like a camera obscura than it really is. Helmholtz, for instance, states in one place, the camera obscura is the optical instrument most similar to the eye in giving real images of objects; in another place, the eye acts on incident light essentially as a camera obscura (Physiologische Optik); but he fully elucidates the differences, and calls in the analogy where it assists his description; he discusses the various hypotheses of accommodation; considers not the lens only, but also the other refractive media, the several curvatures, the great relative thickness of the refractive media, &c. Mr. Templeton is no doubt aware images may be viewed through the sclerotica both in the dead and living eye, and in the latter by the ophthalmoscope; while accommodation is universally experienced, is observable with the ophthalmoscope, producible in the dead eye by galvanism, and measured in the living eye more or less accurately with the ophthalmometer. In production and reception of the image and in adjustment is not the analogy proved?

Comparative study of the visual apparatus would be as efficient in assisting to a correct knowledge of the eye in man as the study of comparative physiology has been in the development of human physiology; but observations to be useful must, I submit, be accurate, and the publication of experiments which do not even give any qualitative result, positive or negative, is undesirable. Mr. Templeton states he has endeavoured to find the principal focus of the crystalline lens without success by experiments which “carry with them a suspicion of inexactness.” I submit such experiments are useless, as no conclusion can be drawn from them: they do not afford even a useful degree of probability, the matter they are instituted to decide remaining as doubtful after as before their institution. The principal focus of the crystalline lens has been measured by Helmholtz, a reference to whose experiments† will point out the precautions to be taken, and may explain why Mr. Templeton’s experiments were not decisive.

* Physiologische Optik.
Mr. Templeton has given an extract without mentioning his authority. I submit, this should have been given, as readers might probably infer from the heading of Mr. Templeton’s paper that the quotation represented my opinion. Mr. Templeton insists "that further inquiry should take place before it be assumed that ‘the known properties of light afford a complete elucidation of the whole mechanism of vision and the use of every part of the visual apparatus.’" I have never met with the above statement in works on vision; on the contrary, I find the admission very general that knowledge in this direction is very incomplete.

Helmholtz states that doubts may be justly entertained if the state of this young and rising branch of science admits of such treatment, even only preliminary, as the plan of his book demands. And in another place, as the attempt to introduce order and connexion must ultimately be made, and as the science must be ultimately freed from contradictions, he has undertaken the task under the conviction that order and connexion, even on an unstable basis, are better than contradiction and disconnexion. He also states that the history of the science would only have an interest corresponding to the labour of its compilation if the condition of the science itself were riper than it is at present*. In the body of the same work it will be found stated that much remains to be cleared up authoritatively. I have chosen Helmholtz as the leading work on the subject; but reference to other writers will prove the general prevalence of the above opinion. Ophthalmic surgical works will, I think, be found to state the matter differently from Mr. Templeton.

I have remarked above on the information afforded concerning vision by elementary works on "Natural Philosophy," in stating that such works are not to be regarded as authorities while more special works exist. I would not have it understood I think inaccuracy in such works should be excused on the ground of their general character; I consider such works should have that reliability which reference to the latest special works alone can ensure. Accuracy both in fact and expression, and strictness in drawing conclusions, are absolutely necessary in works used as much in forming habits of thought as in communicating facts. I regret to say these conditions are not always fulfilled in the general works referred to in treating of vision. For example, in the third edition of a text-book on Physics, a translation from the French, dated July 1868, in the chapter on the Eye considered as an Optical Instrument, I find the following statements: the eye is compared to a camera obscura, "of which the pupil is the aperture, the crystalline the condensing lens, and the retina the screen." In this extract the analogy to a camera obscura is car-

* Preface to Physiologische Optik.
ried to inaccuracy, and Mr. Templeton's objection is perfectly valid. In referring to the eye of an albino &c., "this has the advantage that, as the choroid is destitute of pigment, light can traverse it without loss;" in other words, the choroid is perfectly transparent. The distance of distinct vision has been retained, although it has been proved such does not exist as a definite distance; the region of accommodation, near point, and far point are not referred to; myopia and presbyopia are regarded as opposite defects; short sight is defined as "the habitual accommodation of the eyes for a distance less than that of ordinary vision;" spectacles are calculated from the distance of distinct vision; as a proof of chromatic aberration in the eye, it is stated, the image of a white disk on a dark ground, if formed before or behind the retina, appears surrounded by a very narrow blue edge, and the optical centre of the lens is used without reference to the effect of the thickness of the refractive media. Brevity cannot be urged as an excuse for inaccuracy. Dr. Donders's 'Anomalies of Refraction and Accommodation' was published in English in 1864; and Professor Helmholtz's Physiologische Optik was published early in 1867.

Carpenter's 'Physiology' has been brought up to date; this and the recent publication of several surgical works must tend to disseminate views consonant with the more recent discoveries. The work selected by me as an example no doubt does good service in the cause of elementary scientific education; but I think such works as Professor Tyndall's 'Heat' and 'Sound,' and the excellent philosophical elementary works of Professor Huxley, do more good, and are more useful to the teacher, than works which treat briefly of ten or twelve subjects. Professor Huxley's elementary lectures appear, in my humble opinion, perfect models, not cramming many facts into little space, and fully developing the capacity of elementary scientific instruction for training the mind in correct habits of thought. I do not think such general works as those referred to above could be made to approach such a model without greatly increasing their bulk and making the expense of using them a still greater objection than it is at present; while the vast labour of reference in compiling a work on so many subjects must preclude the possibility of the facts being so reliable as in more special works. It may be urged that a work treating only of the rudiments of a subject can be easily kept up to date; but there is yet the danger of even this foundation being shaken by discovery, and of statements no longer true being copied from time to time, from the difficulty, or even impossibility, of one man being equal to the labour of the necessary reference.
XLV. On Approach caused by Vibration.
By Frederick Guthrie.

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In the Proceedings of the Royal Society of London, vol. xix. p. 35, there is published a considerable portion of the accompanying paper "On Approach caused by Vibration." The chief part thence omitted is that relating to the modifications which Mr. Faraday's "surface-currents" undergo when excited in the neighbourhood of a rigid plane. I beg to submit the complete series of experiments to your readers, because I believe that the examination of these currents has a distinct bearing upon the chief question—not in regard to its ultimate solution, but in regard to the method by which that solution is obtained.

Yours truly,

Frederick Guthrie.

September 14, 1870.

§ 1. The chain of experiments which I have to describe arose from the endeavour to explain an observation that a delicately suspended piece of cardboard moves from a considerable distance towards a vibrating tuning-fork. It will be preferable to detail the experiments, not in the order in which they occurred to me and were actually performed, but in the order in which I conceive them to form a logical sequence.

§ 2. The experiment of Clément shows that when a continuously renewed current of air passes between two parallel disks from the common axis towards the circumference, the disks are urged together. Consequently, in seeking to explain the fact observed, § 1, it was necessary to examine the air surrounding the resonant fork in order to ascertain whether air-currents existed in its neighbourhood—and, further, to distinguish between such currents as might be found to move in closed curves, forming whirlwinds in the immediate neighbourhood of the fork, and such as might radiate in unclosed paths from the fork through the air.

§ 3. In 1831 Mr. Faraday*, in tracing the cause of the accumulation of light particles on the internodal points and lines of vibrating bodies, came to the conclusion that such accumulation was due to minute whirlwinds, and not, as had been held by M. Savart†, to the existence of secondary nodes. By fastening

* Philosophical Transactions, 1831, p. 299.

pieces of card in planes perpendicular to the plane of the vibrating body, Mr. Faraday was able to control the accumulation of the powder, and by performing the experiment in vacuo he showed that the internodal deposition no longer took place. By scattering a light powder upon a tuning-fork, one prong of which was vertically above the other, both being horizontal and vibrating in a vertical plane, Mr. Faraday showed how the powder collected along the middle line of the prong in little travelling heaps, each heap consisting of moving particles which rose from the fork in the middle of the heap, fell down the side of the heap and were gathered in at the bottom. A general conclusion at which Mr. Faraday arrived was this: whenever the different parts of a surface are vibrated to different degrees, there is always a tendency for the air to flow along the surface of the vibrating body towards the more violently agitated portions from the less agitated.

§ 4. It is clear that, before examining the possible connexion between these superficial whirlwinds and the fact mentioned in § 1, it is necessary to examine into the existence of air-currents of unclosed paths. The tuning-fork which was most employed, and which I call fork $A$, vibrated 256 times per second (UT$_2$), and had the following dimensions (Plate IV. fig. 1.):—

$$
\begin{align*}
OY &= 0.0230 \\
OX &= 0.0172 \\
OZ &= 0.3255.
\end{align*}
$$

I call the three faces intersecting in $O$ the faces $a$, $b$, and $c$ respectively. Let the symbol $H_a$, &c., denote the position of the fork when $c$ is horizontal, &c.

§ 5. Experiment 1.—The fork $A$ was set vibrating by drawing the bow across the edge $OY$; the plane of vibration was accordingly parallel to $b$. The fork was then brought into the neighbourhood of an ascending thread of smoke. The smoke and fork had in succession the three relative positions:—

1. $H_a$. The smoke passed across the face $a$ parallel to $OY$.
2. $H_b$. The smoke passed across the face $b$ parallel to $OX$.
3. $H_c$. The smoke passed across the face $c$ parallel to $OX$.

In all cases the smoke clung to the surface across which it passed in the same manner as if the fork were at rest.

§ 6. Experiment 2.—A cylindrical glass tube $T$ (fig. 10), 0.4 metre long and 0.042 metre internal diameter, was fastened in a horizontal position. One end was left open, the other carried a cork, through the centre of which passed a horizontal tube $t$, 0.04 metre long and 0.0035 metre internal diameter. These tubes were filled with smoke, and the fork $A$, which had been set vibrating
as in § 5, was brought to the open end of the wide tube. The fork and tube had in succession the four relative positions shown in fig. 2, namely:

(1) $H_c$, axis of tubes perpendicular to $a$.
(2) $H_c$, axis of tubes perpendicular to $b$.
(3) $H_a$ or $H_b$, axis of tubes perpendicular to $c$.
(4) The same as (3), but having one prong of the fork thrust as far as possible into the tube $T$. In none of the cases did the smoke show any tendency to escape through the tube $t$; nor was fresh air drawn in.

§ 7. Experiment 3.—The cork and tube $t$ of Experiment 2 were withdrawn and replaced by a film bubble of glycerine-soap water. The combinations of position of Experiment 2 were repeated. In none of the cases did the bubble show any variation from the vertical plane.

§ 8. Hence I conclude that when a tuning-fork is in a state of plane vibration, no permanent true air-currents are formed; that is, no air-currents could be detected departing from any side of the fork and penetrating the surrounding air in unclosed paths.

§ 9. The superficial whirlwinds examined by Mr. Faraday may be supposed to be greatly modified when they are excited in the immediate neighbourhood of a solid body; and as the "attraction" which formed the starting-point of the present examination (§ 1) is exerted upon a solid body in the neighbourhood of the resonant fork, some experiments supplementary to those of Mr. Faraday were found necessary.

§ 10. In general, when a light powder is strewn upon a smooth surface and a vibrating fork is brought near the surface, the arrangement of the particles of the powder will depend upon (1) the superficial whirlwinds, and (2) the nodal lines of the surface which vibrates by induction. Accordingly, and to get rid of the latter class of phenomena, my first experiments in this direction were made by employing an anvil with a smooth surface to receive the powder. However, as experiment speedily showed me that the arrangement of the powder on the surface was the same upon a sheet of glass, cardboard, or paper as upon an anvil, I adopted the first of these substances, on account of its smoothness, and because it enabled me to examine the motions of the particles from below.

§ 11. Experiment 4.—Freshly baked carbonate of magnesium was evenly strewn upon a horizontal sheet of plate glass. The fork $A$, screwed into its sounding-box, was so adjusted that when at rest ($H_a$) the surface $a$ was parallel to the plate. The bow was drawn across $f$. When the fork was in full vibration (0.0025 metre amplitude) the glass plate was raised vertically.
At a distance of about 0·005 metre the magnesia begins to be disturbed. It is dragged from a distance of about 0·008 metre on both sides of the fork towards the fork. It collects in three ridges beneath the fork—namely, in two very fine ridges exactly beneath the edges of the prong, and a very much thicker ridge beneath the centre of the prong. Seen in section parallel to \(c\), the accumulations appear as in fig. 3 (1). The general appearance when the fork is withdrawn is shown in fig. 3 (2).

§ 12. It appears from the ultimate distribution of the powder, as well as from the motion of the particles during the commencement of the disturbance, that four cylindrical whirlwinds are here concerned, the position and direction of which are shown in section in fig. 4. As the air moving in the whirl \(m\) and the air moving in the whirl \(n\) approach one another horizontally, they drag the powder with them towards \(s\). When they meet and turn vertically upwards, there is necessarily about \(s\) a region of comparative calm. For the same reason there is a region of comparative calm about \(t\). Similarly the air which descends at \(v\) is divided as it approaches the rigid surface, some of it entering the whirl \(n\) and some of it entering the whirl \(o\). At the exact centre of \(v\) there is calm, where the air goes neither to the right nor left. That there are upward currents above \(s\) and \(t\) is shown by the projection of particles in the directions \(q\) and \(r\) when the vibration is very violent and the plate is very near the fork.

§ 13. That there is a region of calm where the axis of the descending current would, if produced, cut the horizontal surface, and where the axis of the vertically ascending current, formed by the clashing of two horizontal currents, would meet the surface, is shown by blowing dry air upon the strewn plate, (1) through a single vertical tube, and (2) through two neighbouring ones. In the first case, fig. 5 (1), the powder appears undisturbed immediately below the centre of the tube; and in the second case the powder remains undisturbed, not only below each tube, but also midway between the two, fig. 5 (2).

It is clear that the occurrence of the powder in the places described may be partly attributed to the equilibrium between equal and opposite forces. This would account for the repose of the particles in the places specified, but not for their accumulation there.

§ 14. Experiment 5.—An arrangement was employed precisely similar to that described in § 11, with the exception that \((H_b)\) the face \(b\) was horizontal. On stroking the fork across \(OY\) and bringing the magnesia near it, no disturbance was shown until the interval between the fork and powder was about 0·001 metre. Then the powder moved slightly \(en masse\) towards that portion of the plate which was accidentally a little the fur-
thest from the fork, fig. 6 (1) and (2). When both plate and fork were accurately horizontal, and the strongly vibrating fork was very close to the powder, the latter partly separated into irregularly shaped flakes which showed no inclination to adjust themselves into any definite geometric form.

§ 15. It may be remarked that when powder is strewn on the face $b$, when the fork is vibrating as in § 14, the particles are jerked in straight lines backwards and forwards to distances depending upon their masses. These lines cross the central line of the fork's $b$-face; and although, after a time, it is along this line that powder chiefly remains, this is because, at other parts, the powder is cast off at one or other edge as each particle falls within the amplitude of the fork's vibrations.

§ 16. I conclude from §§ 14 and 15 that no sensible whirlwinds are established in the air in contact with the $b$-face when the plane of vibration is parallel to that face.

§ 17. Experiment 6.—The fork was fastened vertically downwards ($H_2$); and being set in vibration as in § 11, the glass plate strewn with magnesia was approached vertically. As in Experiment 5, the two had to be brought very near before any disturbance occurred. When within 1 or 2 millims. the magnesia began to be scattered, the portion nearest to the edges parallel to $f$ (fig. 1) being most disturbed and being driven from under the fork.

This distribution of the powder is evidently to be traced to the different lengths of path and consequent different rates of motion of the different parts of the face $c$. The edges parallel to $f$ (fig. 1) move further and faster than the central line between them, since (fig. 8) $CA > CB$. Indeed, assuming that the prong remains unbent and swings as a pendulum about $C$, then

\[
\frac{\text{mean velocity of } A}{\text{mean velocity of } B} = \frac{CA}{CB},
\]

or in the case of fork $A$, which has the dimensions given in § 4,

\[
\frac{\text{mean velocity of } A}{\text{mean velocity of } B} = \frac{3256}{3255} = 1.0003.
\]

The result is that the prevailing tendency to disturbance depends upon the interference of two currents, $p$ and $q$ (fig. 8), which are set up alternately. As before, the space $v$ between $p$ and $q$ will be a spot of comparative calm, but not a spot for accumulation.

§ 18. If the face $c$ be horizontal but turned upwards, and if the powder be sprinkled upon the upturned face, it is set in violent agitation. Each particle moves to and fro with great velocity in the plane of vibration. Those nearest the edge are pretty sure to be presently thrown off. Those which are so situated as to be
thrown across the middle line parallel to $f$, remain on the fork for a longer time. The apparent effect produced is assisted by the retention of images on the retina; and the appearance is as though a nebulous band of powder rested along the middle of the $c$ face parallel to $f$. On account of the greater amplitude of the vibrations at the edges of the prong, a particle there will be hurled forward with greater force than one nearer to the central line. If the particle projected happen to alight upon the fork when the latter is moving in the same direction as the particle, it will be hurled forwards again, and perhaps be precipitated over the edge parallel to $f$. If it alight when the fork is moving in the opposite direction it will be thrown back again.

§ 19. It appears, then, from §§ 11–18 that when the fork is vibrating in a plane parallel to $b$, it is only on face $a$ that any appreciable air-circuits are formed, and that there such circuits do not extend sensibly beyond at most 0.006 metre.

§ 20. We shall see that the existence of such air-circuits, confined as they are to the immediate vicinity of the fork, are quite insufficient to account for the class of phenomena which have to be described, and which are similar to the fundamental fact mentioned in § 1.

§ 21. Experiment 7.—To one end of a splinter of wood, 0.5 metre long, a card 0.08 metre square was fastened in such a way that the plane of the card was vertical, and contained the line of the splinter. The whole was hung from a fibre of unspun silk (fig. 9) and counterpoised. The tuning-fork $A$ was set in vibration as before, and was brought towards the card in the three relative positions corresponding to those of § 6; namely:

(1) ($H_a$). The face $a$ parallel to the card.
(2) ($H_b$). The face $b$ parallel to the card.
(3) ($H_a$ or $H_b$). The face $c$ parallel to the card.

In all three cases the card moved towards the fork. The rate at which the card moved was greatest when the fork was sounding loudest. In all three cases it was possible to draw the card from a distance of 0.05 metre at least,—a distance quite beyond the direct influence of the superficial whirls which exist in position (1) (on face $a$).

§ 22. There is perhaps nothing essentially contrary to reason in the conception of two bodies in space free to move, so related to one another that while the first has no tendency to move towards the second, the second has a tendency to move towards the first. But if the tendency of the one to move is caused by the condition of the medium between the two, it seems inevitable that the tendency shall be mutual. Thus, if that tendency result from a general diminution in the tension of an elastic medium
between the two, they will be urged towards one another. To test the reciprocity of the motive tendency in the case under consideration, the following experiment was tried.

§ 23. Experiment 8.—The tuning-fork \( A \) was fastened to the end of a rod 1 metre long; the other end of the rod was counterpoised, and the whole was hung from a silk tape. If the vertical plane passing through the rod be called \( V \), then the rod and fork received in succession the three relative positions:

1. \((H_c), \) \( V \) parallel to \( a \).
2. \((H_e), \) \( V \) parallel to \( b \).
3. \((H_a \text{ or } H_b), \) \( V \) parallel to \( c \).

In (1) and (2) the fork was simply hung from the suspended rod; in (3) it was fastened to an iron rod in the direction of its axis, and the two were then attached to the suspended rod at their common centre of gravity. The fork was sounded by the bow as before, and a piece of card 0.05 metre square was brought near the face \( a \) in (1), \( b \) in (2), and \( c \) in (3). In all cases the suspended fork approached the card; but, owing to the great inertia of the suspended fork and counterpoise, the motion was much slower and less striking than was the case when the card was hung.

§ 24. Experiment 9.—Further, instead of a card, a second fork \( B \) (sounding \( A \)) was set in vibration, and brought into the neighbourhood of the vibrating suspended fork \( A \). The three faces \( a', b', c' \) of the fork \( B \) were held in succession parallel to the three faces \( a, b, c \) of the fork \( A \)—that is, parallel to \( V \) when the faces \( a, b, c \) were in each of the three positions described in § 23. There were thus nine combinations effected. In every case the suspended fork approached the stationary one. Hence, to whatever cause the approach is due, the action is mutual.

§ 25. The next question, the solution of which promised to throw light upon our problem, was this:—What is the general or mean condition as to tension of a medium in which undulations are generated? Though this question has received very great attention from theoretical physicists, it has not been approached, as far as I am aware, from the side of experiment in the manner to be described.

§ 26. Experiment 10.—The fork \( A \) was fixed in an upright position in its sounding-box. One of its prongs was closed in a glass tube \( T \), 0.4 metre long and 0.042 metre internal diameter, carrying a cork through which the prong passed. The upper end of \( T \) (fig. 10) also carried a cork through which passed a narrow tube \( t \), bent twice at right angles and dipping into water. The internal diameter of \( t \) was 0.0035 metre. The corks of the tube \( T \) were made tight with wax, and a little air was expelled
from the tube T by warming it with the hand, so that, when the atmospheric temperature was regained, the water stood at some distance up the tube t. The tube t was firmly clamped in several places to prevent vibration, and consequent centrifugal effect. On passing the bow across f, the enclosed prong was also set in vibration. When the amplitude of the vibration was as great as possible, the water had sunk in the tube t to the amount of 0.003 metre. The moment both prongs were suddenly stopped the level of the water in t was restored. The depression of the level in t cannot be due to increased temperature; for, if it were so, the increase of volume would be gradual and accumulative, and on stopping the vibration the contraction due to cooling would be also gradual, whereas the attainment of maximum depression and the restoration to normal volume are practically instantaneous.

§ 27. We have here accordingly an experimental proof that the rapid motion (in this instance vibration) of a body in a medium produces on the whole an effect similar to that which would be produced by the expansion of the body, namely, a displacement of the medium. If air were perfectly elastic and had no inertia, no such total displacement could ensue; and I think I may safely predict that the apparent expansion of the medium will be found, in the case of hydrogen less, and in the case of carbonic acid greater, than in that of air.*

§ 28. Though we know the dimensions of the fork and its rate of vibration, and though we can measure with tolerable accuracy the amplitude of its vibrations, we can only calculate from this the mean velocity of any given point, because in the middle of a vibration the fork is moving very much faster than towards the commencement or termination. Hence this vibratory displacement cannot, with our present data, be connected with the known rate at which air enters a vacuum.

§ 29. The fundamental experiment of §§ 1 and 21 next suggested for its explanation the following question. Let there be two equal and opposite forces, P and Q, producing equilibrium upon a body having inertia; let one of them, P, be increased and diminished by a series of equal increments and decrements following one another in rapid succession. Will the continually varying force, whose mean is P, maintain average equilibrium with the unaltered force Q? The plane of the cardboard in §§ 1 and 21 is the seat of two opposing forces, namely the pressure of the atmosphere on both sides. When the sounding-fork is held on one side, the pressure on that side undergoes successive and equal increments and decrements. Accordingly, if the question just proposed be answered in the negative, a sufficient ground

* Compare the sighing of an organ-pipe after it has been sounded.
Mr. F. Guthrie on Approach caused by Vibration. 353

will be at hand for the approach of the cardboard to the fork.

§ 30. Experiment 11.—A "Cartesian diver" was made out of a test-tube, a bubble of air, and a beaker-glass of water. This was so nicely adjusted that it rose when near the surface of the water, and sank when the top of the tube was 0·05 metre below the surface. When resting on the bottom of the beaker, the top of the test-tube was below the surface of the water. When the diver was resting on the bottom of the beaker, the tuning-fork A in a state of vibration was presented to the glass in various directions with regard to the tube. The fork was placed sometimes in contact with the water, sometimes in the neighbouring air, and sometimes in contact (towards the base of the fork) with the glass. Although the vibration of the bottom of the beaker caused the diver to leap up, it invariably sank again and showed no sign of undergoing any alteration in specific gravity. If, now, the question in § 29 were answerable in the negative, the equilibrium would have been destroyed, because the atmospheric pressure on the one hand, and the elasticity of the confined air on the other being equal and opposite forces, an alteration in one, caused by its subjection to successive sonorous waves, would have altered the volume of the confined air and so destroyed the equilibrium.

§ 31. I hoped to throw light upon the fundamental experiments of §§ 1 and 21 by varying the nature of the surface of the body which received the vibrations, with the view on the one hand of preserving them, and on the other of dispersing them as much as possible. With this view Experiments 12 to 15 were undertaken.

§ 32. Experiment 12 (fig. 11).—Upon one end of a splinter of wood 0·5 metre long, a cylinder of cardboard 0·03 metre in diameter and 0·04 metre deep, closed at the bottom, was fastened in such a manner that its axis was horizontal and its bottom in the plane V. The cylinder was counterpoised, and the whole was hung from an unspun silk thread. The vibrating fork A was brought near the open end of the cylinder in the three positions already described, and also with one prong inserted into and nearly touching the bottom of the cylinder. In all cases motion towards the fork ensued.

§ 33. Experiment 13.—A handful of cotton-wool was hung upon the splinter in place of the cylinder of Experiment 12. The cotton moved towards the fork from a distance of at least 0·05 metre, when the latter was presented to it in either of the three positions, § 6.

Muslin and washleather behaved in a similar manner.

§ 34. Experiment 14.—A paper circular drum, 0·25 metre in
diameter, having a rim 0·025 deep, was hung by a silk tape in the same manner as the cylinder of § 32. Parchment was stretched across the wide end of a funnel 0·20 metre in diameter. The neck of the funnel was placed in the mouth, and the drum of the funnel was brought opposite and parallel to the edged face of the paper drum. Air was rapidly forced into and drawn out of the funnel. The paper drum moved towards the funnel even from a distance of 0·1 metre.

§ 35. Experiment 15.—A sheet of cardboard 0·4 metre square was hung in the plane V from a rod 1 metre long. The cardboard was counterpoised and hung from a silk tape. The paper drum of § 34 was placed 0·05 from the cardboard and parallel to it, and was then tipped. The cardboard moved towards the drum.

§ 36. Experiment 16.—A rod of brass 1·2 metre long, provided at the ends with disks of brass perpendicular to the rod 0·26 metre in diameter, was set in longitudinal vibration by means of resined leather. One of the disks was held during the vibration near to the cardboard of § 35, also near the cotton-wool and muslin of § 33. In all cases the suspended body moved towards the disk. By this means it was easy to cause motion when the two were at the distance of 0·2 metre.

§ 37. I have in the preceding paragraphs sought to eliminate systematically secondary and disturbing influences from the fundamental experiment. The experimental results appear to me to point to the following conclusions.

Whenever an elastic medium is between two vibrating bodies, or between a vibrating body and one at rest, and when the vibrations are dispersed in consequence of their impact on one or both of the bodies, the bodies will be urged together.

The dispersion of a vibration produces a similar effect to that produced by the dispersion of the air-current in Clément’s experiment; and, like the latter, the effect is due to the pressure exerted by the medium, which is in a state of higher mean tension on the side of the body furthest from the origin of vibration than on the side towards it.

In mechanics—in nature there is no such thing as a pulling force. Though the term attraction may have been occasionally used in the above to denote the tendency of bodies to approach, the line of conclusions here indicated tends to argue that there is no such thing as attraction in the sense of a pulling force, and that two utterly isolated bodies cannot influence one another.

If the ætherial vibrations which are supposed to constitute radiant heat resemble the ærial vibrations which constitute radiant sound, the heat which all bodies possess, and which they are all supposed to radiate in exchange, will cause all bodies to be urged towards one another.
XLVI. Experimental and Theoretical Researches into the Figures of Equilibrium of a Liquid Mass without Weight.—Ninth, Tenth, Eleventh and last Series. By Professor Plateau*.

Ninth Series.—Secondary causes which affect the persistence of liquid films.—Film-figures of great permanence.—Historical survey of observations relating to liquid films.—Capillary ascension to great heights in tubes of large diameter.—Constitution of a current of gas passing through a liquid.

In the last Series I endeavoured to show that, although cohesion and internal viscosity play the chief part in the development of all liquid films, these causes are not sufficient when we have to do with films which are both large and durable, like those formed by soap-water, and that in such cases other and entirely distinct conditions must concur—namely, great superficial viscosity and a comparatively weak tension. But when such films have been actually produced, their duration is affected by a certain number of secondary causes, which I pass in review in this Series.

The first of these causes consists in the small disturbances communicated to the films by the movement of the surrounding air and by the vibrations conducted by the ground. These small disturbances no doubt act by overcoming the inertia and frictional resistance of the molecules; they thus hasten the descent of the molecules, and consequently the attenuation of the film; and besides this, they cause the breakage of the parts that are very thin. It is partly on this account that the films generally last longer in closed vessels; for then one of the causes of disturbance (namely the movement of the air) is got rid of.

A second cause is evaporation (when the liquid constituting the film is susceptible of it). From the experiments described in the last Series, I conclude that in the case of liquids which do not admit of being blown into bubbles, evaporation is favourable rather than hurtful to the permanence of the films. I try to account for this singular fact, and I show that the contrary is true in regard to liquids that are easily blown out into bubbles; that is to say, in the case of these the persistence of the films is diminished by evaporation. For instance, hemispherical bubbles about a centimetre in diameter, formed at the surface of a solution of Marseilles soap, last for several hours in an atmosphere.

saturated with aqueous vapour, but only for a few minutes when they are freely exposed to the air. The glycerine-solution not only does not emit vapour, but, on the contrary, absorbs the moisture of the surrounding air; and it is partly because of this that films of this liquid last so long even when exposed to the air.

In the third place, since gravity constantly causes the liquid to descend towards the base of the films, it is plain that, by getting rid of or lessening the action of this force, the duration of the film must be increased. Hence it evidently follows that, other things being equal, a horizontal film will last longer than one which is inclined or vertical. I have made this comparison in the case of films of soap-water formed upon rings of iron wire 7 centims. in diameter and exposed to the air, one being horizontal and the other vertical. The average persistence of the first was 25 seconds, and that of the latter 13 seconds. Hence the position, or, more accurately, the greater or less degree of inclination, of the film must be reckoned as one of the secondary causes that we are considering.

In the fourth place, combinations of films always last a much shorter time than figures formed of a single film. This is because the highly concave surfaces of the small masses of liquid which form the liquid edges, and especially those which exist at the points of junction of these edges, produce a continual drain upon the liquid of the films and thus tend powerfully to make them thinner. The combination of films into systems is therefore likewise one of the secondary causes which modify their permanence.

In the fifth place, films generally last longer in proportion as they are of smaller size. For instance, if systems of films are produced upon two skeletons of similar shape but of different sizes, the one on the smaller skeleton lasts the longest.

If the persistence generally diminishes when the size of the films is increased, this is, I think, simply because the greater a film is, the greater is the chance that one point or another will yield to some cause of rupture. Under certain circumstances this effect of size does not show itself; for instance, films of soap-water formed upon rings 10, 7, 2, and 1 centim. in diameter lasted on the average for the same length of time. This last fact may be explained by the consideration that the drain of liquid caused by the great concave curvature of the small quantity of liquid which connects the film with the whole of the inner circumference of the ring, tends to make the smaller films last a shorter time, and thus the effects of size and curvature may neutralize each other.

Lastly, it is needful, in the sixth place, to take account of the nature of the solid to which the film adheres, and of the condi-
tion of its surface. For instance, we know that films formed
upon rings or frames made of iron wire that has not been ox-
dized break immediately, or last only for a very short time; and
according to the Abbé Florimond, soap-bubbles of a much larger
size can be blown with a glass pipe than with a clay one &c.

From this examination of all the accessory circumstances, it
follows that a film of given size will last longest if it is a plane
horizontal film, attached all round to the side of a glass vessel,
entirely shielded from evaporation, and protected from the mo-
tion of the surrounding air, and, as much as possible, from the
tremors conducted along the ground. Now all these conditions
were fulfilled in the case of a film 7 centims. in diameter men-
tioned in my Seventh Series, formed of the glycerine solution
and placed inside a bottle: accordingly this film lasted eighteen
days.

I next pass to another subject. The beauty of the film-figures
of the glycerine-solution naturally gives rise to the wish to have
them entirely permanent. In the case of one of them (the
sphere) this object is attained, as every one knows, by means of
molten glass; but the production of other figures in this mate-
rial, especially of such as are formed by an assemblage of films,
would present difficulties, and in any case it would not be con-
venient. The first idea that suggests itself is to employ a liquid
which produces films that become solid by simple evaporation in
the cold, such as collodion, solution of albumen, &c.; but with
a liquid of this kind no result can be obtained except by limiting
our attempts to figures of very small size.

Hence, in order to succeed in producing figures of tolerable
size, we are obliged to have recourse to substances which, like
glass, are liquid only at high temperatures, and to seek for one
which fulfils the double condition of not requiring a very high
temperature to melt it, and of being capable of extension, in the
molten state, into films of sufficient size. I succeeded almost
completely with a mixture of one part of pure gutta percha and
five parts of resin, kept at a temperature of about 150° C.; the
frame employed was a cube measuring 5 centims. along the edge.
The system of films that was produced was very firm, and lasted,
I think, more than two years, when a slight blow reduced it to
fragments—from which we must conclude that the constitution
of the films had undergone a gradual change. I think one
would succeed still better, and that the gradual alteration would
be less, if a somewhat larger proportion of resin were employed.

I conclude the part of my work which is specially devoted to
liquid films by a succinct account of every thing that, as far as
my knowledge goes, has been published in relation to such films
independently of my own researches.
According to figures represented on an Etruscan vase preserved in the Museum of the Louvre, it appears that even in ancient times children amused themselves with blowing complete bubbles. With regard to modern times, I will content myself in this extract with saying that the colours, tension, pressure, average thickness, persistence, constitution, and different modes of production of the films, the phenomena of endosmose exhibited by them, a few special facts and applications, the form of certain films, and, subsequently to my investigations, of systems of films have been made the subject of these observations.

Before returning to the general questions connected with the figures of equilibrium, I discuss in the present Series two phenomena, the discussion of which could not well be brought in elsewhere.

As every one knows, the ascent of liquids in tubes, the sides of which they are able to wet, does not take place to any considerable extent except when the internal diameters of the tubes are very small, whence has arisen the expression capillary phenomena; and gravity always puts a limit to the height of the column that is raised. But if the action of gravity were neutralized, these limitations ought to disappear, and a liquid ought to be able to rise to any height in a tube of any diameter.

It appeared to me to be interesting to try this application of my processes, by using either oil surrounded by the alcoholic liquid, or the alcoholic liquid surrounded by oil. The tubes that I employed were both of them 42 centims. long, and one 14 and the other 15 centims. in internal diameter. I describe in the memoir a series of indispensable precautions, which it would occupy too much space to mention here, and by means of which the experiments completely succeeded. The oil in one of the tubes and the alcoholic mixture in the other rose gradually to the top; the motion of the oil, however, was retarded, while that of the alcoholic mixture was accelerated. The oil required 21 minutes 1 second in order to rise 4 decims.; the first decimetre was traversed in 1 minute 47 seconds, and the fourth in 9 minutes. To rise in like manner 4 decimetres, the alcoholic mixture took only 5 minutes 55 seconds; it accomplished the first decimetre in 1 minute 42 seconds, and the fourth in 1 minute 16 seconds.

The second fact that I discuss here is the constitution of a current of gas traversing a liquid. A current of air issuing from a round hole and rising through a liquid may be considered as the converse of a liquid vein projected downwards through the air from a similar round hole. I prove that, apart from molecular figurative forces, the forms of the current of gas would be completely analogous to that of the liquid vein, also considered as not subject to figurative forces. In both cases the form would
of a Liquid Mass without Weight. 359

approach to a cylinder, or rather to a very elongated cone; only one of the figures would exhibit as depressions what the other exhibited in relief. Now, considered with regard to molecular forces, the conditions of equilibrium and stability are exactly the same for hollow figures as they are for figures in relief. Hence, just as the liquid vein changes into isolated masses under the action of the molecular forces, so the current of gas ought to transform itself into separate bubbles—which, as every one knows, is conformable to experiment.

But I point out that there is this difference between them; that the liquid vein always presents a continuous portion of greater or less length, while with the current of gas, unless it has quite an enormous velocity of translation, the bubbles ought to be formed very near the opening, and hence the current cannot have a continuous part. I verified this deduction by means of air escaping under a pressure of 130 centims. of water from an opening 5 millims. in diameter, below a stratum of water only 20 millims. in thickness: this current, notwithstanding its great rapidity and its having to pass through a very shallow layer of water, caused a bubbling at the surface, thus proving that it had been already changed into bubbles.

Tenth Series.—Results arrived at by Geometricians, and experimental verifications.

Beer discussed analytically in a first memoir the rotation-experiments of my First Series; in a second memoir he came back to the same subject, treating it more precisely by the aid of elliptic functions; and he also gave by the same means the integral equation to the meridian lines of the equilibrium-figures of revolution for the case of rest. M. Delaunay, considering surfaces of revolution with a constant mean curvature from a purely mathematical point of view, arrived at an elegant method of generating their meridian lines. M. Lamarle has applied his geometrical methods to the same subject. M. Mannheim has pointed out a simple rectification of the meridian lines in question. In relation to the surfaces generated by these same lines, M. Lindelöf has arrived at a series of remarkable results, relating particularly to the measurement of areas and volumes. Goldschmidt, and more recently MM. Lindelöf and Moigno, have discussed the catenoid analytically. In conclusion, as the last result bearing specially upon equilibrium-figures of revolution, M. Lamarle has shown that, among ruled surfaces, the cylinder is the only one that has a finite and constant mean curvature.

Poisson was, I believe, the first to investigate the general differential equation of the figures of equilibrium of a liquid mass
without weight; and he accordingly thus obtained the equation to surfaces of constant mean curvature. Meusnier had pointed out the skew helicoid with directing plane as a surface whose mean curvature was nothing; M. Catalan has proved that this helicoid and the plane are the only ruled surfaces of the mean curvature zero. M. Lamarle has integrated the general equation in the case of helicoids, and has thus found four other surfaces besides the skew helicoid with directing plane. Mr. Jellett has indicated a simple condition which every closed surface of constant mean curvature, except the sphere, must satisfy.

The general case of surfaces whose mean curvature is zero was first treated by Monge and Legendre; M. Scherk has deduced, from the integral which they gave, the equations in finite coordinates of five particular surfaces. M. Ossian Bonnet has made known another more convenient integral, and has applied his method to the investigation of surfaces of the kind in question which pass through a determinate continuous contour. M. Serret has shown how they can be made to pass through a series of straight lines not situated in the same plane; and M. Mathet, by a method different from that of M. Bonnet, how they can be made to pass through a given plane curve. M. Catalan has published another integral still of the general equation, and has deduced from it several surfaces.

I also recall the researches of Dupré, Rennes, and M. Van der Mensbrugghe relating especially to the tension of liquid surfaces, researches of which I have spoken already in the Eighth Series.

Now let us see what are the experimental verifications. I have measured the limit of stability of the catenoid by means of a laminar catenoid formed between two equal rings, whose distance could be gradually varied and exactly measured; and the result was found to agree perfectly with that deduced from Goldschmidt's calculation.

I have applied the general principle which concludes my Seventh Series to the realization of the skew helicoid with directing plane, by employing a closed outline of iron wire composed of two spires of a regular helix, of a part of the axis, and of two straight lines connecting this part with the extremities of the helix. When this outline is taken out of the glycerine-liquid, it is found to be occupied by a beautiful curved film which represents exactly the helicoid in question.

I have realized in the same way in the laminar form, upon an appropriate framework, a portion of a remarkable surface first discovered by M. Scherk, and since discussed by M. Catalan.

M. Van der Mensbrugghe, by applying still the same principle, has also realized another of M. Scherk's surfaces.
I have verified a consequence of M. Bonnet's researches—namely, that an infinity of surfaces having the mean curvature zero can always pass through a closed outline, either plane or not plane, of absolutely any form. I have had the strangest and most complicated closed outlines made of iron wire, and on issuing from the glycerine-solution each of them was found to be occupied by a single film: this experiment proves, in the first place, that, given any closed outline whatever, there is always at least one surface of mean curvature zero a finite portion of which can fill it. I next show how to make the film undergo as many changes of shape as may be wished without its equilibrium being destroyed, and without its ceasing to rest upon the whole of the closed outline; but I show that it is no longer a finite portion of each of these new surfaces that occupies the given outline.

I had found by calculation that the volume of the limiting catenoid is half that of the cylinder on the same base and of the same height; and I have verified this result by means of a full catenoid of oil, formed between two disks within the alcoholic solution, and having the limiting height; the disks were then brought nearer until the liquid mass formed a cylinder; and the height of this cylinder was found to be half that of the catenoid.

Lastly, by the use of suitable solid frameworks, I have realized a portion of one of M. Lamarle's helicoids, likewise by means of oil surrounded by the alcoholic solution.

**Eleventh Series.**—Limits of stability of figures of equilibrium. General theory of the stability of these figures.—Stability of systems of films.—Stability in cases when gravity comes into play.

As might be seen from the preceding Series, the sphere is very probably the only closed figure of equilibrium, all the rest having infinite dimensions in certain directions. When the attempt is made to realize partially one of these last, either by means of oil surrounded by the alcoholic solution, or with a film of the glycerine-solution in air, it is generally found that, if the solid terminations to which the mass or the film adheres are placed so as to comprise too great a portion of the figure, the latter will not form; whence we must conclude that, with the terminations separated to this extent, it would be unstable. In the present Series I investigate, in the first place, by aid of experiment, calculation, and reasoning, the limits of stability of most of the figures of equilibrium that I have studied, and especially of the figures of revolution contained between two equal bases perpendicular to the axis.

When a sphere of oil is freely suspended in the alcoholic mixture, it always exhibits perfect stability of form. If this form is altered by movements imparted to the surrounding liquid, the mass
always resumes exactly its previous shape. A soap-bubble isolated in the air exhibits equally a permanent and stable form. The sphere, then, has no limit of stability; that is to say, whatever may be the extent, relatively to a complete sphere, of the portion of a sphere actually produced, this portion is necessarily in a state of stable equilibrium. Thus, for instance, a perfectly permanent double convex lens of oil can be produced in the alcoholic mixture upon a ring of iron wire.

This result, being independent of the radius, and consequently of the curvature of the sphere, is equally true when the radius becomes infinite, or, in other terms, when the surface of the sphere becomes a plane. Accordingly the plane also has no limit of stability; that is, it can be produced within a solid outline of any extent without ceasing to be stable, as can be verified by the formation, for instance, of a film of the glycerine-solution within a plane outline of iron wire of any shape and of any size.

My first experiments upon liquid cylinders proved that such a cylinder is unstable when the ratio of its length to its diameter exceeds a value comprised between 3 and 3·6, which I have called the limit of stability of the cylinder. I had arrived at this result by means of cylinders of oil formed within the alcoholic mixture between two solid rings or disks. In the present Series I attack the theoretical investigation of the precise value of the limit in question. Suppose one of our cylinders of oil to have been produced between two disks, and short enough to be stable. If, by gently pushing the oil towards one of the disks with a glass rod, we cause the artificial formation of a bulging and a constriction, and if this modification of the figure does not go beyond a certain limit, the mass, when afterwards left to itself, resumes spontaneously the original cylindrical form; but if the change of shape exceeds this limit, it increases spontaneously, and the transformation becomes complete—that is to say, the mass separates into two unequal portions.

Now, if we were to produce the precise degree of alteration which separates these tendencies to two opposite effects, it is evident that the mass would be indifferent to either one or the other; there would therefore in that case result a condition of equilibrium, although of unstable equilibrium. And since the figure would still be a figure of revolution, made up of an enlargement and a contraction, it would necessarily form a portion of an undulating surface or unduloid (*onduloid*). In the second place, since this partial unduloid would constitute the degree of alteration at which there would begin a spontaneous tendency to a more profound alteration, it would differ less from the initial form (namely the cylinder) in proportion as the latter was nearer to its limit of stability; and this is confirmed by experiment. Lastly, when the cylinder is
actually at this limit, the unduloid would coincide with it, or, if the expression is preferred, it would differ infinitely little from it, since the faintest trace of an enlargement and a contraction would then be sufficient to bring about the spontaneous transformation. Hence, when a cylinder of liquid is precisely at its limit of stability, we can always conceive of a partial unduloid which differs infinitely little from this cylinder, and is made up of exactly one enlargement and one contraction.

Now we have the differential equation of the first order of the meridian lines of equilibrium-figures of revolution, and in the case of an unduloid infinitely near to a cylinder, this equation can be integrated by the ordinary methods. It then gives a curve of sines \( \sin \omega \) for the meridian line; and if we try what is the sum of the lengths of the chords of a convex and of a concave arc of the curve of sines, we find that, representing this sum by \( L \), and the radius of the cylinder by \( r \), we have the relation

\[
L = 2\pi r.
\]

But the length \( L \) is evidently that of the cylinder at its limit of stability; and if it is divided by the diameter \( 2r \), it gives what I have called the limit of stability of the cylinder, which it will be seen is exactly equal to the ratio \( \pi \).

I have verified this result of calculation by new experiments more accurate than my first, and likewise performed upon cylinders of oil contained between disks and surrounded by the alcoholic mixture. Before stating their results it is necessary to make one remark. When we are not too near the limiting condition, there are two characters which clearly indicate stability or instability: if, when a cylinder has been formed approaching the limit, we produce in it artificially a slight enlargement and contraction by impelling the oil with the point of the syringe, and the figure then resumes its previous form, it is evident that it still possesses actual stability; on the other hand, if while we are trying to produce the cylinder (that is, while there is still an excess of oil and we are withdrawing some so as to arrive at the cylindrical form) the figure begins to change of its own accord before this form is reached, we must conclude that the cylinder which it is wished to produce would be unstable.

In the experiments in question, I gave in succession the following values to the ratio between the distance of the disks to their diameter:

\[
3.6, \ 3.3, \ 3.18, \ 3.14, \ 3.09, \ 3.11, \ 3.13.
\]

It will be seen that the first three ratios exceeded the limit 3.14, but that they became successively nearer and nearer to it; now, for these three ratios the above-mentioned character of instability was distinctly shown, but in a decreasing degree from the first
to the last. The last three ratios were, on the contrary, below the theoretical limit, but also successively approached it; and with them the character of stability was recognized, although likewise becoming less and less marked from one to another. Lastly, with the theoretical ratio 3.14 itself neither of these characters was exhibited: in this case, as with the lower ratios, the exact cylinder was arrived at without difficulty; but when this cylinder was left to itself, it began, after remaining apparently unaltered for a few seconds, to change, at first with extreme slowness, but afterwards gradually more and more quickly. The figure divided itself as usual into a bulging and a constricted portion, and the change of shape continued until complete separation had taken place. The results of experiment accordingly agreed completely with that of calculation.

As I proved in my Second Series, a liquid cylinder, of which the length is considerable as compared with its diameter, separates of its own accord into bulging portions alternating with constricted portions, both becoming more and more distinct until the whole figure is changed into a succession of isolated spheres. I then arrived at the following conclusions,—that a cylinder of indefinite length, with its surface entirely free, and formed of a liquid without weight and completely devoid of viscosity, would in all probability transform itself in such a way that the sum of the lengths of an enlargement and a contraction would be equal to that which corresponds to the limit of stability. But I showed at the same time that the sum of these lengths increases with the resistances, either external or internal, that retard the transformation. Now in the present Series, supposing a cylinder of indefinite length, or only of very great length, formed of a real liquid, and therefore one in which the transformation is necessarily interfered with, at least by viscosity, and assuming that at the commencement of this transformation the meridian line of the figure is still a curve of sines, I investigate mathematically the difference between the capillary pressures exerted by a contracted and those exerted by an expanded portion; and in this way I find that the excess of the former above the latter increases with the length of the contracted and expanded portions. It will thus be understood how, when there are resistances, the transformation spontaneously adjusts itself so as to overcome them, increasing the difference of the pressures by an increase in the length of the contracted and enlarged portions.

Experiment, moreover, confirms the result of the above calculation, leaving out of account any hypothesis as to the nature of the original meridian line. I have in fact established that if cylinders of oil are formed between the same pair of disks, exceeding to a greater and greater extent the limit of stability, as can
be done by help of an appropriate artifice, the transformation becomes more and more rapid. For the limiting ratio 3·14, the duration of the phenomenon was eleven minutes; with the ratio 3·18 it was only four minutes; with the ratio 3·3 it was two minutes; and with the ratio 3·6 one minute. Now it is plain that a more rapid transformation implies forces of greater intensity.

It follows, besides, from the calculation of which I have spoken, that if the ratio of the distance between the bases to their diameter is less than \( \pi \), and if we suppose one half of the figure to be slightly increased and the other half to be slightly diminished in width, the capillary pressures of the swollen portion will overcome those of the contracted portion, and so the cylindrical form tends to reproduce itself. Lastly, the same calculation shows that if the ratio is exactly equal to \( \pi \), and if the swelling and the contraction are of infinitely small amount, the capillary pressures are the same in both parts. We have accordingly here a second method which gives the precise limit of stability of the cylinder; only it assumes à priori that at the commencement of the transformation the meridian line is a curve of sines.

I next pass to the unduloid. Here the conditions of stability are different according as the middle of the figure produced is occupied by a constriction or by a bulging portion: in the former case these conditions seem to vary according as the unduloid differs more or less from a cylinder; but in the second case the limit is distinctly recognizable. A partial unduloid with a bulge at the middle is exactly at its limit of stability when its bases coincide with the circular sections of the necks of the two constrictions between which the bulging portion is contained. I draw this conclusion both from experiment and from a course of reasoning which would take up too much room for insertion here. M. Lindelöf has arrived at the same result mathematically.

Hence we get another method still by which we can rigorously determine the limit of stability of the cylinder. It is well known that M. Delaunay has proved that the meridian lines of surfaces of revolution of constant mean curvature are generated by one of the foci of a conic section which rolls upon the straight line constituting the axis of revolution. In the case of the unduloid, the rolling conic section is the ellipse; and it is plain that the portion of the described line which is comprised between any two consecutive points of minimum distance from the axis, corresponds to one complete rotation of the ellipse. Hence the partial unduloid generated by this portion (that is to say, the unduloid at its limit of stability) has a length equal to the periphery of the ellipse in question; now, when this ellipse becomes a circle, the unduloid becomes a cylinder; and consequently this,
at its limit of stability, has a length equal to the circumference of the rolling circle. But this circumference is evidently equal to that of the cylinder; hence the limiting cylinder has a length equal to its own circumference; and hence, lastly, the ratio of the length to the diameter has the precise value $\pi$.

I have already discussed the question of the limit of stability of the catenoid in my Tenth Series, and I then described experiments which fully confirm the theoretical result.

No general statement can be given of the limit of stability of the nodoid ($nodoïde$), whether the figure be generated by a portion of a node of the meridian line, or by an arc of this line convex towards the outside.

Except in the case of the cylinder, the partial figure of revolution can be contained between unequal circular bases; and then the conditions of stability are necessarily different. We have a curious instance of this in the case of the catenoid: if the circular section of the neck be taken as one of the bases, the figure has no longer any limit of stability; that is to say, the other base may be taken as far away as we please in the indefinite figure without the portion contained between the two bases ceasing to be stable. I verify this conclusion experimentally upon a catenoid of which the circle at the neck had a diameter of only 3·5 centims., while the diameter of the other base was 20 centims.

I next consider from a general point of view the question of the stability of figures of equilibrium. It is admitted by geometericians, as a result of analysis, that the surfaces represented by the equation $\frac{1}{R} + \frac{1}{R'} = C$ (that is to say, surfaces of constant mean curvature), are also those which, for a given volume, have the smallest superficial area. But if we were to accept this principle without restriction, it would follow that every partial liquid figure of equilibrium terminated by a solid system would be necessarily stable, whatever portion it might represent of the complete figure: the cylinder, for instance, would be completely stable however great its length; the unduloid likewise would remain perfectly stable whatever the number of bulging and constricted portions contained between its two solid bases, &c. In fact the superficial layer, being, as is now known, really in a state of tension, makes a constant effort to contract; hence, if its area were always a minimum in a state of equilibrium, any very small change of form would increase its area, and consequently the superficial layer would tend to resume its former dimensions and to restore the figure of equilibrium.

Geometericians have been led to the above principle by the analytical result that the variation of such surfaces is always zero,
of a Liquid Mass without Weight.

which seems necessarily to imply either a minimum or maximum of extent; and since it is evident that, with a given volume the surface can always be increased by a proper change of form, it has been concluded that a minimum ought to be chosen. Now there was an equally legitimate intermediate supposition, which has not been made, and which corresponds with the actual fact; that is, that, beyond definite limits, the surface is a minimum relatively to certain kinds of small changes of form, while it is a maximum relatively to others.

I prove the truth of this last principle by the study of the cylinder. Let us consider a liquid cylinder, terminated by two solid bases, of any given length as compared with its diameter. It is obvious, in the first place, that it may be made to undergo small modifications of form which, without changing its volume, increase its surface. This would evidently be the case, for instance, if it were to become grooved with fine longitudinal ridges and hollows, such that the sum of the ridges was equal in volume to the sum of the hollows, each being measured relatively to the original surface; and it is probable that it would also be the case for any other modification which should change the figure of revolution.

But let us suppose that the figure, without ceasing to be a figure of revolution, were to transform itself into a succession of alternately expanded and contracted portions, this change of shape being of finite but excessively small amount. Then, if we suppose the meridian line of the figure thus modified to be a curve of sines, the area of the surface of the portion composed of one expansion and one contraction can be found by calculation, the condition that the volume shall not change being always kept in view, and it is thus found that if the length of the portion in question exceeds the circumference of the original cylinder, the surface is less than that of the corresponding portion of the cylinder; and since the same result is applicable to each similar portion of the entire figure, it follows that, under these circumstances, the total surface diminishes. The surface of the cylinder is accordingly a maximum relatively to the kind of alteration we have just indicated, and it is by a change of this kind that, as we know, the spontaneous transformation of the cylinder takes place.

Thus a liquid cylinder whose length exceeds its circumference, or, in other words, in which the ratio of the length to the diameter exceeds the quantity \( \pi \), is necessarily unstable, because the constant tendency of its superficial layer to diminish its area favours the kind of change of shape which we have just been considering. I may notice in passing that the theory of the constitution of liquid veins which I have set forth at the end of my Second Series depends upon this necessity.
One point still remained to be cleared up. According to the calculation I have been speaking of, in order that the surface may be diminished by the kind of transformation here indicated, it is sufficient that the sum of the lengths of an expanded and contracted part of the figure should exceed the circumference of the cylinder; and this allows us to attribute to this sum an infinite number of different values. But nevertheless, as has been shown in the Second Series, in a cylinder that is very long relatively to its diameter, when the transformation takes place quite regularly, the sum in question is always the same for the same cylinder under the same circumstances, whence we must infer that there is some special condition which regulates the choice of the mass. We may add that analogous considerations apply to the other unstable figures. I examine the matter, and arrive at the following as a very probable conclusion;—that, among all possible changes of shape which would diminish the surface, the molecular forces choose that one which allows the smallest possible departure of the mass from another figure of equilibrium. In the cylinder, for example, the mass will assume at the beginning of the transformation the figure which, considering the resistances, makes the nearest possible approach to the unduloid.

The combination of liquid films, which I have studied particularly in my Sixth Series, likewise presents some remarkable phenomena in relation to their stability. I tried to establish experimentally that every equilibrated system of films in which more than three films meet at one liquid edge, or more than four liquid edges at one liquid point, is an unstable system. M. Lamarle has since discussed the question in detail in his memoir "On the Stability of Liquid Systems formed of thin Films." Setting out from the general principle that I had laid down at the end of my Sixth Series—namely, that in every permanent assemblage of films the sum of the areas of the films must be a minimum,—he succeeds in giving a strict demonstration of the conditions above mentioned relatively to the number of films and of liquid edges; and he arrives besides at many other interesting results.

Lastly, I recall the fact that M. Duprez, in his memoir "On a particular case of the Equilibrium of Liquids," has investigated a phenomenon in which the stability or instability of a liquid surface depends conjointly on the action of gravity and of molecular forces. The phenomenon referred to is the familiar one of the suspension of a liquid in a vertical tube open at the lower end when the diameter of the opening is below a certain limit; but M. Duprez has shown that this limit is much greater than was generally supposed. He has held up water in this way in a tube the diameter of whose opening was 19·85 millims; while
the theory founded upon the conditions of stability of a liquid surface gave him 21.13 millims. as the exact value of the limiting diameter.

I conclude by pointing out that all my Series together, counting from the second inclusively, establish the *Experimental and Theoretical Statics of Liquids acted on solely by Molecular Forces*.

The present Series is followed by an analytical index to the contents of the eleven Series.

---

**XLVII. Proceedings of Learned Societies.**

ROYAL SOCIETY.

[Continued from p. 308.]

June 16, 1870.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communications were read:

"Note on the Construction of Thermopiles." By the Earl of Rosse, F.R.S.

Although in the measurement of small quantities of radiant heat by means of the thermopile much may be done towards increasing the sensibility of the apparatus by carefully adjusting the galvanometer and rendering the needle as nearly astatic as possible, there must necessarily be some limit to this; and it therefore appears desirable that the principles on which thermopiles of great sensibility can be constructed should also be carefully attended to.

With the view of obtaining a pair of thermopiles of greater sensibility and of more equal power than I had been able to procure ready made, I made a few experiments with various forms of that instrument; and I was led to the conclusion (one which might have been foreseen) that the sensibility of the thermopile is much increased by reduction of its mass, and more especially by a diminution of the cross section of the elements.

To obtain a clear idea of the problem before us, which is how to construct the thermopile so that, with a given amount of radiant heat falling on its face, the greatest current may be sent through the galvanometer, let us consider the thermopile under two different conditions:

1. With the circuit open.

2. With the circuit complete.

In the first case, when radiant heat falls on the face of the pile, the whole mass of metal rises in temperature, the rise being greatest at the anterior face, and less and less as you approach the other end. This rise of temperature will increase till the heat radiated from the anterior face, together with that which traverses the depth of the pile and is radiated from the posterior face, is just equal to that radiated to the anterior face at that moment, or when

\[ k(t+\delta t) = kt + \frac{sc}{T} (t-\delta t) = \Omega, \]

where \((t, \delta t)\) are respectively the temperatures of the anterior and
posterior faces, $s$, $l$ the cross-section and depth of the pile, $c$ proportional to the mean conductivity of the material of the pile, $Q$ the quantity of heat falling on the pile in a unit of time, and $k$ a constant.

Let us now suppose the circuit completed, and we shall have, in addition to the above, two causes operating to reduce the temperature of the anterior face—the abstraction of heat by the electric current, and proportional to that current $= LI$, where $I$ is the intensity of the current and $L$ a constant; then there will be equilibrium when

$$k (t + t') + LI = kt + \frac{so}{l} (t - t') + LI = Q.$$ 

It is quite clear therefore that if $Q$ be constant, $I$ will become the larger the smaller the other two terms become; and therefore as long as the first term continues small compared with the remaining terms, and the resistance in the pile is very small compared with that in the rest of the circuit, we shall increase the intensity of the current by every reduction of the cross section of the elements of the thermopile.

There is another point which, though less important, cannot be entirely lost sight of—namely, that the more we reduce the mass of the anterior face and adjacent parts of the pile, the more rapidly will the temperature rise to its state of equilibrium, and therefore the more convenient will it be for use where the needle is liable to disturbances from various causes, and where consequently, the more speedily the needle can be brought to rest, the more accurately will its observed motion be a measure of the radiant heat falling at that moment on the face of the pile.

Let us now compare the case of a single pair of small cross section with a metal disk soldered to the junction of the two bars, and of sufficient size to catch all the radiant heat required to be measured, with that of a pile of $n$ pairs, each of equal dimensions with those of the single pair, the area of face being the same in the two cases.

By increasing the number of elements from one to $n$, we increase the number of solderings in that proportion; consequently the average amount of heat reaching any soldering is $\frac{1}{n}$ as great as that reaching the soldering of the single pair; therefore, if the same percentage of the total heat be lost by conduction, the total electro motive force is the same in the two cases. But inasmuch as the total cross section of metal to conduct the heat away from the anterior face is $n$ times as great in the pile as in the pair, and the resistance of the pile is $n$ times as great as that of the pair, the pile will be inferior in power to the pair, unless these two causes of inferiority are counterbalanced by the loss due to the greater average distance to the soldering from the points where the heat reaches the face, in the case of the pair, than that of the pile of $n$ pairs.

The experiments already referred to were made with three different thermoelectric pairs. These consisted each of a pair of bars of
bismuth and an alloy of twelve parts of bismuth and one part of tin of different thicknesses, of about equal lengths in each case, and soldered about $\frac{1}{4}$ inch apart, upright, on disks of sheet copper of $\frac{1}{2}$ inch diameter. A slip of wood was placed between the two bars, to protect them from injury; and to it they were fixed with thread. The three piles were compared with a pile of four elements, made by Messrs. Elliott; and, the deviation due to the latter being taken equal to unity, the following deviations were obtained for the three thermopairs:

<table>
<thead>
<tr>
<th></th>
<th>Weight of disk face.</th>
<th>Weight of two bars.</th>
<th>Deviation.</th>
<th>Metals employed.</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>8 grains</td>
<td>42 grains</td>
<td>0.676</td>
<td>Bismuth, antimony.</td>
</tr>
<tr>
<td>II</td>
<td>$4\frac{1}{2}$</td>
<td>6</td>
<td>1.35</td>
<td>Bismuth ${\text{Bismuth,}}$</td>
</tr>
<tr>
<td>III</td>
<td>$\frac{1}{2}$ grain</td>
<td>3</td>
<td>3.23</td>
<td>Bismuth ${\text{Bismuth,}}$</td>
</tr>
</tbody>
</table>

A heavy and a light pile were also compared, taking the interval between raising and depressing the screen, first $=\frac{1}{2}$ minute, and then $=2$ minutes; and it was found that, in the first case,

$$\frac{\text{Deviation due to light pair}}{\text{Deviation due to heavy pair}} = 2.6;$$

and, in the second case,

$$\frac{\text{Deviation due to light pair}}{\text{Deviation due to heavy pair}} = 2.9;$$

so that the light pair arrived rather more rapidly at the condition of equilibrium than the heavier pair.

Although the above experiments are far less complete than I could have wished, they are sufficient to show that the sensibility of thermopiles may be considerably increased by diminution of the section of the bars composing them. Whether they may be with advantage reduced to a greater extent than I have already done I cannot say; but I am inclined to think that they may. I have ascertained from Messrs. Elliott that the alloys used by them in the construction of thermopiles, at the time when I received mine from them, were 32 parts of bismuth +1 part of antimony, and $14\frac{1}{2}$ of bismuth +1 part of tin. If allowance be made for the substitution of the first of these two alloys for pure bismuth, the difference between Elliott's pile and the pairs II. & III. will be rather greater. The pile by Messrs. Elliott, if made of the same metals as I employed, would have been reduced in power from 1 to 0.9.

The construction of thermo-couples, on the plan I have described, is comparatively easy. In about two hours I was able to make one;
and in more experienced hands their construction would be still easier.

An experiment was made with one of the piles to ascertain whether, when the heat was not directed centrally on the pile, much diminution of power would take place. There was less deviation, in consequence of the increase of the mean distance which the heat had to travel before it reached the soldering; but I believe that this defect might be remedied, probably without diminution of the power of the pile, by increasing the thickness of the face and leaving the dimensions of the bars the same.

"On the Radiation of Heat from the Moon."—No. II. By the Earl of Rosse, F.R.S.

In a former communication to the Royal Society I gave a short account of some experiments on the radiation of heat from the moon, made with the three-foot reflector at Parsonstown, during the season of 1868–1869. I then showed:

1st. That the moon's heat can be detected with certainty at any time between the first and last quarter, and that, as far as could be ascertained from so imperfect a series of observations, the increase and decrease of her heat with her phases seems to be proportional to the increase and decrease of her light as deduced by calculation*.

2ndly. That a much smaller percentage of lunar than of solar rays is transmitted by a plate of glass; and we therefore infer that a large portion of the rays of high refrangibility which reach the moon from the sun do not at once leave the moon's surface, but are first absorbed, raise the temperature of the surface, and afterwards leave it as heat-rays of low refrangibility.

3rdly. That, neglecting the effect of want of transparency in our atmosphere, and assuming, in the absence of any definite information on the subject, that the radiating-power of the moon's surface is equal to that of a blackened tin vessel filled with water, the lunar surface passes through a range of 500° F. of temperature; consequently the actual range is probably considerably more.

4thly. The proportion between the intensity of sunlight and moonlight, and between the heat which comes from the sun and from the moon, as deduced from those observations, agreed as nearly as could be expected with the values found by independent methods, and for this reason might be considered the more reliable.

During the past season these observations have been continued: but much time has been spent in trying various modifications of the apparatus; and a satisfactory comparison of observations made on different nights, under different circumstances, has been impossible. However, by more numerous and more complete experiments, made alternately with and without an interposed plate of glass, the second conclusion arrived at during the previous season has been to a great extent confirmed.

The following Table gives the values found for the percentage of the moon's heat which passes through glass:

* See Phil. Mag. vol. xxxviii. p. 317.
The Earl of Rosse on Lunar Heat-Radiation.

<table>
<thead>
<tr>
<th>Date of observation</th>
<th>Distance of moon from opposition</th>
<th>Altitude of the moon</th>
<th>Percentage of moon’s heat transmitted by glass.</th>
</tr>
</thead>
<tbody>
<tr>
<td>April 15th, 1870</td>
<td>5</td>
<td>15</td>
<td>13'3</td>
</tr>
<tr>
<td></td>
<td></td>
<td>20(\frac{1}{2})</td>
<td>15'5</td>
</tr>
<tr>
<td></td>
<td></td>
<td>24</td>
<td>16'6</td>
</tr>
<tr>
<td>April 16th ..........</td>
<td>15</td>
<td>15</td>
<td>14'5</td>
</tr>
<tr>
<td></td>
<td></td>
<td>24</td>
<td>14'6</td>
</tr>
<tr>
<td>April 17th ..........</td>
<td>31</td>
<td>20(\frac{1}{2})</td>
<td>10'0</td>
</tr>
<tr>
<td></td>
<td></td>
<td>20</td>
<td>10'0</td>
</tr>
<tr>
<td>March 13th ..........</td>
<td>50</td>
<td>50</td>
<td>7'1</td>
</tr>
<tr>
<td>February 10th ......</td>
<td>66</td>
<td>44</td>
<td>8'4</td>
</tr>
<tr>
<td>February 9th ......</td>
<td>77</td>
<td>32</td>
<td>9'3</td>
</tr>
<tr>
<td>April 9th ..........</td>
<td>81</td>
<td>44</td>
<td>11'0</td>
</tr>
<tr>
<td></td>
<td></td>
<td>16</td>
<td>11'0</td>
</tr>
<tr>
<td>April 8th ..........</td>
<td>93</td>
<td>30</td>
<td>12'0</td>
</tr>
<tr>
<td>March 8th ..........</td>
<td>109</td>
<td>27</td>
<td>13'0</td>
</tr>
</tbody>
</table>

Mean = 11'88.

The same plate of glass which was used in I. and II. on April 15th, and in the experiments on the two following nights, was tested for the solar rays, and the following values of the percentage of heat transmitted were obtained:—

<table>
<thead>
<tr>
<th>Date of observation</th>
<th>Percentage of heat transmitted</th>
</tr>
</thead>
<tbody>
<tr>
<td>April 15th</td>
<td>86'2</td>
</tr>
<tr>
<td>April 18th</td>
<td>86'6</td>
</tr>
<tr>
<td>Mean on April 18th</td>
<td>86'8</td>
</tr>
</tbody>
</table>

The piece of glass used on the other occasions, instead of being placed at six or eight inches from the pile, was laid against the end of the protecting cone, or about half an inch from the face of the pile. When it was placed in this position and tested for solar rays, an increase of deviation in the proportion of 1'1 to 1 was obtained, owing to the “bottling up” of the sun’s rays as in an ordinary greenhouse, and the keeping off of currents of air.

It seems therefore to be clearly proved that there is a remarkable difference between the sun’s and the moon’s heat in regard to their power of passing through glass. The amount transmitted varies from night to night; and in the later observations the value was generally larger than in the earlier ones. Possibly this may have arisen from the formation of a slight and imperceptible film of moisture on the surface of the glass, which was much more unlikely to form during the much shorter period* of exposure to the night air in the later observations.

* About 12 minutes in place of 30 to 60 minutes.
The experiment made during the previous season to determine the ratio between the heating-power of the moon and of the sun was repeated with more care; and the value found, taking what appeared to be the most probable mean heating-power of full moon, as determined on various nights, was

\[
\frac{\text{Sun's total heat}}{\text{Moon's total heat}} = 82600.
\]

Taking the percentage of light transmitted by glass\(^*\) = 92

Do. do. of sun's heat = 87

Do. do. of moon's heat = 12

Do. do. of heat from a body at 180° F. = 1.6

If \(\frac{0}{0' + l}\) and \(\frac{l}{0' + l}\) represent respectively the percentage of dark and luminous rays present in the moon's radiant heat, and \(\frac{0'}{0' + l'}\) and \(\frac{l'}{0' + l'}\) the corresponding quantities for the sun's radiant heat, we have

\[
\frac{0 \times 0.016 + l \times 0.92}{l + 0} = 0.12,
\]

and

\[
\frac{0' \times 0.016 + l' \times 0.92}{l' + 0'} = 0.87;
\]

\[
\therefore \frac{l'}{l} = \frac{0 + l \times 854}{0' + l' \times 104} = 82600 \times \frac{854}{104} = 678300.
\]

In all the foregoing experiments on lunar radiation the quantity measured by the thermopile was the difference between the radiation from the circle of sky containing the moon's disk and that from a circle of sky of equal diameter not containing the moon's disk; we have obtained no information in reference to the absolute temperature of either the moon or the sky.

The following experiment was therefore made with the view of trying to connect the radiation of the sky with that of a body of known temperature. The deviation due to each degree (Fahrenheit) difference of temperature between a blackened tin vessel containing hot water and subtending a given angle at the pile and a similar vessel containing colder water was first ascertained; then a similar determination of that due to the difference of radiation from one of these vessels, and from a portion of sky of equal diameter, was made. The following was the result:—

* All these values, except the first, were determined by experiment for the specimen of glass employed.
If the temperature of space be really as low as is supposed, this result seems to indicate considerable opacity of our atmosphere for heat-rays of low refrangibility.

The ever varying transparency of our atmosphere has been found to be a very serious obstacle; but the much greater steadiness of the needle during the later experiments (the mean error of the last few nights' observations having been from two to three and a half per cent. only of the whole deviation*) encourages us with the hope that, by taking advantage of favourable moments, and measuring the moon’s light simultaneously with her heat, more accurate information on this subject may soon be acquired.

The observations were examined with the view of ascertaining how far the heating-power of the moon’s rays varies with her altitude. Owing to the interference of clouds, and the limited range of altitude within which the observations were made, it is hardly worth while to give the results in detail; however, I may just say that the heating-power of the moon’s rays appears to diminish with her altitude only about one-third as fast as the intensity of the solar chemical rays as ascertained by Roscoe and Thorpe.

An attempt was made to ascertain, by comparing two measurements of the moon’s light at different altitudes with two corresponding measurements of her heat, whether our atmosphere intercepts the heat-rays to a greater extent than the luminous rays. It was found that while the light was diminished with the altitude in the proportion of about 3 to 1, the heat was diminished in the proportion of about 5 to 1. In consequence, however, of much of the moon’s light and heat being intercepted by hazy clouds or condensed vapour at the lower altitude, the experiment was inconclusive as to the effect of a transparent atmosphere on the dark rays of heat.

The accompanying diagram shows the proportion between the amount of lunar heat found on various nights at various ages of the moon. There appears to be a general accordance between the variation of her radiant heat with her phase and the corresponding amount of her light as deduced by calculation.

* During the experiments of the previous season the mean error varied between 27 per cent. and 85 per cent. or more.
As far as we can judge from so few and imperfect experiments, the maximum of heat seems to be a little after full moon.
Subjoined is a Table giving the dates of the various observations, with the reference numbers corresponding to those on the diagram, and with remarks on the state of the sky.

<table>
<thead>
<tr>
<th>Number in diagram</th>
<th>Date of observation</th>
<th>Remarks</th>
</tr>
</thead>
<tbody>
<tr>
<td>I.</td>
<td>April 4th</td>
<td>No mention of cloud.</td>
</tr>
<tr>
<td>II.</td>
<td>January 8th</td>
<td>No mention of cloud.</td>
</tr>
<tr>
<td>III.</td>
<td>April 8th</td>
<td>Extremely clear sky.</td>
</tr>
<tr>
<td>IV.</td>
<td>January 9th</td>
<td>No mention of cloud.</td>
</tr>
<tr>
<td>V.</td>
<td>March 8th</td>
<td>Night by a halo.</td>
</tr>
<tr>
<td>VI.</td>
<td>April 9th</td>
<td>Sky not good; thin hazy clouds, followed later in the</td>
</tr>
<tr>
<td>VII.</td>
<td>January 10th</td>
<td>Sky not good; thin hazy clouds, followed later in the</td>
</tr>
<tr>
<td>VIII.</td>
<td>February 9th</td>
<td>Sky not good; thin hazy clouds, followed later in the</td>
</tr>
<tr>
<td>IX.</td>
<td>January 11th</td>
<td>Much wind.</td>
</tr>
<tr>
<td>X.</td>
<td>February 10th</td>
<td>No mention of clouds.</td>
</tr>
<tr>
<td>XI.</td>
<td>January 12th</td>
<td>Occasional small clouds, and rather hazy.</td>
</tr>
<tr>
<td>XII.</td>
<td>November 19th</td>
<td>Clouds producing prismatic colours round the moon.</td>
</tr>
<tr>
<td>XIII.</td>
<td>March 12th</td>
<td>Sky very clear.</td>
</tr>
<tr>
<td>XIV.</td>
<td>April 13th</td>
<td>Sky not good; fleecy clouds.</td>
</tr>
<tr>
<td>XV.</td>
<td>April 14th</td>
<td>Bad night; stopped after 10 minutes, in consequence of</td>
</tr>
<tr>
<td>XVI.</td>
<td>April 15th</td>
<td>Sky very clear.</td>
</tr>
<tr>
<td>XVII.</td>
<td>January 16th</td>
<td>Occasional clouds.</td>
</tr>
<tr>
<td>XVIII.</td>
<td>September 20th</td>
<td>Occasional clouds.</td>
</tr>
<tr>
<td>XIX.</td>
<td>February 16th</td>
<td>Sky hazy at sunset; occasional clouds.</td>
</tr>
<tr>
<td>XX.</td>
<td>April 16th</td>
<td>Sky apparently not quite so clear as on the preceding</td>
</tr>
<tr>
<td>XXI.</td>
<td>April 17th</td>
<td>Sky apparently not quite so clear as on the preceding</td>
</tr>
<tr>
<td>XXII.</td>
<td>November 22nd</td>
<td>Fog and white frost, afterwards drift.</td>
</tr>
<tr>
<td>XXIII.</td>
<td>November 23rd</td>
<td>No remark about cloud.</td>
</tr>
</tbody>
</table>

"Observations with the Great Melbourne Telescope, in a Letter to Professor Stokes." By A. Le Sueur.

Observatory, Feb. 27.

DEAR SIR,—I have little more definite to tell you with reference to the star η Argus. Thinking that a larger dispersion would be of advantage, I have had a supplementary arrangement added to the spectroscope, by means of which a direct prism may be interposed between the collimator and the usual prism.

With this increased dispersion, the red line keeps its place; the yellow one turns out to be slightly more refrangible than D.

The green lines, which, with the smaller dispersion, were very difficult, now become almost unmanageable; this would seem to throw some doubt on their reality, as mere extra dispersion should have little effect on real lines.

The direct prism being a small one, does not take in the whole of the pencil when condensed to the limits bearable by the collimator; but as the arrangements at my disposal do not in any case admit of utilizing the full condensation, the smallness of the prism has not had any material effect.

On the whole, I am now inclined to think that, with respect to Phil. Mag. S. 4. Vol. 40. No. 268. Nov. 1870. 2 C
the green lines, the appearance of the spectra is due to a character
of light somewhat similar to that of α Orionis, &c., — a spectrum of
groups of dark lines, with spaces more or less free between them,
producing the effect (when the light is not sufficient to bear a slit fine
enough for dark lines) of a spectrum crossed by bright lines.

The behaviour of the red line, however (of the blue one, being less
conspicuous, I cannot speak with so much confidence), would lead
to the already drawn inference that it is a real hydrogen line.

I have examined other stars of about the same magnitude as η Argus;
in the majority of these there is not even a suspicion of condensation
in any part of the spectrum; red stars, R Leporis for instance, give a
spectrum not dissimilar to that of η Argus; but the red line on none
of the stars examined is so conspicuous as in η.

The weather since the beginning of this year has been more fa-
vourable, so that I am able, by degrees, to increase the amount of
work done. The routine work is the review of figured nebulae; as
might be expected, the 4 feet gives views considerably different from
the C. G. H. drawings; but at present I have nothing worthy of spe-
cial mention.

The light of the nebulae, as they are taken up for general exami-
nation, is analyzed with the prism; of those which have been exa-
mined I have yet found none of which it may be certainly said that
the light is not of definite refrangibilities.

In irregular nebulae, the bright knots even, which are so distinctly
mottled as to point to a cluster condition, give out, as far as I have
yet seen, light which is monochromatic, or nearly so.

Acknowledged clusters, where discrete stars are plainly discernible,
are of course excluded. Of the nebulosity mixed up with such clus-
ters as 47 Toucan, I cannot speak with certainty; but if the light were
monochromatic, I think that (in the case particularized at least) the
brilliance would be sufficient to afford
a definite impression.

Would you call Lord Rosse’s
attention to 1477–78 (general cata-
logue) of which I enclose a diagram
from measured positions? The config-
uration differs so widely from that
given in the Philosophical Transac-
tions, that, with reference to the ro-
tation of the two nebulous stars, it
would be interesting to have the evi-
dence of any additional observations
made at Parsonstown.

From Mr. Huggins’s observations
of the nebulae in Orion, I gather that
he has seen only the three usual lines;
with a wide slit, I had lately a very
strong suspicion of a fourth line, pro-
bably G. I have not specially ex-
amined the nebulae since; but probably Mr. Huggins will be able to give confirmatory evidence.

On the night of February 1st we had a pretty brilliant auroral display. Being at work at the time, I missed part of it; but as soon as I became aware of its existence I applied the spectroscope. At moments four lines already known were easily visible, the chief line being remarkably brilliant. A much narrower slit than that used could have been borne at the time of maximum display, which, however, lasted only a few moments. I was intent on measuring the lines, as at the time I had no published definite information with reference to other than Angström’s special line; but at moments light was seen at the red end of the spectrum sufficiently bright to leave a distinct impression of colour; when, however, special attention was devoted to that part of the spectrum the aurora had greatly diminished in brilliancy, so that I was unable to make out whether a red line existed, or whether there was a general spectrum at the red end. I incline to the latter opinion, and put it down to the rose-coloured arc; this arc, however, which seemed pretty brilliant after the streamers had disappeared, did not then give a visible spectrum. Probably this phenomenon has been observed before to better purpose; but I cannot find mention thereof in published accounts.

Yours truly,

A. Le Sueur.

"Chemical and Physiological Experiments on Living Cinchona." By J. Broughton, B.Sc., F.C.S., Chemist to the Cinchona Plantations of the Madras Government.

The memoir describes the principal scientific results which have been obtained during the last three years, in the course of chemical work on the Neilgherry Cinchona Plantations.

The chemical characteristics of the various parts of the Cinchona plant are described. The condition in which the alkaloids are met with in the living bark is shown to be that of a slightly soluble tan-nate existing in the parenchymatous cells.

The order of formation of the alkaloids is shown to be, 1st, un-crystallizable quinine; 2nd, crystallizable quinine; 3rd, cinchoni-dine and cinchonine. Reasons are adduced for thinking that the alkaloids are really formed in the tissues in which they are found.

The effect of the solar rays falling on the bark, either while living on the tree or when separated, is shown to be prejudicial to its contained alkaloids. The effect of shielding the bark artificially, and the influence of elevation of the site of growth, are detailed.

The question as to whether the alkaloids are substitutes for the mineral bases is discussed, and a series of experiments is described, which combine to show either that such substitution does not take place, or does so only in a very partial degree.
April 13th, 1870.—Sir P. de Malpas Grey Egerton, Bart., M.P.,
F.R.S., Vice-President, in the Chair.

The following communications were read:—

1. A letter from Dr. Gerard Krefft, dated Sydney, 29th January,
1869, accompanying a model of the left lower incisor of Thylacoleo
carnifex, Owen, and the original fragment from which the model
was made. Dr. Krefft also referred to the fossil remains of Herbi-
vorous Marsupials in the Museum at Sydney, which included, ac-
cording to him, besides a great number of Wombats (Phascolomys),
many wombat-like Kangaroos or Wallabies (Halmaturus). He
proposed to divide the Kangaroos into the following groups:—

(1) Macroopus, dentition as in Macroopus major.
(2) Halmaturus, with the premolar permanent, divided into two
subgroups:—

a. True Wallabies, with the premolars long, narrow, and com-
pressed, and the rami of the lower jaw but slightly anchy-
losed.
b. Wombat-like Wallabies, with the premolars compact,
rounded, and molar-like, and the rami of the lower jaw
firmly anchylosed.

Illustrative sketches and photographs accompanied this paper.

2. “On the Fossil Remains of Mammals found in China.” By
Prof. Owen, LL.D., F.R.S., F.G.S.

The specimens of teeth described by the author were obtained by
Robert Swinhoe, Esq., late H. M. Consul at Formosa, chiefly by
purchase in the apothecary’s shops at Shanghai. They included two
new species of Stegodon (named S. sinensis and S. orientalis), a new
Hyæna (H. sinensis), a new Tapir (Tapirus sinensis), a new Rhino-
ceros (R. sinensis), and a species of Kaup’s genus Chalicotherium
(C. sinense). The author remarked that the whole of these teeth
presented an agreement in colour, chemical condition, and matrix
which led to the conclusion that all belonged to the same period.
But for the presence of the Chalicotherium, they would have been
referred either to the Upper Pliocene or to the Postpliocene period.
The author did not consider that the occurrence of one Anoplothe-
rioid species need affect the determination of the age of these fossils,
especially as Chalicotherium departs in some respects from the type
genus Anoplotherium, and is not known from deposits older than the
Miocene.
3. “Further discovery of the Fossil Elephants of Malta.” By Dr. A. A. Caruana. Communicated by Dr. A. Leith Adams, F.G.S.

The author described a new locality in Malta in which the remains of Elephants had been found recently—the Is-Shantiiin fissure at the entrance of Micabibba. It was filled with a compact deposit of red earth containing fragments of limestone, many teeth and fragments of bones of Elephants, associated with bones of large birds. The author found three small shark’s teeth, and a small tooth which he regarded as belonging to *Hippopotamus*. He indicated the nature of the teeth and bones of Elephants found by him in the newly discovered fissure. The whole of the five localities in which ossiferous fissures have been discovered are in the same part of the island; and the author concluded with some remarks upon the geological conditions under which the remains of mammalia must have been accumulated, and upon the probability that a connexion then existed between Malta and Africa.

In a note appended to the paper Dr. A. Leith Adams stated that the supposed tooth of *Hippopotamus* was a germ true molar of one of the pigmy elephants, and that the Shark’s teeth have probably been derived from the Miocene deposits.

April 27th, 1870.—R. A. C. Godwin-Austen, Esq., F.R.S.,
Vice-President, in the Chair.

The following communications were read:—

1. “On the Species of *Rhinoceros* whose remains were discovered in a Fissure-cavern at Oreston in 1816.” By George Busk, Esq., F.R.S., F.G.S.

The object of this paper was to show that the Rhinoceros whose remains were discovered by Mr. Whidbey in a fissure-cavern at Oreston, near Plymouth, in the year 1816, and described by Sir Everard Home in the ‘Philosophical Transactions’ for 1817, belonged, not as has hitherto been supposed by every one except the late Dr. Falconer, to *Rhinoceros tichorhinus*, but to *Rh. leptorhinus*, Cuv. (*R. megahrinus*, Christ.).

The remains in question are in the Museum of the Royal College of Surgeons, and consist of between thirty and forty more or less broken portions of the teeth and of numerous bones of the skeleton. The greater number being hardly in a condition to afford satisfactory diagnostic specific characters, the remarks in the paper were limited to the teeth and to a perfect metacarpal bone, which appeared amply sufficient for the purpose.

The teeth mainly relied upon were the first or second upper molars (*m₁* or *m₂*) of the right and left sides. Both the teeth were broken, but what was wanting in one was supplied by the other. The characters exhibited were shown to be unlike those of *R. tichorhinus*, and quite in accordance with those of *R. leptorhinus*. These were the thinness and smoothness of the enamel, the configuration of the dorsal surface, the form and size of the columns, and the dis-
position and relations of the "uncus" and "pecten" ("crochet") and "anterior combing-plate," and the consequent absence of the characteristic "tichorhine pit" or fossette. The less strongly marked characters by which the teeth could be distinguished from those of R. hemitoechus, Falc., and R. etruscus, Falc., were also pointed out.

The metacarpal bone selected for the illustration of the diagnosis is 9\(\frac{1}{4}\) inches long, and remarkable for the compression of the shaft and its comparatively slenderess, as contrasted with the same bone in R. tichorinus, specimens of which were exhibited on the table, and which, in no case within the author's knowledge, ever exceeds 7\(\frac{1}{2}\) or 8 inches in length, and is proportionally much thicker than in R. leptorhinus or any other extinct species. The size and form of the bone also showed that the species could not be either R. hemitoechus or R. etruscus; for although the means of direct comparison with the third metacarpal of those species did not, to the author's knowledge, exist in London, its probable general dimensions and proportions could be deduced from those of the corresponding metatarsal, of which bone numerous specimens were available. It was further shown that the Oreston metacarpal exactly corresponded with those of R. leptorhinus, from Grays Thurrock, in the British Museum.

The determination of the species appears to be of considerable interest, inasmuch as it affords an additional instance of the occurrence in England of the great southern Rhinoceros. This is also the only example of the discovery of that species, except in river or other deposits, either in this country or on the Continent.

2. "On two Gneissoid series in Nova Scotia and New Brunswick, supposed to be the equivalents of the Huronian (Cambrian) and Laurentian." By H. Youle Hind, Esq., M.A.

This paper described the relations of two gneissoid series in Nova Scotia and New Brunswick, which have hitherto been regarded as intrusive granites and syenites, and have been thus represented on the published geological maps of those provinces. The author considered that these gneisses were in the main of Laurentian age, the Huronian or Cambrian rocks occurring only in patches over a vast area of Laurentian porphyroid gneiss.

The old gneiss was stated to be brought to the surface by three great undulations between the Atlantic coast of Nova Scotia and the Laurentian axis of America north of the St. Lawrence. These axes were rudely parallel to one another; and in the troughs which lay between them the Silurian, Devonian, and Carboniferous series occurred in regular sequence, the New Brunswick Coal-field occupying the central trough. On the line of section, in the troughs to the north-west and south-east, the Lower Carboniferous was stated to be the highest rock series which has escaped denudation.

The gold-bearing rocks of Nova Scotia are of Lower-Silurian age, and rest either on Huronian strata or, where these had been removed by denudation, on the old Laurentian gneiss. The gold is
found chiefly in beds of auriferous quartz of contemporaneous age with the slates and quartzites composing the mass of the series, which, in Nova Scotia, is 12,000 feet thick; and the auriferous beds are worked, in one district or another, through a vertical space of 6000 feet. Besides auriferous beds of quartz, intercalated beds and true veins are found to yield gold, and are worked.

A series of sharp and well-defined anticlinals ridge the province of Nova Scotia from east to west, while another series of low broad anticlinals of much later date have a meridional course. At the intersection of these anticlinals the gold-districts are situated, because there denudation has best exposed the upturned edges of the auriferous beds of quartz, and rendered them accessible, sometimes exposing also the underlying gneiss. Plans of Waverley and Shebrooke gold-districts were exhibited, showing the outcrop of the edges of the slates and auriferous beds of quartz in semielliptical forms, with the gneiss at the base of the ellipse. On this ground it was suggested that a correct mapping of the gneisses of Nova Scotia would have an important influence on the development of the mineral resources of the province.

A plan of some of the lodes in the Waverley gold-district showed the result of operations in 1869, subsequently to the publication of a geological map and sections of the district furnished to the Department of Mines by the author in 1868. Citations were made from the annual reports just issued of the Chief Commissioner of Mines and of the Inspector of Mines, confirming the correctness of the author's plans exhibiting the geological structure of Waverley, which is a type of all the Nova Scotian gold-districts.

May 11, 1870.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

The following communications were read:


(1) The author first described a specimen of Asaphus platycephalus, of which not only was the hypostome preserved in situ, but also the remains were more or less well preserved of eight pairs of legs, corresponding with the eight segments of the thorax, to the underside of which they had been attached. The appendages take their rise close to the central axis of each segment; and all curve forwards, and are thus most probably ambulatory rather than natatory feet. They appear to have had four or five articulations in each leg.

Three small ovate tubercles on the pygidium may perhaps indicate the processes by which the respiratory feet were attached.

Mr. Billings referred to the large number of Tribolites which have been examined, and expressed his belief that only the most perfectly
preserved specimens are likely to have the organs on the underside preserved.

(2) Mr. Billings next described the doublure or pleura in the Tertiary trilobites, comparing it to that of _Limulus_. He then proceeded to describe a row of small scars and tubercles on the underside of the pleura, to which both Dr. Volborth and Dr. Eichwald believed soft swimming feet or hard horny legs had been attached. As these were first seen by Dr. Pander in a Russian Trilobite, Mr. Billings has called them "Panderian organs." He thinks, soft natatory appendages may have been attached to these scars.

(3) Mr. Billings directed attention to the Protichnites and Climactichnites, which he thinks may now be referred to _Crustacea_, belonging to the division Trilobita.

(4) Finally, Mr. Billings described a section of a rolled-up _Calymeria senaria_, the interior cavity of which appears to be full of minute ovate bodies, from 1/20 to 1/10 of an inch in diameter. These small ovate bodies the author believes to be eggs.

2. "Note on the palpus and other appendages of _Asaphus_, from the Trenton Limestone, in the British Museum." By Henry Woodward, Esq., F.G.S., F.Z.S.

Mr. Woodward, when comparing the Trilobite sent over by Mr. Billings with specimens in the British Museum, presented by Dr. J. J. Bigsby, F.R.S., discovered, upon the eroded upper surface of one of these, not only the hypostome exposed to view, but also three pairs of appendages, and what he believes to be the palpus of one of the maxillae. This furnishes an additional fact to Mr. Billings's most interesting discovery, besides confirming its correctness.

Mr. Woodward considers the so-called "Panderian organs" to be only the fulcral points upon which the pleurae move, and showed that such structures exist in most recent _Crustacea_.

He considered that the evidence tended to place the Trilobita near to, if not in, the Isopoda Normalia.

He remarked that the prominence of the hypostome reminded one strongly of that organ in _Asaphus_, and suggested that we might fairly expect to find that the Trilobita represented a more generalized type of structure than their representatives at the present day, the modern Isopoda.


The object of this paper was to illustrate the structure and affinities of the genera above named, more especially with reference to the author's previous papers on the "Structures in Coal" and the "Conditions of Accumulation of Coal," and to furnish new facts and conclusions as to the affinities of these plants.

With reference to _Sigillaria_, a remarkably perfect specimen of the
axis of a plant of this genus, from the Coal-field of Nova Scotia, was described as having a transversely laminated pith of the *Sternbergia* type, a cylinder of woody tissue, scalariform internally and reticulated or discigenous externally, the tissues much resembling those of Cycads. Medullary rays were apparent in this cylinder; and it was traversed by obliquely radiating bundles of scalariform vessels or fibres proceeding to the leaves. Other specimens were adduced to show that the species having this kind of axis had a thick outer bark of elongated or prosenchymatous cells. The author stated that Prof. Williamson had enabled him to examine stems found in the Lancashire Coal-field, of the type of Binney’s *Sigillaria vascularis*, which differed in some important points of structure from his specimens, and that another specimen, externally marked like *Sigillaria*, had been shown by Mr. Carruthers to be more akin to *Lepidodendron* in structure. These specimens, as well as the *Sigillaria elegans* illustrated by Brongniart, probably represented other types of Sigillarioid trees; and it is not improbable that the genus *Sigillaria*, as usually understood, really includes several distinct generic forms. The author had recognized six generic forms in a previous paper and in his “Acadian Geology;” but the type described in the present paper was that which appeared to predominate in the fossil Sigillarian forests of Nova Scotia, and also in the mineral charcoal of the coal-beds. This was illustrated by descriptions of structures occurring in erect and prostrate *Sigillariae*, on the surface of *Sternbergia*-casts, and in the coal itself.

The erect *Calamites* of the coal formation of Nova Scotia illustrate in a remarkable manner the exterior surface of the stems of these plants, their foliage, their rhizomata, their roots, and their habit of growth. Their affinities were evidently with Equisetaceae, as Brongniart and others had maintained, and as Carruthers and Schimper had recently illustrated. The internal structure of these plants, as shown by some specimens collected by Mr. Butterworth, of Manchester, and soon to be published by Prof. Williamson, showed that the stems were more advanced in structure than those of modern *Equiseta*, and enabled the author to explain the various appearances presented by these plants when the external surface is preserved, wholly or in part, and when a cast of the internal cavity alone remains. It was further shown that the leaves of the ordinary Calamites are linear, angular, and transversely wrinkled, and different from those of the *Asterophyllites* properly so called, though some species, as *A. comosus*, Lindley, are leaves of Calamites.

The *Calamodendron*, as described by Cotta, Binney, and others, and further illustrated by specimens from Nova Scotia and by several interesting and undescribed forms in the collection of Prof. Williamson, are similar in general plan of structure to the Calamites, but much more woody plants—and if allied to Equisetaceae, are greatly more advanced in the structure of the stem than the modern representatives of that order. Specimens in the collection of Prof. Williamson show forms intermediate between Calamites and

*and Affinities of Sigillaria, Calamites, and Calamodendron.* 385
Calamodendron, so that possibly both may be included in one family; but much further information on this subject is required. The tissues of the higher Calamodendron are similar to those of Gymnosperous plants. The wood or vascular matter of the thin-walled Calamites consists of multiporous cells or vessels, in such species as have been examined.

In conclusion, a Table was exhibited showing the affinities of Sigillaria, on the one hand, through Clathraria and Syringodendron with Lycopodiaceae, and, on the other hand, through Calamodendron with Equisetaceae; while in another direction they presented links of connexion with Cycads and Conifers.


The author referred to a previous paper on the Upper Silurian rocks of Nova Scotia, which he stated appeared to him now to be generally repetitions of his Arisaig series. He noticed the occurrence of fossils in one of the beds previously supposed to be almost destitute of organic remains, and described the occurrence, in Arisaig township, of a band of crystalline rocks which appeared to contain Eozoon and were probably of Laurentian age. A note from Prof. Rupert Jones, giving an account of the fossils referred to by Dr. Honeyman, was also read.

May 25th, 1870.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Contributions to a knowledge of the Newer Tertiaries of Suffolk and their Fauna." By E. Ray Lankester, Esq., B.A.

(1) The Suffolk Bone-bed and the Norfolk Stone-bed.—The author pointed out that the recognition of the distinction of these two deposits from the overlying shelly crags was an important step in the determination of the history of these beds. He combated the notion that the Bone-bed and Stone-bed were identical in their contents, and especially dwelt on the differences of the mammalian fauna found in the two. The late Dr. Falconer's views, hitherto prevalent, consisting in regarding the fauna of the Suffolk Bone-bed, Norfolk Stone-bed, and Forest-bed as all of one and the same history and extent, he most strongly opposed. Rhinoceros Schleiermacheri, Tapirus priscus, Hipparion, Hyæna antiqua, and a well-defined Miocene Mastodon (Fauna 1) had been found in the Bone-bed below the Suffolk Crag—the first three in some abundance, but never in the Stone-bed or Forest-bed of Norfolk. They belonged to a different fauna from that indicated by the other mammals common to the Bone-bed and Stone-bed (Fauna 2), viz. Mastodon arvernensis, Equus sp., and certain forms of Cervus (studied by Mr. Boyd Dawkins). On the other hand, the Elephas meridionalis (Fauna 3), occurring in the Norfolk Stone-bed and in the Forest-bed, had neve,
been found in the Suffolk Bone-bed. Mr. Lankester suggested that the association of the first two of these three groups of mammals in Suffolk, and of the second two in Norfolk, might be explained by the hypothesis that they succeeded one another in time, the first (late Miocene) being confined to Suffolk, and dating from before the Diestien period, since he had obtained a Mastodon tooth of the *M. tapiroides* form enclosed in a Diestien box-stone, the third having existed in Norfolk at a period subsequent to the Coralline Crag, but before the Norwich Crag was deposited, chiefly represented in the lower part of the Forest-bed, but also in the Stone-bed, whilst the second group of mammals had existed in both areas at an intermediate period. Mr. Lankester maintained that this was the explanation suited to the facts as they at present stand, and considered that the question was not one to be shirked. All geological inferences from paleontology rest on what is called negative evidence, and hypotheses must be used in investigation. It was shown that the London clay had contributed very little indeed to the number of mammalian remains found in the Suffolk Bone-bed. Six teeth of *Coryphodon* and four fragments of *Hyracotherium* were all that could be found in the various collections.

(2) The Suffolk Box-stones.—These nodules the author had previously described as being the remains of a deposit approximately similar to the Diestien or Black Crag of Antwerp, which had preceded the Coralline Crag in Suffolk. An enlarged list of remains of Mollusca from these nodules was given, and a large series of specimens collected by the author was presented to the Society's Museum. It was from the Diestien beds, containing *Conus Dujardini*, *Voluta auris-leporis*, *Isocardia lunulata*, &c., that the Cetacean remains of the Suffolk Bone-bed were derived.

(3) A new Ziphioid Cetacean from the Bone-bed of Suffolk.—The rostrum (described in detail) was in the collection of the Ipswich Museum. It indicated a Cetacean of the genus *Choneziphius*, differing from *C. planirostris* of Cuvier and *C. Cuvieri* (of Prof. Owen's recent Monograph) in having a solid projecting apex to the rostrum, and no trace of a bifid structure. Mr. Lankester had recently carefully examined Cuvier's original specimens in Paris, and suggested that possibly *Choneziphius planirostris*, *C. Cuvieri*, this, and two other Antwerp specimens are but varieties of one species, according to age and sex. This form, however, was noted as *Choneziphius Packardi*.

(4) A new Mastodon from the Suffolk Bone-bed.—A Mastodon tooth, enclosed in Diestien matrix, and indicating a form with open and clear valleys, had been obtained by Mr. Baker of Woodbridge, and noticed by the author a year ago. He had since, in various collections, detected eight other fragments of a Mastodon, very distinct from *M. arvernensis*, and approaching *M. tapiroides*. Possibly the fragments indicated more than one such distinct species. The condition of these specimens and other evidence tended to associate them with the *Rhinoceros Schlieermacheri*, *Hipparion*, &c. forming
a fauna quite distinct from and older than that which was indicated by *Mastodon arvernensis*.

(5) List of Mammalian Fossils of the Suffolk Bone-bed, with reference to Collections containing them, and Number of Specimens.—The object of this list was to furnish an idea of the actual and relative abundance of the various mammalia, and to afford those interested in the matter information as to the much-scattered materials in private collections.

2. "Notes on an Ancient Boulder-clay of Natal." By Dr. Sutherland, Surveyor-General of the Colony.

The author described the extensive occurrence in Natal of a formation which he considered to present the essential characters of Mr. Bain's "claystone porphyry." It consisted of a greyish-blue argillaceous matrix, containing fragments of Granite, Gneiss, Greenstone, Clayslate, &c., often of large size, exhibited ripple-markings in some places, and in others showed a rude approach to wavy stratification. It rested generally upon Old Silurian Sandstones, the upper surface of which was often deeply grooved and striated. The author regarded this deposit as an ancient Boulder-clay, perhaps of Permian age.


The author described the position of Wastdale Crag and the general distribution of the blocks of granite derived from it, and discussed the hypotheses which have been proposed to account for this distribution, and especially to explain how the blocks could have been transported to the eastern side of the elevated ground of Stainmoor. The author considered that neither of the extant hypotheses, accounting for the transport of these blocks by the agency of a glacier or of icebergs, was tenable; and he indicated what he regarded as the chief objections to each of them. He suggested that their transport had been effected by the agency of coast-ice, the land being depressed to the extent of about 1500 feet, which would leave the Wastdale Crag sufficiently exposed to atmospheric action to enable it to furnish the blocks; the icefloes, serving as rafts, would suffice to convey the blocks to other parts of the coast, whilst they would not require any great depth of water to float them.
XLVIII. *Intelligence and Miscellaneous Articles.*

**NOTE ON SPIRAL NEBULÆ.** BY T. S. ALDIS, M.A.

The following are some points connected with nebulae which I have not seen noticed.

The spiral structure can, of course, only be seen when we view a nebula nearly perpendicularly to its plane of rotation.

The nebulae we see are selected. All those composed of denser substances have long since compacted into stars. Those which are left are of small density; consequently motion in them will be slow. It may be noticed in passing, that to know the density of a nebula of known shape by the period of an outlying mass, the parallax of the nebula is not needed. At twice the distance, on twice the scale, with the same density the period is not altered.

Now in our own system we have reason to believe* that the planets thrown off the denser nebula (viz. the interior planets) were thrown off in comparatively compact portions; the planets thrown off when the nebula was rare (viz. the exterior planets) were thrown off in crescent-shaped masses or ansæ, extending round some considerable portion of the interior mass. It is in such crescent shapes that we find the outer portions of the nebulae now seen. The crescent shape, then, of the detached body is probably a further criterion of their low density, confirming our expectation that the motion will be slow and therefore not easily detected.

The spiral formation itself is easily explained. The portion left behind by the contracting nebula is of a crescent shape, the concavity embracing the central mass. As this cools, the interval between the two increases, whilst the outer slowly pursues its now free orbit. If it underwent no further change it would rotate in its periodic time, keeping its concavity constantly towards the central nebula. But it, as well as the nebula, still contracts, and in so doing acquires increased speed of rotation, and thus the foremost cusp will sweep round slowly into the central body, the hinder cusp will sweep out from it. In this way a spiral arrangement of the different detached portions will arise. Of course the foremost cusp, after swinging round a considerable angle, will fall foul of the central mass and be reabsorbed in it, and the hinder cusp, too, will merge in outer masses; so that masses which have rotated considerably will not appear as elongated masses lying across the coils of the spiral, being shorn of their length in the process, and thus the nebula will take its complexion from the coils which are in the earlier stages of development.

Manchester Free Grammar School,
October 11, 1870.

* See the paper on the Nebular Hypothesis in the Phil. Mag. Oct. 1869,
ON THE MOLECULAR THEORY AND LAWS OF ELECTRICITY.
BY L. LORENZ OF COPENHAGEN.

In a paper on the mechanical theory of heat*, M. A. Dupré has given a lower limit for the number \( N \) of molecules contained in a milligramme of water, namely

\[ N > 125 \times 10^{18}; \]

that is, that the number is greater than 125 trillions. A similar though somewhat higher limit may, I think, be deduced in an entirely different and, as I think, very simple manner.

For measuring the intensity of an electrical current I will choose the electromagnetic unit, and as unit of the quantity of electricity that which passes in a second through the section of a conductor when its intensity is equal to unity. I take, as electricity which has passed through, the sum of the positive quantities passing in the direction of the current, and of the negative in the opposite direction. The repulsion, \( F \), of two electrical bodies with the quantities of electricity \( e \) and \( e' \) and the distance \( r \) is then expressed in absolute units by

\[ F = \frac{a^2 ee'}{r^2}, \]

in which

\[ a = 31074 \times 10^7. \]

Further, let the electrical tension of a body be that quantity which is required to impart to the body the unit of the quantity of electricity. If \( p \) is the electrical tension, \( e \) the quantity of electricity, \( pde \) is the work required for the communication of the electricity \( de \), and the whole work \( A \) of the tension \( p \) and the corresponding quantity \( e \) is determined by

\[ A = \int_0^e pde. \]

If, for instance, the quantity \( e \) is uniformly distributed upon the surface of a sphere whose radius is \( r \), the tension is

\[ p = \frac{e^2}{r}, \]

and the work represented by the electricity is

\[ A = \frac{e^2}{2r} = \frac{pe}{2}. \]

The decomposition of a milligramme of water requires an amount

of work which can be exerted by a certain quantity of electricity; for the tension is diminished by a certain amount. According to Weber and others, a milligramme of water is decomposed by 107 units of current in a second, and therefore by 107 units of electricity. If we denote by $N$ the number of molecules contained in a milligramme, and by $e$ the quantity of electricity which each must receive and give up in order to be decomposed, then

$$ Ne = 107. $$

Further, according to Bosscha, the electromotive force of a Daniell's element in electromagnetic units (or, what is the same thing, the tension of the positive pole of such an element whose negative pole is connected with the earth) is equal to

$$ 10258 \times 10^7, $$
a number which may also be deduced from Favre and Silbermann's experiments. The decomposition of water requires a tension 1.46 as great; if this is denoted by $P$, we have

$$ P = 15 \times 10^{10}. $$

Let us suppose that the molecule of water has a tetrahedral arrangement, then

$$ N = \frac{\sqrt{2}}{\delta^3}, $$
in which $\delta$ is the distance of two adjacent molecules of water, and the quantity of electricity $e$ which a molecule has received must somehow have spread inside a spherical surface the diameter of which is $\delta$. The work $e$ corresponding to this quantity of electricity will have now its smallest value if the electricity is uniformly diffused over the surface of this sphere; for any other arrangement of the electricity would require an increased work. The tension corresponding to this order will be

$$ \frac{2a^2e}{\delta}; $$

and this magnitude must therefore be smaller than the actual tension $P$ resulting from another distribution of the same quantity of electricity. Thus we get

$$ \frac{2a^2e}{\delta} < P. $$

From these equations we find

$$ N < 1360 \times 10^{18} \text{ and } \delta < \frac{1}{10^7} \text{ millim.} $$
This limit to the number sought is eleven times as great, and that for the distance \( \delta \) half as great as that found by Dupré. We readily see moreover that the limit for \( N \) may be put higher, if we assume that the quantity of electricity \( e \) is distributed on the surface of each of the atoms of which a molecule of water consists. The same result is obtained from the consideration of other more easily decomposable bodies; and the same calculation made for oxide of silver \((\text{AgO})\) shows that the limit for \( N \) may be put twenty-seven times as great, and that for \( \delta \) one-third as great, if it be assumed that the molecule of silver oxide contains two atoms, and that an equivalent of silver oxide consists of the same number of molecules as an equivalent of water.

It seems remarkable also that while the quantity of electricity \( e \) of a source of electricity \( P \) performs the work \( Pe \), in a molecule with the same quantity of electricity \( e \) and the same tension \( P \) only half this quantity of work (that is, \( \frac{1}{2}Pe \)) is present as work when it passes from the tension \( P \) to 0. It is possible, therefore, that the amount of the work of the electricity may disappear for chemical action to the extent of one half to occur in another form (as heat).—Poggendorff’s *Annalen*, No. 8, 1870.

---

**EASY PREPARATION OF A LIQUID FOR PRODUCING PLATEAU’S FIGURES.** BY RUDOLPH BÖTTEGER.

For producing these figures, as well as for the formation of soap-bubbles which last for hours and have the most magnificent play of colours, a liquid may advantageously be used which is readily and quickly prepared in the following manner.

In a pretty large flask parings of *palm-oil soap* are placed along with *cold* distilled water, and a solution as saturated as possible prepared by constant agitation. This is filtered through porous grey paper, and is mixed with about a third of its volume of chemically pure concentrated *glycerine*. Each time before using, it is convenient to agitate it gently. By the aid of a small glass funnel about 2 inches in diameter, provided with an india-rubber tube, soap-bubbles of unusual permanence and continually varying splendour of colour may be prepared, provided that immediately after their production they are carefully deposited upon a slightly oxidized iron ring moistened with the soap solution in question. Bubbles of 1 foot diameter, and more, last, when suitably protected against agitation and draught, frequently for five, or even ten minutes; others of 2 or 3 inches diameter for hours, in most cases as long as ten, sometimes twenty hours.—Poggendorff’s *Annalen*, No. 8, 1870.
XLIX. Researches on the Magnetic Rotatory Polarization of Liquids. By Professor A. De la Rive*.

The present researches are a continuation of those I communicated to the Society, and published, in 1868†. In my first memoir I studied the phenomenon of magnetic rotatory polarization in itself, and investigated the causes which exercise an influence on its production and intensity. I showed that the actions which permanently modify the molecular constitution of a solid body (such, for example, as the transmission of a powerful electrical discharge) modify in an equal degree its magnetic rotatory power; while in the case of liquids this power undergoes no modification, whether they are acted upon mechanically or by an electrical discharge or an electric current. I called attention to the influence which seems to be exercised upon the intensity of the magnetic rotatory power by the density of the substance submitted to the action of the magnet, even when this element is not exclusively predominant—citing as an example the considerable magnetic rotatory power (the greatest known) possessed by thallic alcohol, the density of which is enormous (sp. gr. 3.55).

The new researches just terminated have been made upon liquids only; for solids present the inconvenience of a molecular constitution too much varying from one to another to conduct us to any general results; and elastic fluids do not possess sufficient density to render the phenomena in question sensible.

* Translated from a separate copy communicated by the Author, having been read before the Société de Physique et d'Histoire Naturelle de Genève, June 2, 1870.
The present memoir consists of five sections.

In the first I describe the apparatus and the experimental processes I have used.

In the second I give the results furnished by experiment in operating upon a certain number of liquids very different one from another.

In the third I study the influence of variation of temperature on the magnetic rotatory polarization of liquids.

In the fourth I endeavour to determine the ratio which exists between the magnetic rotatory power of a mixture of two liquids and those of the ingredients of which the mixture consists.

In the fifth I exhibit the result of the experiments I have made upon the magnetic rotatory power of some isomeric liquids.

§ 1. Description of the Apparatus and the Experimental Processes.

The electromagnet which I used is composed of two cylinders of soft iron of 12 centims. diameter and 36 centims. long, each with a cylindrical perforation through its axis 3 centims. in diameter, and the two wrapped round with 1665½ metres of insulated copper wire of 2·7 millims. diameter and the total weight of 92½ kilogrammes. The wire encircling one of the cylinders is 835·8 metres long, and makes 1555 turns, forming 16 superposed layers; the other is 829·7 metres long, and makes 1537 turns, forming also 16 superposed layers. The two cylinders of soft iron, thus enveloped, are arranged opposite to each other on a cast-iron stand, so that their axes are in the same horizontal line. A screw, moved by a crank, brings into contact the two interior polar surfaces, or separates them to a maximum distance of 30 centims. A prismatic bar of soft iron, terminated by two shorter bars perpendicular to it, which are 10 centims. wide and 3 centims. thick, serves as an armature connecting the extremities or exterior polar surfaces of the two cylinders. It can slide in such a manner as not to hinder the approach or separation of the interior polar surfaces*. Strong pressure-screws keep in place, at the desired distance, the two cylinders, which, without this precaution, would be liable to rush together at the moment of magnetization and to break the objects placed between the polar surfaces.

For the experiments on rotatory polarization, on the side nearest to the source of light a Nicol's prism is fitted to the opening of the perforation of the nearest cylinder, to serve as polarizer; a similar analyzing-prism is placed at the extremity of the perforation of the other cylinder of soft iron. This extremity is

* The addition of this armature augments the force of the electromagnet in the ratio of 3 : 2.
furnished with a telescope, to which the eye is applied to receive the polarized ray, and with a divided circle which moves with the analyzing-prism and enables one to appreciate to \( \theta \) the angle through which the prism is turned. The substance to be operated on is placed between the two poles in such a manner as to be in the path of the polarized ray and, consequently, traversed by it. We then determine the azimuth of the angle which gives the passage-tint when the electromagnet is magnetized by passing an electric current through the wires which encircle it. Afterwards, by means of a commutator, the direction of the current is reversed, and a second passage-tint is obtained, distant from the first by a certain angle, the azimuth of which is likewise determined. The angle through which the analyzing-prism had to be turned in order to pass from the one passage-tint to the other represents double the rotation of the plane of polarization. This is the angle I have always measured, and which, for the sake of brevity, \( \theta \) call the angle of rotation. After a little practice the operation is effected very quickly and accurately: one eye receives the polarized ray, while, by means of a lens, the graduation of the moveable circle is observed with the other. I will not dwell on other details, easy to be understood, and will merely add that the source of light I used was a gas-burner which gave a very brilliant white light.

The liquids on which I operate are contained in tubes hermetically closed at their two ends with glass, which makes it necessary to take account of the influence of the glass upon the rotation—an influence the existence of which I ascertained by operating on empty tubes, and the amount of which varies with the nature and the thickness of the glass. In order to dispense with this correction I placed the tubes between the interpolar surfaces so that their two extremities entered to about a centimetre within the cylinders of soft iron, the internal diameter of which is greater than the external diameter of the tubes. Theory indicates, and experiment fully confirms, that the portion of the tubes (and consequently their glass ends) which is placed in the interior of the iron of the electromagnet is subject to no influence at all from the magnetism developed by the electric current, and that the magnetism affects only that portion of the tube which may be called interpolar—consequently only the liquid column whose length is equal to the distance of the two polar surfaces.*

* I have made a great number of experiments to confirm the accuracy of this principle. Thus I placed in the interior of one of the cylinders of the electromagnet some tubes, from 10 to 15 centims. in length, filled with sulphide of carbon; and never was any effect produced by them upon the polarized ray which traversed that liquid, even when the magnetization was very strong; but when one of the extremities was withdrawn only a few millimetres, an effect was produced.
A second correction which I should have had to make relates to the variations of intensity of the magnetizing current. I should have been obliged to measure that intensity at every experiment—which would have been a complicated and tedious process. In order to dispense with it, I adjusted end to end two perfectly similar tubes, one filled with the liquid to be operated upon, and the other with distilled water, each tube with its glass ends. One of the tubes was so placed in the interpolar space as to avoid the influence of the stoppers; the other, which was as a prolongation of the first, was placed entirely within the interior of one of the branches of the electromagnet. The first alone was subject to the influence of the magnetism; the second was quite uninfluenced by it; so that the rotation of the polarized ray, which successively passed through them both, took place only in the interpolar tube. It was possible with the greatest facility to place first one and then the other tube in the interpolar space, and thus to obtain, by means of alternate observations (each of which occupied only a few moments), the mean angles of rotation of water and of the liquid, which were found to be independent of any variation in the intensity of the current; besides, the variation had scarcely any amount during the time of a single experiment. From one experiment to another the intensity might vary without entailing the slightest inconvenience, since, the coefficient sought being the ratio between the rotation of the liquid and that of distilled water, the important point was to determine these two rotations in each case under the same intensity. Moreover the variation of intensity of the electric current was so little during the first hours of the experiments that I might almost have dispensed with these precautions, though I never did so*

It, however, occurred to me, when the liquid to be experimented on was not very transparent, not to unite the tube containing it to that which contained the distilled water, but, in order that the polarized ray might not have successively to pass through them both, to place them alternately in the interpolar space, which, though less convenient, could be done rapidly and amounted to the same.

In most of my experiments the liquid column interposed between the polar surfaces had a length of 10 centims. But as I had not always a sufficient quantity of liquid to fill a tube

* The pile I used was a large Bunsen’s, composed of from 40 to 50 pairs, charged with dilute nitric and sulphuric acids (9 parts of water to 1 part of acid); I found no great advantage in using a more powerful pile (60 pairs for example), because the wire of the electromagnet became much heated. What contributed to render constant the intensity of the electric current was the shortness of the duration of each experiment, and consequently the discontinuity which took place in the passage of the current.
large enough to obtain this length, I had sometimes to content myself with a shorter one, taking care, for the purpose of accuracy in the comparison, that in each case the length of the water column should be the same as that of the liquid in question*. I started from the principle that the ratio between the angles of rotation is not affected by the absolute length of the liquid columns, provided this be the same in the case of the two liquids compared. Moreover this principle results from that established by M. Verdet in his beautiful researches on rotatory polarization, in which he showed that the rotation is proportional to the thickness of the substance submitted to the magnetic action, provided that its intensity remain the same even when the thickness varies. Nevertheless I wished to assure myself in a direct manner that it actually is so; and I will here relate briefly the experiments I made with this intention, which, while giving an idea of the absolute magnitudes of the rotations I obtained†, will at the same time pretty accurately indicate the magnetic power of the apparatus I used.

I filled a tube 30 centims. long, and 2 centims. in diameter, successively with distilled water, rectified alcohol (sp. gr. 0.804), sulphide of carbon, and iodide of ethyle, selecting these substances from among those which differ most in magnetic rotatory power.

I placed the tube between the two magnetic poles in such a manner as to give the interpolar liquid column successively the lengths of 25, 20, 15, 10, and 5 centims., by a corresponding approximation of the polar surfaces—an approximation which increased the intensity of the magnetic action, although the increase did not quite compensate, by the magnitude of the rotation, for the diminution induced by the shortening of the column.

As the experiments on each liquid were made at different moments from the rest, and consequently with a different intensity of the magnetizing current, only the results obtained from those on each particular liquid are comparable; but this is sufficient for the end I had in view. The comparison of the liquids with one another, which should give their specific magnetic rotatory power, will be the object of the following section.

For brevity's sake, I limit myself to giving, for each of the four liquids submitted to experiment, the angles of rotation observed when the lengths of the column were 25 centims. and 5

* I ought to mention that the diameter of the column exercises no influence on the absolute magnitude of the rotation; this is influenced by the length alone. I gave the experimental proof of this result in the memoir before mentioned.

† The pile was composed of only 40 pairs; with a stronger pile I obtained, as will be seen, in other experiments, rotations of greater magnitude.
centims., the magnetic force remaining constant during the experiment on one and the same liquid.

**Angles of Rotation.**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>25 centims.</td>
<td>11° 20′</td>
<td>9° 30′</td>
<td>3° 33′</td>
<td>2° 10′</td>
</tr>
<tr>
<td>5 ″</td>
<td>9° 15′</td>
<td>7° 45′</td>
<td>27°</td>
<td>19° 40′</td>
</tr>
</tbody>
</table>

It results from this Table that the ratio between the rotations for the lengths of 25 and 5 centims. is 1:22 for each of the four liquids—that is to say, the same, although the absolute magnitudes of these rotations are very different; so that to obtain the magnetic power of a liquid relative to that of water taken as unity, it is only necessary to give to the water column the same length as to the column of liquid operated upon, whatever the absolute length of the two columns.

It follows also from the numbers contained in the preceding Table, that not much is gained by increasing the length of the path of the polarized ray through the liquid, since the magnetic force, as we have remarked, diminishes in proportion as the distance of the polar surfaces is increased in order to lengthen the liquid column. It is true that the increase and the diminution do not follow the same law, since a less rotation is obtained with short columns than with longer ones; but the difference is not very considerable.

The increase of the magnetic force in proportion as the distance between the poles diminishes can be calculated. In fact, knowing that, if the magnetic force were constant, the rotation would be proportional to the length of the liquid column, and consequently, for each of the liquids, the rotation corresponding to a length of 5 centims. would be one-fifth of that which corresponds to 25 centims., we find that for water it would be 2° 16′ instead of 9° 15′, for alcohol 1° 54′ instead of 7° 45′, for sulphide of carbon 6° 6′ instead of 27°, for iodide of ethyle 4° 50′ instead of 19° 40′. On the other hand, knowing that the rotation is proportional to the magnetic force, we can easily determine how much this force has increased by the diminution of the distance of the poles from 25 centims. to 5 centims. We have only to take the ratio between the rotations such as they would be if the distance had not been diminished and the actual rotations with the distance of 5 centims. Now, for each of the four liquids, this ratio is 4:1, or rather a little less; about 4:08; which proves that by the reduction of the distance of the surfaces to one-fifth the magnetic force has only been increased to fourfold, at least with the apparatus I used: it is probable that with electromag-
Rotary Polarization of Liquids.

399

nets whose polar surfaces were smaller, the increase of distance of those surfaces would produce a greater diminution of the magnetic force. Besides, this has been verified by M. Verdet in his first memoir on magnetic rotatory polarization*, in which he has established the two important laws we have mentioned, namely:—the rotation of the plane of polarization is proportional to the intensity of the magnetic action; and it is likewise proportional to the thickness of the substance through which the polarized ray passes, the intensity of the magnetic action remaining constant.

It seems to me that the phenomenon of magnetic rotatory polarization might be usefully applied to determine the intensity of the force existing between two magnetic poles, in what Faraday calls the magnetic field. It would only be necessary to place between the polar surfaces a liquid the coefficient of magnetic rotatory polarization of which is known, taking care that the tube containing it shall be hermetically closed at its two ends with glass disks. Then the angle of rotation with this liquid should be determined in every case in which we wished to know the intensity of the magnetic field†. The ratio between the angles thus obtained would give the ratio between the corresponding intensities of the magnetic fields. This procedure is applicable to every form of electromagnet: it would only be necessary to adapt to the electromagnet an apparatus for the production of magnetic rotatory polarization, which would in no way obstruct any other experiments with the electromagnet—those, for instance, relative to diamagnetism.

§ 2. Determination of the Specific Magneto-rotatory Power of a few Liquids.

I designate by the name of specific magneto-rotatory power the ratio of the magnetic rotatory power of a body to that of distilled water taken as unity. After various trials, I resolved, as I have said, in all the following experiments (with a few exceptions, which I will indicate) to give a length of 10 centims. to

† It would be necessary to take care always to use the same tube filled with the same liquid, to serve as a standard, and to determine preliminarily the rotation produced by it upon the polarized ray under the influence of a magnetic force to be taken as unit. The liquid chosen as a standard should be one susceptible of being always reproduced identical. Distilled water would doubtless be the best; but sulphide of carbon would be preferable for small intensities, on account of its three times as great sensibility, and can likewise be procured quite pure. I think it would be advisable to take as unit the force of the magnetic field corresponding to a rotation of 5° produced by a liquid column of 5 centims. length and with an interpolar distance of 5 centims. It would be easy afterwards to reduce this unit to any other magnetic unit.
the liquid column between the poles, and consequently that that should be the distance between the two polar surfaces; for it will be remembered that, to avoid the influence of the glass disks, each end of the tube containing the liquid entered about 1 centim. within one of the two soft-iron cylinders of the electromagnet.

Magnetizing by means of a Bunsen's pile of 60 pairs, I obtained, under these conditions, with distilled water, a total rotation of 13°; with a pile of from 50 to 40 pairs, the angle was only from 12° to 9°, according to the strength of the pile.

I will now give the results at which I arrived by operating on some liquids chosen as different as possible from one another.

**Alcohol.**

Five series of experiments, each consisting of several comparative observations of rectified alcohol (sp. gr. 0·804) and water, gave the following numbers:

<table>
<thead>
<tr>
<th>Alcohol</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>10 40'</td>
<td>12 10</td>
<td>0·876</td>
</tr>
<tr>
<td>7 20</td>
<td>8 20</td>
<td>0·873</td>
</tr>
<tr>
<td>8 30</td>
<td>9 40</td>
<td>0·879</td>
</tr>
<tr>
<td>9 20</td>
<td>10 40</td>
<td>0·875</td>
</tr>
<tr>
<td>7 58</td>
<td>9 5</td>
<td>0·877</td>
</tr>
</tbody>
</table>

| Mean | 0·876 |

To give an idea of the differences presented by the consecutive observations of one and the same series, the numbers above given being the means, I here transcribe in detail those composing the fifth. The first column indicates the azimuth of one of the sensible tints, the second that of the other, and the third the difference between these two angles, which represents the total rotation. The observations upon water alternated with those on alcohol; but it is easy to perceive that the intensity of magnetization did not sensibly vary during the experiment.

**Alcohol.**

<table>
<thead>
<tr>
<th>16 55'</th>
<th>9 0</th>
<th>7 55'</th>
</tr>
</thead>
<tbody>
<tr>
<td>17 0</td>
<td>9 5</td>
<td>7 55</td>
</tr>
<tr>
<td>17 0</td>
<td>9 0</td>
<td>8 0</td>
</tr>
<tr>
<td>17 0</td>
<td>9 0</td>
<td>8 0</td>
</tr>
</tbody>
</table>

| Mean rotation | 7 58 |

400 Prof. A. De la Rive's *Researches on the Magnetic*
Rotatory Polarization of Liquids.

Water.

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>17 30</td>
<td>8 20</td>
<td>9 10</td>
</tr>
<tr>
<td>17 25</td>
<td>8 15</td>
<td>9 10</td>
</tr>
<tr>
<td>17 25</td>
<td>8 20</td>
<td>9 5</td>
</tr>
<tr>
<td>17 25</td>
<td>8 25</td>
<td>9 0</td>
</tr>
<tr>
<td>17 25</td>
<td>8 25</td>
<td>9 0</td>
</tr>
<tr>
<td>17 25</td>
<td>8 20</td>
<td>9 5</td>
</tr>
<tr>
<td>17 25</td>
<td>8 20</td>
<td>9 5</td>
</tr>
</tbody>
</table>

Mean rotation .. 9 5

Taking the ratio of 7° 58' to 9° 5', we obtain from this series the number 0·877 to represent the specific magneto-rotatory power of alcohol. The mode of operation was the same for the series which gave the numbers 0·876, 0·873, 0·879, 0·875. A difference of 10' between the observations of one series was quite exceptional, and a greater difference extremely rare.

Sulphide of Carbon.

Three series of experiments gave:

<table>
<thead>
<tr>
<th>Sulphide</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>32 10</td>
<td>10 20</td>
<td>3·130</td>
</tr>
<tr>
<td>31 40</td>
<td>10 0</td>
<td>3·116</td>
</tr>
<tr>
<td>29 30</td>
<td>9 20</td>
<td>3·160</td>
</tr>
</tbody>
</table>

The experiments of the third series were made with sulphide of carbon prepared at Paris, and probably a little purer than that with which I operated at first. I think, then, that we must take the mean of the first two series, viz. 3·123, for the coefficient of the specific magneto-rotatory polarization of the first sample of sulphide of carbon I used, and 3·16 for that of the Paris sample (sp. gr. 1·270).

I shall not transcribe here the Table of the observations which compose each series, because they differ very little from one another (scarcely from 3 to 5 minutes); this was owing to the accuracy with which, with sulphide of carbon, we can seize the sensible tints, which are so pronounced and consequently so distinct. It is the same with the three following liquids:

Compounds of Ethyle.

<table>
<thead>
<tr>
<th>Oxide of Ethyle</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>8 20</td>
<td>10 10</td>
<td>0·838</td>
</tr>
<tr>
<td>Bromide of Ethyle</td>
<td>10 0</td>
<td>1·200</td>
</tr>
<tr>
<td>Iodide of Ethyle</td>
<td>20 0</td>
<td>2·233</td>
</tr>
</tbody>
</table>
Of all liquids sulphuric acid is that which, in the experiments, presented the greatest difficulty. The cause of this was twofold:—first, the difficulty of procuring sulphuric acid quite pure and concentrated; secondly, the facility with which the concentrated acid absorbs humidity from the air—an absorption which, by altering it, modifies singularly its magneto-rotatory power.

Three series of experiments, made upon some sulphuric acid procured from a vendor of chemicals at Geneva, gave:

<table>
<thead>
<tr>
<th>Sulphuric acid</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>8 0</td>
<td>9 50</td>
<td>0.814</td>
</tr>
<tr>
<td>7 55</td>
<td>9 45</td>
<td>0.812</td>
</tr>
<tr>
<td>9 0</td>
<td>11 0</td>
<td>0.818</td>
</tr>
<tr>
<td><strong>Mean</strong></td>
<td></td>
<td><strong>0.8147</strong></td>
</tr>
</tbody>
</table>

Subsequently some sulphuric acid from the works of M. Rousseau, of Paris, gave, in three series of experiments, the following results:

<table>
<thead>
<tr>
<th>Sulphuric acid</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>7 50</td>
<td>9 40</td>
<td>0.810</td>
</tr>
<tr>
<td>7 20</td>
<td>9 15</td>
<td>0.792</td>
</tr>
<tr>
<td>7 20</td>
<td>9 13</td>
<td>0.796</td>
</tr>
<tr>
<td><strong>Mean</strong></td>
<td></td>
<td><strong>0.799</strong></td>
</tr>
</tbody>
</table>

For this number 0.800 may be substituted without sensible error, to express the specific magneto-rotatory power of the Paris sulphuric acid. It afterwards served for the calculation of the results I obtained by operating on mixtures, in various proportions, of the same acid and water.

The following is the last series of observations made upon this acid:

<table>
<thead>
<tr>
<th>Acid.</th>
<th>Water.</th>
</tr>
</thead>
<tbody>
<tr>
<td>16 35</td>
<td>9 15</td>
</tr>
<tr>
<td>16 30</td>
<td>9 15</td>
</tr>
<tr>
<td>16 45</td>
<td>9 15</td>
</tr>
<tr>
<td>16 40</td>
<td>9 20</td>
</tr>
<tr>
<td>16 40</td>
<td>9 20</td>
</tr>
<tr>
<td><strong>Mean rotation</strong></td>
<td>9 13</td>
</tr>
</tbody>
</table>

The ratio of 7° 20' to 9° 13' is 0.796, which represents the specific magneto-rotatory power of sulphuric acid given by this series.
Lastly, I was indebted to the kindness of Professor Marignac for a certain quantity of monohydrated sulphuric acid (HO SO₃) prepared by himself, upon the purity of which I could therefore reckon. This acid gave, in two series of experiments, the following results:

<table>
<thead>
<tr>
<th>Acid (HO SO₃)</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>7 0</td>
<td>9 20</td>
<td>0.750</td>
</tr>
<tr>
<td>7 15</td>
<td>9 40</td>
<td>0.750</td>
</tr>
</tbody>
</table>

The following are the observations of the first series; those of the second present no difference one from another.

<table>
<thead>
<tr>
<th>Acid (HO SO₃)</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>16 15</td>
<td>9 25</td>
<td>17 40</td>
</tr>
<tr>
<td>16 20</td>
<td>9 20</td>
<td>17 40</td>
</tr>
<tr>
<td>16 25</td>
<td>9 30</td>
<td>17 30</td>
</tr>
<tr>
<td>16 30</td>
<td>9 20</td>
<td>17 30</td>
</tr>
<tr>
<td>16 25</td>
<td>9 20</td>
<td>17 30</td>
</tr>
<tr>
<td>16 20</td>
<td>9 20</td>
<td>17 30</td>
</tr>
</tbody>
</table>

Mean rotation . 7 0

The ratio of 7° to 9° 20' is 0.750.

Later, having wished to reserve some of the same acid for new experiments, I found a sensible increase in its magneto-rotatory power, which had become 0.761; a third experiment gave 0.768, and a fourth 0.774. Suspecting that these changes proceeded from alteration produced in the acid by hygrometric water absorbed every time the acid was transferred from the experimental tube to the bottle in which it was kept, and vice versa, I took a portion of the same acid which had not been used. I took care to introduce it into a tube the two disks of which were carefully sealed so as to cut off all communication with the external air. I submitted it at several different times to the action of the magnet; and I always found the coefficient 0.750, or at least one very near this, such as 0.749 and 0.751.

The very remarkable difference between the coefficient of magneto-rotatory polarization of the Paris sulphuric acid (0.800) and that of Prof. Marignac's monohydrated sulphuric acid (0.750) appears to me the more astonishing from the density of the two acids being almost the same,—that of the Paris acid being 1.842 at 13° C., and that of Professor Marignac's acid being 1.832 at 20°. However, the Paris acid presents a slight brownish tint, and has a certain empyreumatic odour, which seems to indicate the presence of some foreign substance; while the other acid is perfectly colourless and transparent, and has not the slightest odour.
404 Prof. A. De la Rive's *Researches on the Magnetic Sulphurous Acid.*

I made four series of experiments on liquefied anhydrous sulphurous acid, taking care that the tube which contained it and the disks which closed the tube should be sufficiently solid. In the first series the magnetization was effected by the current from a Bunsen pile of 60 pairs, in the three others by that from a pile of 40 or 50 pairs. The experiments were singularly concordant. Here is the result of the observations of each series:—

<table>
<thead>
<tr>
<th>Sulphurous acid</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>15° 10°</td>
<td>12° 10°</td>
<td>1:246</td>
</tr>
<tr>
<td>13° 40°</td>
<td>11° 0</td>
<td>1:241</td>
</tr>
<tr>
<td>12° 0</td>
<td>9° 40°</td>
<td>1:241</td>
</tr>
<tr>
<td>13° 40°</td>
<td>11° 0</td>
<td>1:241</td>
</tr>
</tbody>
</table>

Suspecting that the first result was due to the temperature of the acid being lower, I took sulphurous acid at the temperature of 4° or 5°, and obtained:—

<table>
<thead>
<tr>
<th>Sulphurous acid</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>14°</td>
<td>11°</td>
<td>1:272</td>
</tr>
</tbody>
</table>

Then, the acid having attained the temperature of the room (12° or 13°), I again obtained 1:241.

A series of experiments made a few days afterwards gave again the number 1:241; but the room becoming warmer, the ratio was reduced to 1:233; and at the end of the experiment it was 1:207, the temperature of the room having risen to 20° C.

These variations, so considerable for small changes of temperature, are due to the great dilatability of liquefied anhydrous sulphurous acid. In fact we shall see in the next Section the marked influence exercised by the temperature on the magneto-rotatory power of liquids—an influence connected with their dilatability.

I think, then, we may, without sensible error, take the number 1:240 for the coefficient of the specific magneto-rotatory polarization of sulphurous acid at 12° C.

Sulphurous acid dissolved in water in sufficient quantity to saturate it gave:—

<table>
<thead>
<tr>
<th>Acidulated water</th>
<th>Water</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>12°</td>
<td>11°</td>
<td>1:100</td>
</tr>
</tbody>
</table>

We know that water dissolves fifty times its volume of gaseous sulphurous acid. Even under the pressure of three atmospheres, gaseous sulphurous acid presented no trace of magneto-rotatory polarization.

I will only mention, without dwelling upon them, some experiments made upon other liquids, particularly the essences.

The essence of orange and the essence of citron gave me only
unsatisfactory results, on account of the difficulty experienced, with those which have a natural rotatory polarization, in determining the sensible tint. The essence of copaiba is more suitable for experiment. I first determined the natural rotatory polarization of a column of this liquid 10 centims. long, and found it to be 28° 30'. The analyzer, placed so as to give the sensible tint without the action of the magnet, marked 124° 30'. Magnetizing in one direction, I recovered the sensible tint at 118° 45'; magnetizing again in the other direction, I recovered the same tint at 130° 20': this gives for the double of the polarization 11° 35', that of water being 8° 45' in the same circumstances. We thus obtain 1.320 for the specific-polarization coefficient of essence of copaiba. It may be remarked that the two magnetic rotations give angles sensibly equal (5° 45' and 5° 50'), to the left and to the right of 124° 30', the azimuth of the natural sensible tint. It is not so with the essence of turpentine, the natural rotatory polarization of which is greater. Starting from 145° 40', the angle corresponding to the natural sensible tint, we have, according to the direction of magnetization, 141° and 151° 20', which gives 4° 40' in one direction and 5° 40' in the other, together 10° 20' for the double of the angle of magneto-rotatory polarization. Now, the corresponding angle for water being 8° 40', this gives 1.192 for the coefficient of the specific magneto-rotatory polarization of the essence of turpentine. I do not attach much importance to this number, on account of the difficulty of procuring essence of turpentine always of the same composition.

The same remark applies to the creosote of commerce, upon which I made several experiments, but will cite only one; it gave 2.259 for the coefficient of specific magneto-rotatory polarization.

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>19° 20'</td>
<td>9°</td>
<td>2.259</td>
</tr>
</tbody>
</table>

We shall not at present discuss the results recorded in this Section; but we shall return to them at the end of this memoir, when we shall have more data for the deduction of consequences from them.

§ 3. Influence of Temperature on the Magneto-rotatory Polarization of Liquids.

According to some physicists, rise of temperature increases the magneto-rotatory power of solid bodies (of glass, for example); according to others it diminishes that power. It is probable that these divergences arise from the inequalities of the dilatation presented by solids, according to the direction in which they are observed—especially those which, like glass, have a structure which is not uniform. For observations of this kind, liquids are
much preferable to solids, on account of their dilatation being equally uniform in all directions. In all those of which I have studied a sufficient number of specimens, the magneto-rotatory power diminishes with the rise of the temperature. Of this we have already seen a striking example in the liquefied anhydrous sulphurous acid; for it is evident that, as already remarked, to its great dilatability* are due the considerable variations of its coefficient of magneto-rotatory polarization, which, from 1.272 at 4°, becomes 1.240 at 12°, and 1.207 at 20°.

Essence of turpentine and creosote also present a sensible diminution of magneto-rotatory power in proportion as their temperature is raised; thus with essence of turpentine the rotation, which is 10° at the temperature of 12° C., is only 9° at the temperature of 80° C.

With creosote the same rotation is 19° at the temperature of 12° C., and only 17° 35' at the temperature of 80° C. Unfortunately I was unable to make use of the experiments on essence of turpentine and creosote to discover the ratio between the augmentation of their volume and the diminution of their magneto-rotatory power. For this purpose I should have had to make direct determinations of their coefficients of dilatation—not being able to depend upon those attributed to them, on account of the differences of composition presented, as I have said, by the different samples of these two liquids submitted to experiment. I have restricted myself to making this comparison with respect to four liquids very different one from another, which I have been able to procure very pure, and the coefficients of dilatation of which have been very carefully determined by several physicists, particularly M. Isidore Pierre.

These four liquids are rectified alcohol (sp. gr. 0.804), iodide of ethyle, water, and monohydrated sulphuric acid (HO SO³). For the first three, I made use of the Tables of dilatation given by M. Isidore Pierre; and for sulphuric acid I used Muncke's Table.

The tube containing the liquid operated on was surrounded by a metal case, the diameter of which was four times that of the tube; the case was filled with water; and a spirit-lamp afforded the means of gradually heating the water, a thermometer constantly indicating its temperature. I took care, in each experiment, to wait till the thermometer had become stationary, in order to be sure that the interior liquid had exactly the temperature of the water†. The precaution had been taken of leaving

* The coefficient of dilatability of liquid sulphurous acid is 0.0018 at 10° C., and 0.0036 at 80°—that is to say, nearly equal to that of air.

† All the thermometric indications are in Centigrade degrees; C. is put after them in order to distinguish degrees of temperature from degrees of rotation.
in the tube a bubble of air (which was not in the path of the polarized ray), in order to allow the liquid to dilate freely. The heating did not sensibly augment the volume of the tube; so that it effected no change in the length of the liquid column, but solely a diminution of its density. To avoid all chance of error, I operated both by heating the water and by letting it cool after heating.

**Rectified Alcohol.**

<table>
<thead>
<tr>
<th>Temperature</th>
<th>Rotation</th>
</tr>
</thead>
<tbody>
<tr>
<td>7°C</td>
<td>9 10</td>
</tr>
<tr>
<td>33°C</td>
<td>8 55</td>
</tr>
<tr>
<td>40°C</td>
<td>8 52</td>
</tr>
<tr>
<td>60°C</td>
<td>8 40</td>
</tr>
<tr>
<td>70°C</td>
<td>8 30</td>
</tr>
</tbody>
</table>

This Table exhibits the means of the results obtained on arriving at the temperatures indicated either by heating or cooling. From it are deduced the following ratios between the density of the liquid and its corresponding magneto-rotatory power.

<table>
<thead>
<tr>
<th>Temperatures</th>
<th>Ratio between the densities</th>
<th>Ratio between the rotations</th>
</tr>
</thead>
<tbody>
<tr>
<td>7 and 33°C</td>
<td>1.028</td>
<td>1.028</td>
</tr>
<tr>
<td>7°C and 40°C</td>
<td>1.032</td>
<td>1.035</td>
</tr>
<tr>
<td>7°C and 60°C</td>
<td>1.062</td>
<td>1.058</td>
</tr>
<tr>
<td>7°C and 70°C</td>
<td>1.079</td>
<td>1.078</td>
</tr>
<tr>
<td>33°C and 60°C</td>
<td>1.031</td>
<td>1.029</td>
</tr>
<tr>
<td>33°C and 70°C</td>
<td>1.050</td>
<td>1.050</td>
</tr>
<tr>
<td>40°C and 60°C</td>
<td>1.025</td>
<td>1.023</td>
</tr>
<tr>
<td>40°C and 70°C</td>
<td>1.042</td>
<td>1.043</td>
</tr>
<tr>
<td>60°C and 70°C</td>
<td>1.017</td>
<td>1.019</td>
</tr>
</tbody>
</table>

Thus, by raising the temperature of alcohol, its magneto-rotatory coefficient is diminished in nearly the same ratio as its density. In fact it must not be forgotten that in the experiments the length of the column passed through by the polarized ray remains constant, and that it is the density that changes; therefore the ratio of the volume at 33°C to the volume at 7°C gives the ratio of the density at 7°C to the density at 33°C, and the same for the other differences of temperature; and it was by taking the ratios between the volumes at different temperatures, as given in the Tables of dilatation, that I determined the ratios between the densities.

I pass now to my experiments upon iodide of ethyle and amyllic alcohol, in which I used the same process as in those on rectified alcohol.
### Iodide of Ethyle.

<table>
<thead>
<tr>
<th>Temperature</th>
<th>Rotation</th>
</tr>
</thead>
<tbody>
<tr>
<td>12 C.</td>
<td>20 40</td>
</tr>
<tr>
<td>31 „</td>
<td>20 13</td>
</tr>
<tr>
<td>59 „</td>
<td>19 30</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Temperatures</th>
<th>Ratio between the densities</th>
<th>Ratio between the rotations</th>
</tr>
</thead>
<tbody>
<tr>
<td>12 and 31</td>
<td>1.0224</td>
<td>1.0222</td>
</tr>
<tr>
<td>12 „ 59</td>
<td>1.059</td>
<td>1.059</td>
</tr>
<tr>
<td>31 „ 59</td>
<td>1.036</td>
<td>1.036</td>
</tr>
</tbody>
</table>

### Amylic Alcohol.

<table>
<thead>
<tr>
<th>Temperature</th>
<th>Rotation</th>
</tr>
</thead>
<tbody>
<tr>
<td>15 C.</td>
<td>9 30</td>
</tr>
<tr>
<td>30 „</td>
<td>9 22</td>
</tr>
<tr>
<td>35 „</td>
<td>9 20</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Temperatures</th>
<th>Ratio between the densities</th>
<th>Ratio between the rotations</th>
</tr>
</thead>
<tbody>
<tr>
<td>15 and 30</td>
<td>1.014</td>
<td>1.014</td>
</tr>
<tr>
<td>15 „ 35</td>
<td>1.019</td>
<td>1.018</td>
</tr>
</tbody>
</table>

Thus, then, as in the case of rectified alcohol, the diminution which a rise of temperature occasions in the magneto-rotatory power of iodide of ethyle and in that of amylic alcohol will be proportional to the corresponding diminution of density. Of these liquids, again, I determined the ratio between the densities by means of the ratio between their volumes, at different temperatures, as given in the Tables prepared by M. Isidore Pierre*. The results obtained with alcohol are the most conclusive, being derived from experiments upon a greater range of temperature and at the same time more numerous. Nevertheless both series indicate that, if the law to which they seem to conduct is not absolute, it is at any rate very nearly accurate, at least within the limits of temperature within which the observations took place. If, then, we could strictly prove that the magneto-rotatory power of every liquid at a determined temperature is proportional to its density at the same temperature, we should arrive at the conclusion that this power is molecular and remains the same for each molecule, whatever the physical conditions in which it is placed; and the deduction from this would be, that the magneto-rotatory power of a body is only the sum of the magneto-rotatory powers of the particles of which it con-

* *Annales de Chimie et de Physique, S. 3. vol. xv. p. 354, and vol. xix. p. 199 et seqq,*
The researches of M. Biot have caused the admission of this principle with regard to the natural rotatory power possessed by certain substances.

The numerous experiments which I made upon water and upon monohydrated sulphuric acid (\(\text{H}_2\text{SO}_3\)) have not confirmed the law which seemed to result from the preceding observations, as I shall show from the numbers which refer to those two liquids.

Among the series of experiments to which I submitted water (all of which lead to the same conclusion), I select the following—one of the last I made, and on which I bestowed particular care; it has also the advantage of comprising a greater range of temperatures:

**Distilled water.**

<table>
<thead>
<tr>
<th>Temperature</th>
<th>Rotation</th>
</tr>
</thead>
<tbody>
<tr>
<td>10° C.</td>
<td>10 35°</td>
</tr>
<tr>
<td>21 °</td>
<td>10 30°</td>
</tr>
<tr>
<td>31 °</td>
<td>10 25°</td>
</tr>
<tr>
<td>40 °</td>
<td>10 20°</td>
</tr>
<tr>
<td>51 °</td>
<td>10 15°</td>
</tr>
<tr>
<td>60 °</td>
<td>10 10°</td>
</tr>
<tr>
<td>71 °</td>
<td>10 5°</td>
</tr>
</tbody>
</table>

The preceding Table shows that, in the case of water, the magneto-rotatory power diminishes very regularly with the increase of temperature—in particular, that the ratio between the magneto-rotatory power diminishes very regularly with the increase of temperature—in particular, that the ratio between the

magneto-rotatory powers at temperatures differing by 10° remains sensibly the same, with a slight tendency to increase as the temperature rises. But we do not find, as with those liquids on which we operated at first, the ratios between the rotations equal to those between the corresponding densities; the latter are much less, especially at the lowest temperatures. Thus, in water, the effect of rise of temperature upon the magneto-rotation does not depend solely upon the diminution of the density; it acts independently, and the more in proportion as the absolute temperature is lower. For example, the ratio between the density at 40° and the density at 51° is 1.0048, while the ratio between the densities at 10° and 21° (which, like the preceding, differ by 11°) is only 1.0017; and yet the ratio between the rotatory powers is the same (1.008) in both cases; it should have been a little less in the second than in the first. There must, then, have been two factors in the influence of the temperature upon the magneto-rotatory power—the one depending on the variation of the density, the other on the temperature itself independently of its influence on the density—both factors acting in the same direction; in other words, rise of temperature diminishes the magneto-rotatory power both by diminishing the density and directly.

The following Table of observations upon monohydrated sulphuric acid contains the means of the results furnished by two distinct series of experiments:

<table>
<thead>
<tr>
<th>Temperatures</th>
<th>Ratio between the densities</th>
<th>Ratio between the rotations</th>
</tr>
</thead>
<tbody>
<tr>
<td>20° C., 30° C.</td>
<td>8 0</td>
<td>1.0059</td>
</tr>
<tr>
<td>30° C., 40° C.</td>
<td>7 52</td>
<td>1.010</td>
</tr>
<tr>
<td>40° C., 50° C.</td>
<td>7 50</td>
<td>1.015</td>
</tr>
<tr>
<td>50° C., 60° C.</td>
<td>7 43</td>
<td>1.017</td>
</tr>
<tr>
<td>60° C., 70° C.</td>
<td>7 35</td>
<td>1.055</td>
</tr>
</tbody>
</table>

Thus with sulphuric acid, as with water, the magneto-rotatory power diminishes with the rise of temperature more rapidly than it would if the diminution were dependent only on the density.

It therefore appears to me evident that, although diminution of density may be the principal cause of the effect in question, it is not sufficient in all cases to explain it, and that the molecular
effect itself must be admitted to be less energetic when the temperature is higher. With certain liquids, such as alcohol especially, the direct influence of the temperature is almost insensible, which is probably owing to the great dilatability of that liquid rendering preponderant the influence of the decrease of density.


I began by mixing equal volumes of distilled water and rectified alcohol of sp. gr. 0·804. The magneto-rotatory power of the mixture was obtained by means of two series of experiments, made at two different periods, and which gave the following numbers:—

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>11° 40'</td>
<td>12°</td>
<td>0·972</td>
</tr>
</tbody>
</table>

The magneto-rotatory power calculated as the mean of those of water and alcohol is 0·938, that of water being 1·000, and that of alcohol 0·876. The actual density of the mixture was found to be 0·935; the density calculated as the mean of those of alcohol and water would be 0·902. Now the ratio of the real rotatory power to the calculated is 1·0362; and the ratio between the actual and the calculated densities is 1·0365. Thus, in a mixture of equal volumes of water and alcohol, which is accompanied by a sensible contraction (proved by the increase of density), the magneto-rotatory power augments in exactly the same proportion as the density—which proves that the molecular magneto-rotatory power does not change. We have seen, in the preceding Section, that it is the same with alcohol with respect to the changes of density proceeding from variations of temperature. In taking a mixture of equal volumes of water and alcohol, I chose that in which the change in volume is the greatest; it is evident, then, that with mixtures in other proportions the same result would be found.

The mixtures of water and sulphuric acid in different proportions gave very interesting results, which I think deserving of detailed exposition. This kind of experiments present some difficulties, especially with concentrated solutions, on account of the rapidity with which they attract humidity from the air, which changes their identity; nevertheless this inconvenience may be avoided by taking precautions. In fact we have seen that monohydrated sulphuric acid having a rotatory power of 0·750 acquires one of 0·757 after one or two removals from one vessel to another in the air, and one equal to 0·768 after a greater number.

My first experiments were made with the Paris sulphuric acid, of which the rotatory power is 0·800, and the density (taken
with great accuracy) 1.842. With this acid and distilled water I made mixtures in the following proportions:

- 9 volumes of water, and 1 of acid:
- 7 volumes of water, and 1 of acid:
- 5 volumes of water, and 1 of acid:
- 4 volumes of water, and 1 of acid:
- 3 volumes of water, and 1 of acid:
- 2 volumes of water, and 1 of acid:

I ascertained the magneto-rotatory power of each of these solutions; and I compared this real power with that calculated by taking 0.800 for the magneto-rotatory power of the pure acid and 1.000 for that of water, taking account of the proportions of water and acid in the mixture, and supposing that no change of volume takes place by the act of mixing. I likewise determined the ratio between the actual density of each solution and its density calculated by taking 1.8421 for the density of the pure acid and 1.000 for that of water, and supposing that no change of volume is effected by mixing.

The following Table, drawn up from the means of a great number of experiments, gives, for each of the solutions, on the one hand the actual and the calculated rotatory powers and the ratio between them, and on the other the actual and the calculated densities and the ratio between these.

<table>
<thead>
<tr>
<th>Proportion of acid in the solution</th>
<th>Actual rotatory power</th>
<th>Calculated rotatory power</th>
<th>Ratio between the two powers</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.1</td>
<td>1.012</td>
<td>0.980</td>
<td>1.032</td>
</tr>
<tr>
<td>0.3</td>
<td>0.975</td>
<td>0.940</td>
<td>1.037</td>
</tr>
<tr>
<td>0.5</td>
<td>0.934</td>
<td>0.900</td>
<td>1.037</td>
</tr>
<tr>
<td>0.6</td>
<td>0.915</td>
<td>0.880</td>
<td>1.039</td>
</tr>
<tr>
<td>0.7</td>
<td>0.888</td>
<td>0.860</td>
<td>1.032*</td>
</tr>
<tr>
<td>0.8</td>
<td>0.875</td>
<td>0.840</td>
<td>1.040</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Actual density</th>
<th>Calculated density</th>
<th>Ratio between the two densities</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.1</td>
<td>1.118</td>
<td>1.084</td>
</tr>
<tr>
<td>0.3</td>
<td>1.316</td>
<td>1.252</td>
</tr>
<tr>
<td>0.5</td>
<td>1.553</td>
<td>1.421</td>
</tr>
<tr>
<td>0.6</td>
<td>1.607</td>
<td>1.505</td>
</tr>
<tr>
<td>0.7</td>
<td>1.623</td>
<td>1.590</td>
</tr>
<tr>
<td>0.8</td>
<td>1.751</td>
<td>1.674</td>
</tr>
</tbody>
</table>

* The result obtained, both as to the ratio of the actual to the calculated rotation and the ratio of the actual to the calculated density, with the solution containing 7 parts by volume of acid to 3 of water presents an evident anomaly, of which I have not been able to discover the cause; it is possibly the result of an experimental error, though I am not disposed to think so.
This Table shows that, in solutions containing only a relatively small proportion of acid or of water, the magneto-rotatory power increases in the same ratio as the density. Thus the ratio of the actual to the calculated power is 1·032 for the solution which contains, in 10 parts by volume, 1 of sulphuric acid and 9 of water; for the same solution the ratio of the actual to the calculated density is also 1·032. Similarly the ratio between the actual and the calculated rotatory powers is 1·040 for the solution which contains, in 10 parts by volume, 8 of acid and 2 of water; and the ratio of the actual to the calculated density of the same solution is 1·045, or very nearly the same, only a little higher. But if solutions be taken which contain nearly equal quantities of water and acid (such as those comprised between 3 tenth parts of water and 7 of acid, on the one hand, and 3 tenth parts of acid and 7 of water, on the other), then the ratios between the actual and the calculated rotatory powers become much lower than those between the actual and the calculated densities; and this difference attains its maximum in the solution which contains exactly equal volumes of acid and water.

As to the rotatory powers themselves, the ratio between the actual and the calculated power does not differ much from one solution to another; yet it increases slightly, but regularly, with the concentration of the solution.

It seems to me that we may infer from these observations that the combination of the acid and water diminishes the molecular rotatory power, since the ratio between the actual rotatory power of a certain volume of the mixture and the rotatory power of the same mixture calculated on the supposition of there being no contraction is lower than the ratio between the actual and the calculated density, while the two ratios would have been equal if the combination of the water and the acid had not modified the rotatory power of each. When the proportion of water or of acid is very slight, the ingredients combining in very small quantity and mixing in that one of them which is in excess, the solution behaves like a simple mixture. A very curious fact is the almost complete equality of the ratios between the actual and the calculated magneto-rotatory powers of all the solutions, which would seem to indicate that the chemical action which brings to the magneto-rotatory power of the solution a modification which renders it different from what it would be if the liquid were only a simple mixture, acts sensibly in the same degree upon each; I say sensibly, because the ratios, though not differing much, show a tendency to increase with the increase of the proportion of acid relatively to that of water.

I submitted to the same experiments two other solutions of sulphuric acid in water, which were kindly supplied to me by
Professor Marignac. Both were prepared with monohydrated sulphuric acid (\(\text{HO SO}_3\)) of sp. gr. 1.83211 at 20°, and with the specific magneto-rotatory power (as I have stated above) of 0.750. One of these solutions consisted of HO \(\text{SO}_3\)+5 aq; the other of HO \(\text{SO}_3\)+10 aq. The actual magneto-rotatory power of each of these solutions was compared with its magneto-rotatory power calculated as the mean of the rotatory powers of the water and of the acid (HO \(\text{SO}_3\)) mixed in the same proportion in which they enter into the solution. Similarly the actual densities were compared with the densities calculated on the supposition that no contraction occurred in the mixture.

<table>
<thead>
<tr>
<th>(\text{HO SO}_3)+5 aq.</th>
<th>(\text{HO SO}_3)+10 aq.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Actual rotation</td>
<td>0.943</td>
</tr>
<tr>
<td>Calculated rotation</td>
<td>0.906</td>
</tr>
<tr>
<td>Ratio between the two rotations</td>
<td>1.041</td>
</tr>
<tr>
<td>Actual density</td>
<td>1.413</td>
</tr>
<tr>
<td>Calculated density</td>
<td>1.310</td>
</tr>
<tr>
<td>Ratio between the two densities</td>
<td>1.078</td>
</tr>
</tbody>
</table>

We see that, as in the preceding experiments, the ratio between the rotatory powers differs less from the ratio between the densities in just the same degree as the proportions of water and acid in the mixture differ more. What is remarkable is, that, as before, the ratio between the actual and the calculated rotatory powers is sensibly the same for each solution.

It must not be lost sight of that the specific rotatory powers are always taken with equal volumes, and that the calculated powers are the means of the respective powers of water and the acid, assuming the mixture to take place without change of volume. If in the calculation account is taken of the contraction, then we find, as might have been expected, that the calculated power is higher (always with an equal volume) than the actual power; in fact it is 0.965 instead of 0.943 for HO \(\text{SO}_3\)+5 aq, and 0.998 instead of 0.980 for HO \(\text{SO}_3\)+10 aq. Thus the magneto-rotatory power of the compound is less than the mean of the respective powers of the water and the acid which form the combination. This was already evident from the fact that, without taking account of the contraction, the ratio between the actual and the calculated powers is less than the ratio between the actual and the calculated densities.

M. Berthelot had kindly given me, in the spring of 1869, a certain quantity of two isomeric liquids; but not having prepared them himself, he took care to tell me that he did not guarantee their perfect purity. They were ethylvaleric ether and amylacetic ether, both having the general formula C\textsuperscript{14}H\textsuperscript{14}O\textsuperscript{4}. Submitted several times, and at different periods, to experiment, the second always exhibited a stronger magneto-rotatory power than the first. The most accurate experiments gave 0.877 for the specific magneto-rotatory power of the first, and 0.895 for that of the second. When I wished to compare their rotatory powers with their densities, I was much embarrassed on account of the difficulty of ascertaining these densities. Thus, according to M. Delffs, the density of amylacetic ether is 0.863 at 10°, and according to M. Kopp 0.8837 at 0°. The density of ethylvaleric ether is, according to M. Delffs, 0.870 at 13°.5, and according to M. Berthelot himself 0.869 at 14°. On the other hand, M. Adolphe Perret (who was so obliging as to determine for me the densities of most of the liquids I used in my researches) found for the density of both the ethers in question at 16° the number 0.870; so that the samples upon which I operated had, apparently, the same density; the very perceptible difference, therefore, presented by their rotatory powers cannot depend upon a difference of density. Besides, if either the density attributed to amylacetic ether by M. Delffs or that attributed by M. Kopp were taken, this would not be sufficient to explain the superiority of its magneto-rotatory power. There is probably, then, in the different molecular grouping presented by these two isomeric liquids a cause of the greater magneto-rotatory power of amylacetic than of ethylvaleric ether.

Subsequently having been obliged by M. Wurtz with specimens of some isomeric liquids prepared by himself, I was enabled to extend the field of my investigation. Besides the acetate of amyle and valerate of ethyle which I had already submitted to experiment under the names of amylacetic ether and ethylvaleric ether, M. Wurtz sent me a specimen of butyrate of isopropyle, which is isomeric with the two others. The general formula of these three compounds is, according to the notation adopted by M. Wurtz, who doubles the number for hydrogen, C\textsuperscript{7}H\textsuperscript{14}O\textsuperscript{2} instead of C\textsuperscript{14}H\textsuperscript{14}O\textsuperscript{4}. Following the system adopted by M. Wurtz, the formula becomes:

- For acetate of amyle, C\textsuperscript{2}H\textsuperscript{8} (C\textsuperscript{5}H\textsuperscript{11}) O\textsuperscript{2};
- For valerate of ethyle, C\textsuperscript{2}H\textsuperscript{5} (C\textsuperscript{9}H\textsuperscript{9}) O\textsuperscript{2};
- For butyrate of isopropyle, C\textsuperscript{3}H\textsuperscript{7} (C\textsuperscript{4}H\textsuperscript{7}) O\textsuperscript{2}. 
The liquids gave the following results, each deduced from five series of experiments*:

**Acetate of Amyle.**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>8 40</td>
<td>9 35</td>
<td>0.904</td>
<td>8 20</td>
<td>9 30</td>
<td>0.877</td>
</tr>
<tr>
<td>8 26</td>
<td>9 20</td>
<td>0.902</td>
<td>8 5</td>
<td>9 10</td>
<td>0.881</td>
</tr>
<tr>
<td>8 40</td>
<td>9 36</td>
<td>0.903</td>
<td>8 13</td>
<td>9 20</td>
<td>0.880</td>
</tr>
<tr>
<td>8 54</td>
<td>9 50</td>
<td>0.905</td>
<td>8 30</td>
<td>9 45</td>
<td>0.879</td>
</tr>
<tr>
<td>8 50</td>
<td>9 45</td>
<td>0.906</td>
<td>8 12</td>
<td>9 23</td>
<td>0.877</td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td>0.904</td>
<td>Mean</td>
<td></td>
<td>0.879</td>
</tr>
</tbody>
</table>

**Valerate of Ethyle.**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>8 20</td>
<td>9 40</td>
<td>0.862</td>
</tr>
<tr>
<td>8 5</td>
<td>9 20</td>
<td>0.866</td>
</tr>
<tr>
<td>8 30</td>
<td>9 50</td>
<td>0.864</td>
</tr>
<tr>
<td>8 12</td>
<td>9 30</td>
<td>0.863</td>
</tr>
<tr>
<td>8 23</td>
<td>9 40</td>
<td>0.865</td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td>0.864</td>
</tr>
</tbody>
</table>

**Butyrate of Isopropyle.**

I have excluded one of the series of experiments, which gave results, for the three liquids, very different from those furnished by the five series from which the above Tables were constructed; yet the numbers found agreed very well, as to their ratios, with those resulting from the respective means of the three Tables: they were, for acetate of amyle 0.893, for valerate of ethyle 0.858, and for butyrate of isopropyle 0.853. I can only attribute this difference (which only occurred once) to the very high temperature (more than 30°) at which these last-mentioned experiments were made. But if this series be introduced into the three Tables, the mean becomes for the acetate 0.902 instead of 0.904, for the valerate 0.877 instead of 0.879, and for the butyrate 0.862 instead of 0.864: these numbers would then express the specific magneto-rotatory powers of the three liquids respectively. I prefer, however, to adopt the former ones.

In either case it follows from the preceding experiments that the three isomers have sensibly different magneto-rotatory powers—the butyrate a lower power than the valerate, and the valerate a considerably lower one than the acetate, as I had

---

* I ought to remark that I had not sufficient liquid to give the columns a length of 10 centims. Those of acetate of amyle and butyrate of isopropyle were each 6 centims., and that of valerate of ethyle only 5½ centims. long. Of course the water column was always of the same length as that of the liquid with which it was compared.
already found with the two liquids given me by M. Berthelot. It is probable, as I have remarked, that these differences are connected with the mode of molecular grouping—which is not the same in the three substances, as is shown in M. Wurtz's chemical formulæ above given. The boiling-points, too, follow the same order as the rotatory powers: that of the acetate is 138°, that of the valerate 133°, and that of the butyrate 128°. On the contrary, the densities do not seem to have any influence on the phenomenon; for that of the valerate (0·894) is greater than that of the acetate (0·880), though it is true that the density of the butyrate is the least of the three. It ought to be remarked that the valerate of ethyle has a rather strong natural rotatory power, 1° 30' for a length of 10 centims.: this property does not seem to have any connexion with the magneto-rotatory power, since as to this the valerate is placed between the acetate and the butyrate, which have no sensible natural rotatory power.

I pass to two other isomers, amylic alcohol and hydrate of amy-lene, the general formulæ of which is, according to M. Wurtz, C₅H₁₂O. Amylic alcohol has, like the valerate of ethyle, a rather considerable natural rotatory power (1° 40'); but this power is exercised in the opposite direction; it also appears not to affect the magneto-rotatory power.

Three series of experiments were made upon each of these two liquids. The following are the results:—

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>9 40</td>
<td>10</td>
<td>0·967</td>
<td>8 20</td>
<td>8 40</td>
<td>0·961</td>
</tr>
<tr>
<td>9 20</td>
<td>9 40</td>
<td>0·966</td>
<td>7 55</td>
<td>8 16</td>
<td>0·958</td>
</tr>
<tr>
<td>9 15</td>
<td>9 35</td>
<td>0·965</td>
<td>8 0</td>
<td>8 20</td>
<td>0·960</td>
</tr>
</tbody>
</table>

| Mean .   | 0·966 | Mean . | 0·960 |

Thus it would appear that the magneto-rotatory power of amylic alcohol is slightly higher than that of hydrate of amy-lene*. Let us notice again that the boiling-point of the first (132°) is higher than that of the second (104–105°). With regard to the densities, they differ little—that of amylic alcohol being 0·818 at 15°, and that of hydrate of amylene 0·826 at 0°.

M. Wurtz kindly sent me also two other isomeric liquids, amylnime and isoamylnime†, which have in common the formulæ

* I ought to mention that the length of the column of amylic alcohol was 8 centims., while that of the hydrate-of-amylene column was only 4 centims.; but in each case the water column was of the same length as that of the liquid with which it was compared, which makes the ratios quite correct.

† The column of amylnime was 6 centims. long, that of isoamylnime only 4; but, as before, the length of the water column corresponded.
C₅H₁₃N. Unfortunately isoamylamine readily absorbs carbonic acid from the air and forms a solid carbonate—which slightly dulls its transparency, so that I had some difficulty in determining its magneto-rotatory power very exactly. This was not the case with the amylamine; the first three series of observations gave the following results:—

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>10° 56'</td>
<td>10° 40'</td>
<td>1·025</td>
</tr>
<tr>
<td>9 35</td>
<td>9 20</td>
<td>1·026</td>
</tr>
<tr>
<td>9 20</td>
<td>9 5</td>
<td>1·027</td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td>1·026</td>
</tr>
</tbody>
</table>

Three other series, less concordant, gave:—

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>9 44</td>
<td>9 33</td>
<td>1·019</td>
</tr>
<tr>
<td>9 40</td>
<td>9 30</td>
<td>1·017</td>
</tr>
<tr>
<td>9 40</td>
<td>9 15</td>
<td>1·042</td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td>1·026</td>
</tr>
</tbody>
</table>

Thus 1·026 may be regarded as expressing very accurately the magneto-rotatory power of amylamine.

As to the isoamylamine, I think its magneto-rotatory power may, without great error, be set down as 1·017; but the difficulty of observation renders this number rather uncertain. At all events I am able to conclude from my experiments that this substance has a lower magneto-rotatory power than amylamine. Its boiling-point is 78°·5, while that of amylamine is 95°. The density of isoamylamine (0·815 at 0°) is less than that of amylamine (0·826 at 0°).

To sum up, isomeric bodies, notwithstanding the identity of their elementary composition, have not the same magneto-rotatory power; between some the difference is great, between others little. This difference does not appear to have any connexion with their different densities, but rather with the difference of their boiling-points; for, in each group of isomers, that which has the highest boiling-point has the greatest magneto-rotatory power.

Conclusions.

From the experiments I have above related, I think the following conclusions may be drawn:—

1. The magneto-rotatory power of bodies does not appear to be connected with their other physical properties. Although in general the densest and most refracting substances are those which possess this power in the highest degree, there are some notable exceptions; and sulphuric acid is one of the most re-
Rotatory Polarization of Liquids.

419

markable: thus, although denser and more refracting than water, its magneto-rotatory power is only \( \frac{3}{4} \) of that of water (0.750); while liquified sulphurous acid, much less dense, has a higher magneto-rotatory power (1.240). The chemical nature of the substance therefore exerts a preponderant influence: in fact, as we have just seen, one equivalent more of oxygen in its combination with sulphur is sufficient to reduce nearly one half the magneto-rotatory power of the compound; on the contrary, bromine and, especially, iodine, on entering into a combination, contribute remarkably to the greatness of its magneto-rotatory power. To be assured of this, one has only to compare in this respect the three compounds of ethyle. The presence of nitrogen in a compound also tends to increase the magneto-rotatory power; this follows from the observations made upon amylamine and isoamylamine, in which nitrogen replaces the oxygen in amyl alcohol and hydrate of amylene, the composition of which, excepting this substitution, is almost identical with that of the two former substances. In general, the greater the proportion of oxygen in a compound, the less is its magneto-rotatory power.

It is probable, however, that the mode of combination, and particularly the mode of grouping of the atoms, consequently the atomic volume, has, independently even of the nature of the atoms, a great influence upon the intensity of the magneto-rotatory power. This follows especially from the differences in that respect presented by isomeric substances.

2. Rise of temperature diminishes, in all liquids, the magneto-rotatory power, first by lessening their density and consequently diminishing in a given volume the number of the particles which act upon the polarized ray, and then by a direct effect independent of the dilatation, an effect most perceptible in those liquids, such as water, which are but little dilatable*.

3. A mixture of two liquids, when its preparation is not accompanied by strong chemical action, has a rotatory power which is the mean of the powers of the ingredients of which it is composed. The example of the sulphuric-acid solutions seems to prove that, when the solution contains less than two tenth parts of one or the other component, the rotatory power of the mixture

* Various physicists, and specially M. Wulner (Poggendorff's Annalen, vol. cxxxii. pp. 1-53), have stated that the refractive power of a liquid diminishes with the rise of temperature in the same ratio as the density; but, as M. Bertin (Ann. de Chim. et de Phys. S. 4. vol. xiv. p. 499) rightly remarks, the experiments were made with variations of temperature so small that it is impossible to prove from them a general law; I am convinced that the above is only an approximative one, as in the case of the magneto-rotatory powers.
is almost exactly the mean of the powers of the two liquids which compose it—account being taken of the proportion of each in the mixture, and of the alteration of density, in this case very small. On the contrary, when the proportion of acid or of water is more than two tenths, the rotatory power increases in a much less proportion than the density, proving that the combination diminishes the molecular magneto-rotatory power. It is true that in this case there is a strong chemical action, as is proved by the great disengagement of heat. I am disposed to believe that in all cases there is formed a compound of water and acid, of which the magneto-rotatory power is always less than the mean of the rotatory powers of the water and the acid in combination, even allowing for the contraction, as we have seen above. Only, when the proportions of the two substances mixed are very different, the compound, dissolved in a large quantity of the liquid which is in excess, cannot sensibly modify the mean rotatory power. It is not so when the proportions of the two liquids are nearly equal, the compound being in proportionally much larger quantity in the mixture. Magnetic rotatory polarization, therefore, might with advantage be used for the purpose of distinguishing simple physical solutions or weak combinations from definite chemical compounds.

4. Our fourth conclusion is that the phenomenon of magneto-rotatory polarization presents a means of penetrating into the intimate constitution of bodies, and may thus be really serviceable to science. For example, the still very imperfect investigation we have made shows already that the relations of ponderable particles to the ether in which they are immersed do not depend solely on the nature of the particles, but also on their mode of grouping in the combinations which they form; for simple mixture is not sufficient to modify these relations; combination is necessary in order that modification may take place. I think, therefore, that it will be especially by operating on solutions and isomeric bodies that we shall succeed in throwing some light both on the nature of the phenomenon of magnetic rotatory polarization and on the atomic constitution of bodies, by determining for this purpose the differences existing between the magneto-rotatory powers of isomers on the one hand, and on the other the differences presented by the rotatory powers of simple mixtures compared with those of true chemical combinations. I shall endeavour, if I can procure the necessary substances, to study these two points more completely than I have been able to study them in the present memoir.
L. On Hills and Dales.
By J. Clerk Maxwell, LL.D., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I find that in the greater part of the substance of the following paper I have been anticipated by Professor Cayley, in a memoir "On Contour and Slope Lines," published in the Philosophical Magazine in 1859 (S. 4. vol. xvi. p. 264). An exact knowledge of the first elements of physical geography, however, is so important, and loose notions on the subject are so prevalent, that I have no hesitation in sending you what you, I hope, will have no scruple in rejecting if you think it superfluous after what has been done by Professor Cayley.

I am, Gentlemen,

Your obedient Servant,

J. Clerk Maxwell.

Glenlair, Dalbeattie,
October 12, 1870.


The results of the survey of the surface of a country are most conveniently exhibited by means of a map on which are traced contour-lines, each contour-line representing the intersection of a level surface with the surface of the earth, and being distinguished by a numeral which indicates the level surface to which it belongs.

When the extent of country surveyed is small, the contour-lines are defined with sufficient accuracy by the number of feet above the mean level of the sea; but when the survey is so extensive that the variation of the force of gravity must be taken into account, we must adopt a new definition of the height of a place in order to be mathematically accurate. If we could determine the exact form of the surface of equilibrium of the sea, so as to know its position in the interior of a continent, we might draw a normal to this surface from the top of a mountain, and call this the height of the mountain. This would be perfectly definite in the case when the surface of equilibrium is everywhere convex; but the lines of equal height would not be level surfaces.

Level surfaces are surfaces of equilibrium, and they are not equidistant. The only thing which is constant is the amount of work required to rise from one to another. Hence the only consistent definition of a level surface is obtained by assuming a standard station, say, at the mean level of the sea at a particular
place, and defining every other level surface by the work required to raise unit of mass from the standard station to that level surface. This work must, of course, be expressed in absolute measure, not in local foot-pounds.

At every step, therefore, in ascertaining the difference of level of two places, the surveyor should ascertain the force of gravity, and multiply the linear difference of level observed by the numerical value of the force of gravity.

The height of a place, according to this system, will be defined by a number which represents, not a lineal quantity, but the half square of the velocity which an unresisted body would acquire in sliding along any path from that place to the standard station. This is the only definition of the height of a place consistent with the condition that places of equal height should be on the same level. If by any means we can ascertain the mean value of gravity along the line of force drawn from the place to the standard level surface, then, if we divide the number already found by this mean value, we shall obtain the length of this line of force, which may be called the linear height of the place.

On the Forms of Contour-lines.

Let us begin with a level surface entirely within the solid part of the earth, and let us suppose it to ascend till it reaches the bottom of the deepest sea. At that point it will touch the surface of the earth; and if it continues to ascend, a contour-line will be formed surrounding this bottom (or Inmit, as it is called by Professor Cayley) and enclosing a region of depression. As the level surface continues to ascend, it will reach the next deepest bottom of the sea; and as it ascends it will form another contour-line, surrounding this point, and enclosing another region of depression below the level surface. As the level surface rises these regions of depression will continually expand, and new ones will be formed corresponding to the different lowest points of the earth’s surface.

At first there is but one region of depression, the whole of the rest of the earth’s surface forming a region of elevation surrounding it. The number of regions of elevation and depression can be altered in two ways.

1st. Two regions of depression may expand till they meet and so run into one. If a contour-line be drawn through the point where they meet, it forms a closed curve having a double point at this place. This contour-line encloses two regions of depression. We shall call the point where these two regions meet a Bar.

It may happen that more than two regions run into each other at once. Such cases are singular, and we shall reserve them for separate consideration.
2ndly. A region of depression may thrust out arms, which may meet each other and thus cut off a region of elevation in the midst of the region of depression, which thus becomes a cyclic region, while a new region of elevation is introduced. The contour-line through the point of meeting cuts off two regions of elevation from one region of depression, and the point itself is called a Pass. There may be in singular cases passes between more than two regions of elevation.

3rdly. As the level surface rises, the regions of elevation contract and at last are reduced to points. These points are called Summits or Tops.

Relation between the Number of Summits and Passes.

At first the whole earth is a region of elevation. For every new region of elevation there is a Pass, and for every region of elevation reduced to a point there is a Summit. And at last the whole surface of the earth is a region of depression. Hence the number of Summits is one more than the number of Passes. If S is the number of Summits and P the number of passes,

$$S = P + 1.$$  

Relation between the Number of Bottoms and Bars.

For every new region of depression there is a Bottom, and for every diminution of the number of these regions there is a Bar. Hence the number of Bottoms is one more than the number of Bars. If I is the number of Bottoms or Immits and B the number of Bars, then

$$I = B + 1.$$  

From this it is plain that if, in the singular cases of passes and bars, we reckon a pass as single, double, or n-ple, according as two, three, or $n+1$ regions of elevation meet at that point, and a bar as single, double, or n-ple, as two, three, or $n+1$ regions of depression meet at that point, then the census may be taken as before, giving each singular point its proper number. If one region of depression meets another in several places at once, one of these must be taken as a bar and the rest as passes.

The whole of this theory applies to the case of the maxima and minima of a function of two variables which is everywhere finite, determinate, and continuous. The summits correspond to maxima and the bottoms to minima. If there are $p$ maxima and $q$ minima, there must be $p + q - 2$ cases of stationary values which are neither maxima nor minima. If we regard those points in themselves, we cannot make any distinction among
them; but if we consider the regions cut off by the curves of constant value of the function, we may call \( p - 1 \) of them false maxima and \( q - 1 \) of them false minima.

**On Functions of Three Variables.**

If we suppose the three variables to be the three coordinates of a point, and the regions where the function is greater or less than a given value to be called the positive and the negative regions, then, as the given value increases, for every negative region formed there will be a minimum, and the positive region will have an increase of its periphraxy. For every junction of two different negative regions there will be a false minimum, and the positive region will have a diminution of its periphraxy. Hence if there are \( q \) true minima there will be \( q - 1 \) false minima.

There are different orders of these stationary points according to the number of regions which meet in them. The first order is when two negative regions meet surrounded by a positive region, the second order when three negative regions meet, and so on. Points of the second order count for two, those of the third for three, and so on, in this relation between the true minima and the false ones.

In like manner, when a negative region expands round a hollow part and at last surrounds it, thus cutting off a new positive region, the negative region acquires periphraxy, a new positive region is formed, and at the point of contact there is a false maximum.

When any positive region is reduced to a point and vanishes, the negative region loses periphraxy and there is a true maximum. Hence if there are \( p \) maxima there are \( p - 1 \) false maxima.

But these are not the only forms of stationary points; for a negative region may thrust out arms which may meet in a stationary point. The negative and the positive region both become cyclic. Again, a cyclic region may close in so as to become acyclic, forming another kind of stationary point where the ring first fills up. If there are \( r \) points at which cyclosis is gained and \( r' \) points at which it is lost, then we know that

\[
r = r';
\]

but we cannot determine any relation between the number of these points and that of either the true or the false maxima and minima.

If the function of three variables is a potential function, the true maxima are points of stable equilibrium, the true minima points of equilibrium unstable in every direction, and at the other
stationary points the equilibrium is stable in some directions and unstable in others.

On Lines of Slope.

Lines drawn so as to be everywhere at right angles to the contour-lines are called lines of slope. At every point of such a line there is an upward and a downward direction. If we follow the upward direction we shall in general reach a summit, and if we follow the downward direction we shall in general reach a bottom. In particular cases, however, we may reach a pass or a bar.

On Hills and Dales.

Hence each point of the earth's surface has a line of slope, which begins at a certain summit and ends in a certain bottom. Districts whose lines of slope run to the same bottom are called Basins or Dales. Those whose lines of slope come from the same summit may be called, for want of a better name, Hills.

Hence the whole earth may be naturally divided into Basins or Dales, and also, by an independent division, into hills, each point of the surface belonging to a certain dale and also to a certain hill.

On Watersheds and Watercourses.

Dales are divided from each other by Watersheds, and Hills by Watercourses.

To draw these lines, begin at a pass or a bar. Here the ground is level, so that we cannot begin to draw a line of slope; but if we draw a very small closed curve round this point, it will have highest and lowest points, the number of maxima being equal to the number of minima, and each one more than the index number of the pass or bar. From each maximum point draw a line of slope upwards till it reaches a summit. This will be a line of watershed. From each minimum point draw a line of slope downwards till it reaches a bottom. This will be a line of Watercourse. Lines of Watershed are the only lines of slope which do not reach a bottom, and lines of Watercourse are the only lines of slope which do not reach a summit. All other lines of slope diverge from some summit and converge to some bottom, remaining throughout their course in the district belonging to that summit and that bottom, which is bounded by two watersheds and two watercourses.

In the pure theory of surfaces there is no method of determining a line of watershed or of watercourse, except by first finding a pass or a bar and drawing the line of slope from that point. In nature, water actually trickles down the lines of slope, which generally converge towards the mathematical watercourses,
though they do not actually join them; but when the streams increase in quantity, they join and excavate courses for themselves; and these actually run into the main watercourse which bounds the district, and so cut out a river-bed, which, whether full or empty, forms a visible mark on the earth's surface. No such action takes place at a watershed, which therefore generally remains invisible.

There is another difficulty in the application of the mathematical theory, on account of the principal regions of depression being covered with water, so that very little is known about the positions of the singular points from which the lines of watershed must be drawn to the summits of hills near the coast. A complete division of the dry land into districts, therefore, requires some knowledge of the form of the bottom of the sea and of lakes.

**On the Number of Natural Districts.**

Let \( p_1 \) be the number of single passes, \( p_2 \) that of double passes, and so on. Let \( b_1, b_2, \&c. \) be the numbers of single, double, &c. bars. Then the number of summits will be, by what we have proved,

\[
S = 1 + p_1 + 2p_2 + \&c.,
\]

and the number of bottoms will be

\[
I = 1 + b_1 + 2b_2 + \&c.
\]

The number of watersheds will be

\[
W = 2(b_1 + p_1) + 3(b_2 + p_2) + \&c.
\]

The number of watercourses will be the same.

Now, to find the number of faces, we have by Listing's rule

\[
P - L + F - R = 0,
\]

where \( P \) is the number of points, \( L \) that of lines, \( F \) that of faces, and \( R \) that of regions, there being in this case no instance of cyclosis or periphraxy. Here \( R = 2, \) viz. the earth and the surrounding space; hence

\[
F = L - P + 2.
\]

If we put \( L \) equal to the number of watersheds, and \( P \) equal to that of summits, passes, and bars, then \( F \) is the number of Dales, which is evidently equal to the number of bottoms.

If we put \( L \) for the number of watercourses, and \( P \) for the number of passes, bars, and bottoms, then \( F \) is the number of Hills, which is evidently equal to the number of summits.

If we put \( L \) equal to the whole number of lines, and \( P \) equal to the whole number of points, we find that \( F, \) the number of natural districts named from a hill and a dale together, is equal to \( W, \) the number of watersheds or watercourses, or to the whole number of summits, bottoms, passes, and bars diminished by 2.
LI. On Solar Protuberances (being an extract from a Supplement to his second Paper). By Professor J. C. F. Zollner*.

To Dr. Francis, F.L.S. &c.

My dear Sir,

As the Government has determined to afford assistance to the expeditions that will proceed to Spain and Sicily to observe the solar eclipse in December next, I send to you without loss of time a translation of a part of a paper by Professor Zöllner,

* From No. 1772 of the Astronomische Nachrichten.

2 F 2
containing his description of an arrangement whereby the solar protuberances are rendered readily visible by means of a telescope of only 12 inches focal length combined with a spectroscope.

Yours faithfully,

W. G. Lettsom.

The magnitude of the solar image in the refractor, or, in other words, the focal length of the object-glass employed, plays a very prominent part in the entire method of observing the protuberances. It follows directly from the theory developed in a former part of this paper, that with one and the same spectroscope the contrast between the protuberance and the general ground is dependent upon the width of the slit alone. But now, as with a constant width of the aperture a so much greater part of the protuberance is seen at once the smaller the image of the sun is, it follows that we should endeavour to obtain the amplification of the protuberances we wish to observe, not by means of the solar image (that is to say, not by the employment of an object-glass of great focal length), but rather, as much as possible, by means of the lenses of the spectroscope; and this can be readily brought about by having recourse to a collimator of short focal length compared with that of the observing-telescope. Assuming, for instance, that we have a refractor of 10 feet focal length to which a spectroscope is adapted, the focal length of both object-glasses being equal—if in this state of things it is necessary to open the jaws of the slit 1 millim. in width to obtain at once a view of the whole of a protuberance of a certain extent, the opening of the slit might be reduced to one-tenth of its former width, provided the image of the sun were ten times as small; while the protuberance would still remain visible to its entire extent, and would be seen in ten times as strong contrast relatively to the ground of the spectrum; while, in order to arrive at the same amount of amplification of the protuberance in the field of view (an amplification that we have sacrificed by a diminution of the size of the sun's image), all we have to do is to give the collimator a focal length ten times as short as that of the spectroscope. We should therefore, adhering always to the instance we have selected, with the same optical amplification of the protuberance and with the same system of prisms, obtain a ten times as good effect by employing, instead of a refractor of 10 feet focal length, an instrument of only 1 foot focal length, and by giving to the collimator a focal length of about 2 inches, and to the observing-telescope a focal length of about 20 inches.

The quality of the images, as far as it is dependent on the system of lenses, would be very little affected thereby, inasmuch
as the imperfections arising from chromatic aberration are altogether absent, owing to the homogeneous quality of the light of the protuberances; and hence, as I have convinced myself by repeated experiment, non-achromatic lenses, when suitably selected, may be employed without hesitation for arrangements of the nature here contemplated.

This extremely compact form of instruments suitable for observing the solar protuberances admits of a delicate motion being given to them with facility by clockwork, and holds out the prospect of our seeing realized by these simple means very shortly the idea already broached by me in my former paper, of obtaining an artificial solar eclipse, of any duration desired, for the simultaneous observation of all the protuberances situated on the edge of the sun.

Leipzig, August 26, 1869.

---

LII. On the Principles of Thermodynamics.

By the Rev. J. M. Heath*.

I

HAVE to apologize to Mr. Rankine for attributing to him an admission which it appears he never intended to make. I understood him to mean that the period in which the best and original writers on thermodynamics had been careful observers of the true principles of the science was to be dated from the time when the revived theory of molecular vibration in gases had superseded the older one of centres of repulsive force; and I believed that this event did not happen until long after the first speculations in thermodynamics. But since Mr. Rankine disclaims having made any such admission at all in any shape, I of course acknowledge my mistake and beg to withdraw the assertion. But this point is not very material to the main purpose of the argument of this discussion. The new position from which we now start is this. Mr. Rankine contends that all good writers on thermodynamics, up to the very earliest of them, have been reasoning correctly and from properly assumed premisses; whereas my complaint is that all those same writers, down to the very latest of them, have been and are reasoning from premisses improperly assumed at the beginning. To be of any use, therefore, this discussion must now be turned to the consideration of what these assumptions are, whether or not they have been justifiable, and, if not, what others ought to have been assumed in place of them.

Mr. Rankine has stated it to be the business of the thermo-

* Communicated by the Author.

dynamist to distinguish the forces which have done mechanical work into two groups—those which have accelerated molecular motions, and those which have only "stored up energy" in some previously exhausted magazine. The words are mine and not his; but I have stated the proposition in such a form that I think he will still acknowledge it as his own, while on my side I can also assent to it, which I could not do in the form he had given to it. So far, then, I believe there is entire agreement between us. It is in the next step taken towards making this distinction among the forces that I begin to dissent from him and, I fear, from the unanimous opinion of all scientific men. That step is the adoption, as the rule required, under the name of the first law of thermodynamics, of the condition that it is those forces that "do work," according to a certain technical definition of that term, which are considered as those which "accelerate molecular motion;" and, of course, by necessary consequence, those which do no work are those which produce no motion, but "store up energy."

The counter assertion, which I rely upon being able to sustain, is that those forces only are employed in generating motion which, according to the definition of work, do no work—and that those which do work generate no motion or heat, but do "store up energy."

Before proceeding to substantiate these opinions, it is as well, to prevent misunderstanding, that I should say what I understand that definition of work done to be, against which I am protesting. I understand, then, that work is done, according to this definition, by a force P, when, in acting upon a body m, it meets with a resistance Q equal and opposite to itself, which it "overcomes" by driving m in a direction opposite to the action of Q through a space dv without increasing its vis viva. And the measure of the work so done is \( \int Q dv \), or, for the sake of simplicity, let us say Qdv. If I have misstated this definition, I shall be sincerely grateful to Mr. Rankine or any one else who will point out my error, or show me where a better one has been given. It is against this definition only that I contend; and if Mr. Rankine disowns its correctness, I am and have long been fighting against a mere shadow. But I shall proceed to examine it on the supposition that it is a correct description of what is meant by the work done by P.

The general equation of vis viva, simplified as above for our purposes, is this, \( (P - Q)dv = \frac{mv^2}{2} \). This gives us the vis viva \( \frac{mv^2}{2} \) that is generated in m when urged through a space dv by the action of a force P against a resistance Q. If P is
equal to \( Q \), as in the case we are considering, \( \frac{mv^2}{2} \) is nothing; that is, no *vis viva* is generated in a body by the action of two equal and opposite forces. I do not know how Mr. Rankine, and those whose opinions he shares, explain this fact consistently with their belief that it is under these conditions, and these only, that \( P \) accelerates molecular motions and therefore generates heat, and *does not* store up energy in \( Q \). But, "speaking under all reserve," I suppose that they contrive to continue to attribute the character of a dynamical equation to the equation \((P - Q)\delta v = 0\) which it had when it retained its full form \((P - Q)\delta v = \frac{mv^2}{2}\), and imagine that \( P \) does *drive* the body \( m \) along the length \( \delta v \) against all the efforts of \( Q \), and that it is in doing this that it generates heat. But if this is the view of any one, I think that person will not see the truth in this matter very clearly until he has discarded it. If \( P \) could by any possibility do any one thing which \( Q \) could not prevent, then the motion \( P \) would communicate to \( m \) would be accelerated, not uniform, as it is assumed to be. Those, if there are any, who deliberately maintain such an opinion as this, have not observed that \((P - Q)\delta v\) is no longer the equation of *vis viva* at all, but has become the *statical* equation of virtual velocities, and that in that equation the motion of \( m \) through \( \delta v \) is not the work of either of the two forces \( P \) or \( Q \), but any arbitrary possible motion derived from an external cause. In the dynamical equation the motion is due to the forces in action. In the statical equation it is independent of and unaffected by those forces; and the supposed case of work done is a case of *statics* only and not of dynamics. I rely upon this, therefore, for the proof of one half of my assertion, viz. that no acceleration, and therefore no heat, is generated by the action of forces which are in equilibrium, and subject to the equation of virtual velocities, or, in other words, of forces in that condition in which they are said to do work.

Mr. Rankine has adduced a mathematical demonstration, derived from the theory of the collision of elastic bodies, which he relies on as a positive argument on the other side. What I have just now given is a direct and positive proof that the force above the piston cannot accelerate, so long as it is equilibrated by the resistance below. Mr. Rankine's argument is, in form, equally direct and positive to prove that it does. I shall simply point out a very important mistake which vitiates the whole of his proof; and unless he can maintain that his reasoning is sound, the proposition he attempts to support must be taken as disproved. His mistake is this. He represents the piston, in fact, as imparting
vis viva to the particles without losing any of its own — contrary to the principle which he himself appeals to, that of the conservation of force. He finds \(+ (u + v)\) for the velocity of the particle relatively to the piston after impact; and to get the absolute velocity of the particle, he adds this to \(u\), the velocity of the piston before the impact, forgetting that that velocity has been altered by the impact. The consequence of this oversight is that the velocity of the centre of gravity of the two bodies, and also the sum of their vis viva, are both greater after impact than before it—results impossible according to the true laws of impact. I claim, therefore, at least for the present, to say that Mr. Rankine has failed to prove that his proposition is true, and that I have tendered a proof, as yet uncontroverted, that it is false.

I will now pursue the remainder of the main argument, and examine the contrast between my own opinions and those represented by Mr. Rankine, as to cases where the force meets with no resistance, and does, therefore, no work. When \(Q = 0\), the general equation of vis viva takes the form \(P\delta v = \frac{mv^2}{2}\); and it is obvious that in this case all the force is employed in producing acceleration, and none of it in "storing up energy." All the force, to use the ordinary language of mechanics, is employed dynamically, or is doing dynamical work, and none is producing statical work, which is pressure. If a gun-barrel is placed horizontally and no friction or atmospheric resistance acts to resist the motion of the ball through its length, the vis viva it has at the muzzle measures the expenditure of the internal energy of the exploded charge, which has become externalized, and is now no longer internal energy in the gas, but actual energy of motion in the bullet. All this, therefore, is energy lost to the gas. But, say the thermodynamists, this gas has met with no resistance, done therefore no work, and can have lost no heat. Have I misrepresented them? Or how do they explain such a monstrous conclusion?

Lastly, I will take the equation in its general form, where forces are employed in both the ways that we have now considered separately, viz. both statically and dynamically, in producing both pressure and motion. The use of this equation, properly treated, will be to enable us to distinguish these forces into two groups, which, as Mr. Rankine tells us, is the business of thermodynamics, but which I contend has not as yet been properly done.

The equation \((P - Q)\delta v = \frac{mv^2}{2}\) may be written

\[
(P - Q' + Q' - Q)\delta v = \frac{mv^2}{2}.
\]
The Rev. J. M. Heath on the Principles of Thermodynamics. 433

And this, again, if we suppose $Q' = Q$, may be resolved into two separate equations, a statical one,

$$(Q'-Q)\delta v = 0, \quad \ldots \ldots \ldots (1)$$

and a dynamical one,

$$(P-Q')\delta v = \frac{mv^2}{2}. \quad \ldots \ldots \ldots (2)$$

Whenever, therefore, $P$ is not equal to (here supposed greater than) $Q$, we may resolve it into two parts, $Q'=Q$, which is therefore the reaction of the lower surface of the piston equal and opposite to the pressure of the gas below, and $P-Q'$, the remaining part of $P$, which acts upon the particles of the gas as if they were perfectly free. The effect of $P-Q'$ upon the gas is therefore wholly dynamical; it does not alter the pressure, and its entire effect is the generation of motion. Thermodynamists teach (as far as I may venture to speak of teachings which I can give no mental assent to, and may therefore unintentionally misrepresent in consequence of misunderstanding them) that the whole gain of heat is determined by the first, or statical one, of our two equations. The force $Q'$ (part of $P$) presses down the piston through $\delta v$ against the resistance $Q$, and the work done is $Q\delta v$, and it is done by $Q'$, a part only of $P$; and the remainder, $P-Q'$, being an unresisted force, has no effect either on the work done or on the gain of heat. I think this is a true representation of what they would say, because it appears to me to be what they must say if the case is reversed, and we consider, instead of a condensation by piston, the case of expansion, as the discharge of a bullet from a gun-barrel. If $P$ is considered to represent the explosive force of the charge of a gun, and $Q$ the resistance arising from friction, the weight of the bullet, and the pressure downwards of the external air, then $Q$ is a fixed quantity, and the whole energy $P\delta v$ expended can do no more work than $Q\delta v$; that is to say, that the largest charge that can be put into the gun can do, technically, no more work, and can therefore expend no more heat, than that small charge $Q'$ which is just able to counterbalance $Q$, so that the ball may move uniformly along the barrel and fall to the ground when it reaches the muzzle. In this case I apprehend thermodynamists must relegate the excess of gas-force, $P-Q'$, over the resistance, into that group which we have already discussed, of forces which do no work and exhaust no heat.

The truth is, I have never seen anywhere any recognition among writers on this subject of the case of unequal forces acting in this way in condensing or in expanding the gas; and although the opinion is wholly untenable, I believe that the general conception is, at all events when the load on the piston descends
and condenses, that the reaction of the gas is always equal to the pressure upon the piston, whatever it may be. That this opinion, however, is wrong is evident from this, that if the pressure of the gas were always equal to the external pressure put upon it by the piston, the gas would never yield and be condensed by the descent of the piston, but would continue to sustain it. It appears to me to be certain that the internal pressure of a gas is never any thing else than that given by Mariotte’s law as depending upon the density and the temperature only, and that any additional external force applied to its surface acts dynamically, and not statically, upon it.


The equation of Laplace’s coefficients has attracted the attention of three distinguished English mathematicians, all of whom within a comparatively brief space have passed from the scene—the late Judge Hargreave, Mr. Boole, and Professor Donkin.

Having adopted Laplace’s method of transformation and reduction, Mr. Hargreave gave, in the Philosophical Transactions for 1841, the first solution of the problem in finite terms.

One peculiarity of this method is that the equation actually integrated by it is not that which was originally proposed, but a derivative from the latter, the problem solved being in fact vastly more general than that proposed for solution—a circumstance of which the solution obtained by it affords ample proof†.

Proceeding by the method of separation of symbols of operation from those of quantity, Mr. Boole, by each of three independent methods, arrived at an expression for the integral; and one of great elegance, obtained on the same principle, was given by Professor Donkin in the Philosophical Transactions for 1857.

In a paper published in the Philosophical Magazine for July last I showed that the equation

\[ 0 = R \frac{d^2z}{dx^2} + S \frac{d^2z}{dx dy} + T \frac{d^2z}{dy^2} + P \frac{dz}{dx} + Q \frac{dz}{dy} + U z, \]  

where \( R, S, T, P, Q, U \) are functions of \( x \) and \( y \) only, in the cases which are not amenable to Monge’s method will always have an integral in which \( z \) is represented by a series (finite or infinite according to circumstances) of the form

\[ z = A X(u) + A_1 \int dwX(u) + A_2 \int \int dw dwX(u) + \&c., \]

* Communicated by the Author.
† For instance, the expression thus obtained for the fiftieth coefficient involves upwards of fifty arbitrary functions.
or by a pair of such series; where \( \chi \) is arbitrary, \( u \) is determined by the equation

\[
0 = R \left( \frac{du}{dx} \right)^2 + S \frac{du}{dx} \frac{du}{dy} + T \frac{du}{dy}^2,
\]

\[
A = e^{-\beta dx},
\]

\[
A_1 = -e^{-\beta dx} \int dx \gamma_1 e^{\beta dx},
\]

\[
A_2 = -e^{-\beta dx} \int dx \gamma_2 e^{\beta dx},
\]

\&c. \quad \&c.,

where

\[
\beta = \frac{R \frac{d^2 u}{dx^2} + S \frac{d^2 u}{dxdy} + T \frac{d^2 u}{dy^2} + P \frac{du}{dx} + Q \frac{du}{dy}}{2R \frac{du}{dx} + S \frac{du}{dy}},
\]

\[
\gamma_1 = \frac{R \frac{d^2 A}{dx^2} + S \frac{d^2 A}{dxdy} + T \frac{d^2 A}{dy^2} + P \frac{dA}{dx} + Q \frac{dA}{dy} + UA}{2R \frac{dA}{dx} + S \frac{dA}{dy}},
\]

\[
\gamma_2 = \frac{R \frac{d^2 A_1}{dx^2} + S \frac{d^2 A_1}{dxdy} + T \frac{d^2 A_1}{dy^2} + P \frac{dA_1}{dx} + Q \frac{dA_1}{dy} + UA_1}{2R \frac{dA_1}{dx} + S \frac{dA_1}{dy}},
\]

\&c. \quad \&c.

The equation of Laplace’s coefficients may readily be integrated by this general method, some of the peculiarities of which are well illustrated by its application to that equation, of which it probably offers the simplest solution which is obtainable.

Retaining the original spherical coordinates, the equation may be written

\[
0 = \frac{d^2 \omega}{d\phi^2} + \sin^2 \theta \frac{d^2 \omega}{d\theta^2} + \sin \theta \cos \theta \frac{d\omega}{d\theta} + n \cdot n + 1 \cdot \sin^2 \theta \cdot \omega;
\]

and putting \( \theta \) and \( \phi \) for \( x \) and \( y \) respectively in the preceding formulæ, we shall find

* In the paper above referred to, I have stated “that no constants are to be introduced” in effecting the integrations here indicated. This is true so long as in (1) we have \( U = 0 \); but where this does not hold, the omission of the constants would greatly curtail the generality of the result, as a glance at the mode in which each of the quantities \( A_1, A_2, \&c. \) is derived from its immediate predecessor will at once show.
Mr. R. Moon on the Equation of Laplace’s Coefficients.

\[ u = \pm \phi \sqrt{-1 + \log \left( \frac{\tan \theta}{2} \right)} \]
\[ \beta = 0, \]
\[ \gamma_1 = \frac{1}{2} \left( \sin \theta \frac{d^2 A}{d\theta^2} + \cos \theta \frac{dA}{d\theta} + n \cdot \frac{n+1}{2} \sin \theta \cdot A \right), \]
\[ \gamma_2 = \frac{1}{2} \left( \sin \theta \frac{d^2 A_1}{d\theta^2} + \cos \theta \frac{dA_1}{d\theta} + n \cdot \frac{n+1}{2} \sin \theta \cdot A \right), \]
\&c. \quad \&c.,

whence we have
\[ A = c, \]
\[ A_1 = -\int \gamma_1 d\theta = -\frac{1}{2} \left( \sin \theta \frac{dA}{d\theta} + n \cdot \frac{n+1}{2} \int \sin \theta \cdot A \cdot d\theta \right), \]
\[ A_2 = -\int \gamma_2 d\theta = -\frac{1}{2} \left( \sin \theta \frac{dA_1}{d\theta} + n \cdot \frac{n+1}{2} \int \sin \theta \cdot A_1 \cdot d\theta \right), \]
\[ A_3 = -\int \gamma_3 d\theta = -\frac{1}{2} \left( \sin \theta \frac{dA_2}{d\theta} + n \cdot \frac{n+1}{2} \int \sin \theta \cdot A_2 \cdot d\theta \right), \]
\&c. \quad \&c.;

or, substituting for \( A \) and performing the integrations,
\[ A_1 = \frac{1}{2} \left\{ c \cdot \frac{n \cdot \frac{n+1}{2}}{1} \cdot \cos \theta + c_1 \right\}, \]
\[ A_2 = \frac{1}{2^2} \left\{ c \cdot \frac{n \cdot \frac{n+1}{2} \left( n \cdot \frac{n+1}{2} - 1 \cdot 2 \right) \cos^2 \theta + c_1 \cdot \frac{n \cdot \frac{n+1}{2}}{1} \cos \theta + c_2 \right\}, \]
\[ A_3 = \frac{1}{2^3} \left\{ c \cdot \frac{n \cdot \frac{n+1}{2} \left( n \cdot \frac{n+1}{2} - 1 \cdot 2 \right) \left( n \cdot \frac{n+1}{2} - 1 \cdot 2 \cdot 3 \right) \cos^3 \theta}{1 \cdot 2 \cdot 3} + c_1 \cdot \frac{n \cdot \frac{n+1}{2} \left( n \cdot \frac{n+1}{2} - 1 \cdot 2 \right) \cos^2 \theta}{1 \cdot 2} + \left( c \cdot n \cdot \frac{n+1}{2} \left( n \cdot \frac{n+1}{2} - 1 \cdot 2 \right) + c_2 \cdot \frac{n \cdot \frac{n+1}{2}}{1} \cos \theta + c_3 \right\} \]
\&c. \quad \&c.

If the condition be imposed that the series shall terminate, it may be satisfied by means of the constants introduced in the subsidiary integrations.

* Instead of taking for the base of the arbitrary function the above value of \( u \), we may take \( e^\pm u \) for the base where \( u \) has the above value, the form so obtained being identical with that given by Professor Donkin. Its adoption, however, adds materially to the complexity of the result.
To effect this, let \( A_m \) be the coefficient of the last term of the series for \( \omega \); then from what has preceded it is evident that we may assume

\[
A_m = a_m \cos \theta^m + a_{m-1} \cos \theta^{m-1} + a_{m-2} \cos \theta^{m-2} + \text{&c.},
\]

where \( a_m, a_{m-1}, \text{&c.} \) are constants; and since by hypothesis \( A_{m-1} = 0 \), the formula (2) give us

\[
0 = \sin \theta \frac{dA_m}{d\theta} + n \cdot n + 1 \int \sin \theta \cdot A_m \, d\theta.
\]

The substitution in this equation of the above expression for \( A_m \) gives

\[
0 = ma_{m+1} \cos \theta^{m+1} + m-1a_{m-1} \cos \theta^m + m-2a_{m-2} \cos \theta^{m-1} + m-3a_{m-3} \cos \theta^{m-2} + \text{&c.}
\]

\[
- ma_m \cos \theta^m - m-1a_{m-1} \cos \theta^{m-2} + \text{&c.}
\]

\[
-n \cdot n + 1a_m \cos \theta^{m+1} - n \cdot n + 1a_{m-1} \cos \theta^m - n \cdot n + 1a_{m-2} \cos \theta^{m-1} - n \cdot n + 1a_{m-3} \cos \theta^{m-2} + \text{&c.}
\]

whence we derive the equations

\[
0 = (m - \frac{n \cdot n + 1}{m + 1}) a_m,
\]

\[
0 = (m - 1 - \frac{n \cdot n + 1}{m}) a_{m-1},
\]

\[
0 = (m - 2 - \frac{n \cdot n + 1}{m - 1}) a_{m-2} - ma_m,
\]

\[
0 = (m - 3 - \frac{n \cdot n + 1}{m - 2}) a_{m-3} - m - 1a_{m-1},
\]

\&c.

and, ultimately, the following, viz.:

\[
\begin{align*}
m &= n, \\
& a_{m-1} = 0, \\
& a_{m-2} = -\frac{n \cdot n - 1}{n \cdot n + 1 - n - 1 n - 2} a_m, \\
& a_{m-3} = 0, \\
& a_{m-4} = \frac{n \cdot n - 1 n - 2 n - 3}{(n \cdot n + 1 - n - 1 n - 2)(n \cdot n + 1 - n - 3 n - 4)} a_m, \\
& \&c.
\end{align*}
\]
from which the law of formation of the terms in the series for \( A_m \) or \( A_n \) is obvious. These being known, \( A_{n-1} \) can be determined by means of the equation

\[
A_n = -\frac{1}{2} \left( \sin \theta \frac{dA_{n-1}}{d\theta} + n \cdot n + 1 \int \sin \theta A_{n-1} d\theta \right),
\]

or

\[
0 = 2A_n + \sin \theta \frac{dA_{n-1}}{d\theta} + n \cdot n + 1 \int \sin \theta A_{n-1} d\theta ; \quad (3)
\]

for, from what has preceded, it is evident that we may assume

\[
A_{n-1} = b_{n-1} \cos \theta^{n-1} + b_{n-3} \cos \theta^{n-3} + b_{n-5} \cos \theta^{n-5} + \&c.
\]

(where \( b_{n-1}, b_{n-3}, \&c. \) are constants); and substituting this value in (3), we shall get

\[
0 = 2a_n \cos \theta^n + 2a_{n-2} \cos \theta^{n-2} + 2a_{n-4} \cos \theta^{n-4} + \&c.
\]

\[
+ n-1 b_{n-1} \cos \theta^n + n-3 b_{n-3} \cos \theta^{n-2} + n-5 b_{n-5} \cos \theta^{n-4} + \&c.
\]

\[
- n-1 b_{n-1} \cos \theta^n - n-3 b_{n-3} \cos \theta^{n-2} - n-5 b_{n-5} \cos \theta^{n-4} - \&c.
\]

whence, equating to zero the coefficients of the different powers of \( \cos \theta \), we derive the following:

\[
\begin{align*}
 b_{n-1} & = \frac{2na_n}{n \cdot n + 1 - n \cdot n - 1}, \\
 b_{n-3} & = \frac{2n-2 a_{n-2} - n \cdot n - 2 b_{n-1}}{n \cdot n + 1 - n \cdot 2 - 3}, \\
 b_{n-5} & = \frac{2n-4 a_{n-4} - n \cdot n - 4 b_{n-3}}{n \cdot n + 1 - n \cdot 4 - 5},
\end{align*}
\]

\( \&c. \quad \&c. \)

In like manner, if we assume

\[
A_{n-2} = c_{n-2} \cos \theta^{n-2} + c_{n-4} \cos \theta^{n-4} + c_{n-6} \cos \theta^{n-6} + \&c.,
\]

we may determine the coefficients \( c_{n-2}, c_{n-4}, \&c. \) by means of the equation

\[
A_{n-1} = -\frac{1}{2} \left( \sin \theta \frac{dA_{n-2}}{d\theta} + n \cdot n + 1 \int \sin \theta A_{n-2} d\theta \right),
\]

or

\[
0 = 2A_{n-1} + \sin \theta \frac{dA_{n-2}}{d\theta} + n \cdot n + 1 \int \sin \theta A_{n-2} d\theta ;
\]
whence, proceeding as before, we find

\[
\begin{align*}
\frac{2n-1}{n} b_{n-1} c_{n-2} &= \frac{2n-1 b_{n-1}}{n-1} n-2, \\
\frac{2n-3}{n} b_{n-3} c_{n-4} &= \frac{2n-3 b_{n-3}}{n-2} n-3 c_{n-2}, \\
\frac{2n-5}{n} b_{n-5} c_{n-6} &= \frac{2n-5 b_{n-5}}{n-4} n-5 c_{n-4}, \\
&\quad \text{&c.}
\end{align*}
\] . (5)

Hence, putting \( T \) for \( \log_e \left( \tan \frac{\theta}{2} \right) \), and \( \Psi(u) \) for \( \int \int \cdots \int \mathrm{d}v \cdot \chi(u) \), we have the following expression for \( \omega \), viz.

\[
\omega = \left( a_n \cos \theta \right)^n + a_{n-2} \cos \theta^{n-2} + \text{&c.} \cdot \left\{ \begin{array}{l}
\Psi_1(T+\phi \sqrt{-1}) \\
+ \Psi_2(T-\phi \sqrt{-1})
\end{array} \right\} + \left( b_{n-1} \cos \theta \right)^{n-1} + b_{n-3} \cos \theta^{n-3} + \text{&c.} \cdot \left\{ \begin{array}{l}
\Psi_1'(T+\phi \sqrt{-1}) \\
+ \Psi_2'(T-\phi \sqrt{-1})
\end{array} \right\} + (c_{n-2} \cos \theta^{n-2} + c_{n-4} \cos \theta^{n-4} + \text{&c.}) \cdot \left\{ \begin{array}{l}
\Psi_1''(T+\phi \sqrt{-1}) \\
+ \Psi_2''(T-\phi \sqrt{-1})
\end{array} \right\} + \text{&c.}
\]

where, if we choose to put \( a_n = 1 \), as we may do, we shall have

\[
\begin{align*}
a_n &= 1, \\
a_{n-2} &= -\frac{n \cdot (n-1)}{n \cdot (n-1) n-2}, \\
a_{n-4} &= \frac{n \cdot (n-1) n-2 n-3}{(n \cdot (n-1) n-2) (n \cdot (n+1) n-3 n-4)}, \\
a_{n-6} &= \frac{n \cdot (n-1) n-2 n-3 n-4 n-5}{(n \cdot (n+1) n-1 n-2) (n \cdot (n+1) n-3 n-4) (n \cdot (n+1) n-5 n-6)}, \\
&\quad \text{&c.}
\end{align*}
\]

and where the constants \( b_{n-1}, b_{n-3}, \text{&c.}, c_{n-2}, c_{n-4}, \text{&c.} \), taken in order, are determined by equations (4) and (5), &c. in terms of known quantities.
Dr. W. J. M. Rankine on the Meteor of November 19, 1870.

If \( n \) be even, the last two terms of the series will be of the form

\[
+ \left( h \cos^2 \theta + k \right) \left\{ \psi_1^{(n-1)}(T + \phi \sqrt{-1}) + \psi_2^{(n-1)}(T - \phi \sqrt{-1}) \right\} + l \left\{ \psi_1^{(n)}(T + \phi \sqrt{-1}) + \psi_2^{(n)}(T - \phi \sqrt{-1}) \right\}.
\]

If \( n \) be odd, the last two terms will be

\[
+ \left( h_1 \cos^3 \theta + k_1 \cos \theta \right) \left\{ \psi_1^{(n-1)}(T + \phi \sqrt{-1}) + \psi_2^{(n-1)}(T - \phi \sqrt{-1}) \right\} + l_1 \left\{ \psi_1^{(n)}(T + \phi \sqrt{-1}) + \psi_2^{(n)}(T - \phi \sqrt{-1}) \right\},
\]

where \( h, k, l; h_1, k_1, l_1 \) are known constants.

Lincoln’s Inn, November 12, 1870.
LV. On a New Method of determining Resistances.
By Thomas T. P. Bruce Warren*.

The following method of measuring resistances may be of interest to electricians, as it supplies a means of ascertaining resistances for a wide range without using a set of resistance-coils, it being necessary to have only one resistance-coil of known value.

A condenser, charged from a constant battery, is discharged through a galvanometer of known resistance, shunted with an unknown resistance \(x\); a known resistance \(a\) is then added to the shunt, and the discharge again taken. Assuming the deflections of the needle to be proportional to the quantities of current, it is required to determine the value of the resistance \(x\), and also its shunt-power.

Resistance of galvanometer . . . . = 5465 B.A. units.

\[ \frac{5465 + x}{x} : \frac{5465 + (x + 300)}{x + 300} :: 347 : 228, \]

or

\[ \frac{5465 + x}{x} = \frac{5465 + (x + 300)}{x + 300} \cdot 1.522, \]

which gives for \(x\) 500 B.A. units, and 11.93 for its shunt-power. The determination of \(x\) may be very much simplified if the galvanometer be first shunted with the known resistance and afterwards with the unknown resistance, or the two resistances conjointly.

Let \(a\) (the known resistance) . . . . . = 500 B.A. units.

Discharge from condenser with 500 units shunt = 228 divisions.

\[ \frac{5465 + 500}{500} = \left( \frac{5465 + x}{x} \right) \frac{347}{228}, \]

* Communicated by the Author.

or

\[
\frac{5465 + x}{x} = \frac{11.93}{1.522} = 7.83 \text{ shunt-power of } x.
\]

Resistance of \( x = \frac{G}{n-1} = \frac{5465}{6.83} = 800 \text{ B.A. units.} \)

As this method does not involve direct completion of the battery-circuit, it is adapted to the determination, more especially, of the resistances of liquids which are subject to electrolysis.

If the galvanometer-needle be deflected when the terminals are immersed in a liquid, it may be brought to zero in the ordinary way. Such a current will not interfere with the results, so long as it acts without interruption.

In these cases, and also for the measurement of the internal resistance of batteries, \( a + x \) together with the liquid should be inserted as a shunt, and \( x \) afterwards varied by a known quantity.

When measuring the internal resistance of a battery-cell, it may be necessary to insert a small but known resistance between its poles, so as to diminish the action of its current on the needles. The following illustration will explain this operation:

\[
\text{Resistance of galvanometer} = 5384 \text{ B.A. units.}
\]

\[
\text{battery-cell (shunted)} = x
\]

\[
a \text{ added to battery-cell} = 40
\]

\[
\text{interposed between poles of battery} = 1 \text{ B.A. unit.}
\]

Discharge from condenser, galvanometer shunted with \( (a + x) \) = 180 divisions.

\[
(a + 44 + x) = 360
\]

writing \( x \) for \( a + x \),

\[
\frac{5384 + x}{x} = \left( \frac{5384 + (44 + x)}{44 + x} \right)^2, \text{ or } x = 45.2,
\]

\[
a = 40; \therefore a - x = 5.2;
\]

and by the formula for the derived circuit,

\[
\frac{40 \times x \times 1}{40 + x + 1} = 5.2,
\]

from which we obtain 6.1 B.A. units for the resistance of the cell.

This method also supplies a simple means of arriving at the position and resistances of faults in insulated wires or cables. The most accurate results are obtained when the two ends of the cable or core are available for simultaneous operation. With submerged cables it will be more satisfactory to perform the tests from each end; but if this be impossible, the distant end should
be put to earth, when required, by an attendant stationed at that end.

The coil or shunt of known value should be, preferably, equal to that of the conductor of the cable. The battery-power required must depend on the resistances of the fault and conductor of the cable. The resistance of the conductor must be carefully ascertained.

In the case of iron-covered cables, it will be difficult to connect the ends of the cable to the galvanometer terminals without interfering with the zero-position of the needles. The needles may be deflected by a current set up between the copper conductor and the iron sheathing of the cable, or even by the tanks themselves.

When the resistance of the fault is low, the deflected position of the needle may be taken as zero, or the needle may be set to zero by the aid of other magnets. Care must be taken that the electromotive force of this current shall be insignificant when compared with that of the testing-battery.

Supposing the resistances of the galvanometer and conductor to be known, and the two ends of the cable to be led into the testing-room, the connexions will be made alternately as shown in figs. 1 and 2.

Let \( R \) = resistance of conductor \( A B \),
\[ G = \text{galvanometer}, \]
\[ r = \text{from end } A \text{ to } x, \text{ including } x, \]
\[ r' = \text{from } B \text{ to } x, \text{ including } x, \]
\( x \) being the resistance of the fault.

If a condenser, first charged from a constant battery, be discharged through the galvanometer shunted with \( R, r, r' \) consecutively, the deflections will be proportional to the shunt-powers of these resistances respectively, or to
\[ \frac{G + R}{R}, \frac{G + r}{r}, \frac{G + r'}{r'}; \]
\[ 2 G 2 \]
On a New Method of determining Resistances.

and the value for \( \frac{G+R}{R} \) being known, the values of the others may be easily obtained from the observed discharges.

The shunt-powers being obtained, the values for \( r \) and \( r' \) may be eliminated from the following formula, in which \( S = \) shunt-power or multiplying-powers of shunt,

\[
S - 1 = \frac{G}{R};
\]

then as

\[
r = Ax + x,
\]

\[
r' = Bx + x,
\]

\[
r + r' = Ax + Bx + 2x,
\]

or

\[
= R + \text{twice the resistance of the fault}.
\]

Deducting the resistance of the fault from \( r \) or \( r' \), the distance of the fault from either end will be obtained in units of resistance.

**Experiment (see figs. 1 and 2).**

Galvanometer resistance \( = 5480 \) B.A. units.

Resistance of coil \( = 49.39 \) "

Discharge from condenser, galvanometer shunted with coil (49.39) \( = 278 \) divisions.

Discharge from condenser, galvanometer shunted with \( Ax \) (B end free) \( = 209 \) "

Discharge from condenser, galvanometer shunted with \( Bx \) (A end free) \( = 86 \) "

Shunt values: \( AB = 112.0 \), resist. \( = 49.39 \) units.

\( Ax = 149.0 \) " \( = 36.8 \) "

\( Bx = 362.0 \) " \( = 15.2 \) "

\( Ax + Bx = 36.8 + 15.2 = 52.0 \)

\[ 52.0 - 49.39 = 2.6; . . . \text{resist. of fault} = 1.3 \) unit.

\[ 36.8 - 1.3 = 35.5 \) units = distance of fault from A,

\[ 15.2 - 1.3 = 13.9 \) " \( \) " \( \) B.

Tamworth House,
Mitcham Common.
LVI. Notices respecting New Books.


This is the first published of a series of Text-books designed not merely as school books, but as elementary treatises which, while strictly scientific, shall exhibit the practical applications of the theories they expound. The Series will comprise about sixteen volumes; and though several are announced as “nearly ready,” some time will probably elapse before the whole is completed.

It is rather unfortunate that the first published of the series, however excellent in itself, should be a new edition, and not a completely new work, as we understand the remaining volumes of the series are to be. But though a new edition of a work first published in the year 1860, it is by no means a mere reprint, but has been rewritten and greatly enlarged. In fact it contains nearly twice as much matter as the first edition. The general arrangement and scope of the two are the same, the enlargement being effected by the expansion of some articles, and the insertion of additional articles here and there. Thus:—The introduction has been expanded from seven to eighteen pages. In the first edition there is an article (156) on Hooke’s joint; this is given almost unchanged in art. 171; but there is added an article of nearly equal length (172) on the effect of interposing a double joint between the axes. The articles on the parallel motion are nearly the same in the two editions; but in the present an additional article (180) gives a brief account of the modification required to adapt the parallel motion to an engine worked by both a high- and a low-pressure cylinder; and so on in other cases.

It need scarcely be added that great improvements are hereby introduced. The work is written with great clearness and a thorough knowledge of the subject; and though essentially a treatise on a particular branch of Geometry, it will be readily intelligible to a reader possessing no more than a very moderate acquaintance with abstract mathematics. Its purely elementary character has rendered necessary a certain want of system in the arrangement; but a reader who has mastered its contents will find it an excellent guide for making out the complicated arrangements of machinery actually in use. Should the succeeding volumes of the series preserve the same practical and elementary character, they will be most useful aids to scientific education.
THE following communications were read:

"On the Theory of Continuous Beams." By John Mortimer Heppel, M. Inst. C.E.

In venturing to present to the Royal Society a paper on a subject which has engaged the attention, more especially in France, of some of the most eminent engineers and writers on Mechanical Philosophy, the author feels it to be incumbent on him to state the nature of the claim to their attention which he hopes it may be found to possess in point of originality or improvement on the method of treatment.

To do this clearly, however, it will be necessary to advert to the principal steps by which progress in the knowledge of this subject has been made, both in France and in this country.

The theory of continuous beams appears to have first attracted attention in France about 1825, when a method of determining all the conditions of equilibrium of a straight beam of uniform section throughout, resting on any number of level supports at any distances apart, each span being loaded uniformly, but the uniform loads varying in any manner from one span to another, was investigated and published by M. Navier. This method, although perfectly exact for the assumed conditions, was objectionable from the great labour and intricacy of the calculations it entailed. Messrs. Molinos and Pronnier, in their work entitled 'Traité Théorique et Pratique de la construction des Ponts Métalliques,' describe this process fully, and show that for a bridge of $n$ openings, the solution must be effected of $3n+1$ equations, involving as many unknown quantities, these equations being themselves of a complex character; and they observe, "Thus, to find the curve of the moments of rupture for a bridge of 6 spans, 19 equations must be operated on; such calculations would be repulsive; and when the number of spans is at all considerable this method must be abandoned."

The method of M. Navier, however, remained the only one available till about 1849, when M. Clapeyron, Ingénieur des Mines, and Member of the Academy of Sciences, being charged with the construction of the Pont d'Asnières, a bridge of five continuous spans over the Seine, near Paris, applied himself to seek some more manageable process. He appears to have perceived (and, so far as the writer is informed, to have been the first to perceive) that if the bending-moments over the supports at the ends of any span were known as well as the amount and distribution of the load, the entire mechanical condition of this portion of the beam would become known just as if it were an independent beam. Upon this M. Clapeyron proceeded to form a set of equations involving as unknown quantities the bending-moments over the supports, with a view to their determination. He found himself, however, obliged to introduce into
these equations a second set of unknown quantities ("inconnues auxiliaries"), being the inclinations of the deflection-curve at the points of support, and, not having arrived at a general method of eliminating these latter, was obliged to operate in each case on a number of equations equal to twice the number of spans. M. Clapeyron does not appear, as yet, to have made any formal publication of his method, but to have used it in his own practice, and communicated it freely to those with whom he came into contact.

In 1856, M. Bertot, Ingénieur Civil, appears to have found the means of eliminating this second set of unknown quantities \( n + 1 \) in number for a bridge of \( n \) spans, and thus reducing the number of equations to \( n - 1 \).

Each of these equations involved as unknown quantities the bending-moments over three consecutive supports, and was considered, from its remarkable symmetry and simplicity, to merit a distinctive name, that of "The Theorem of the three Moments."

The method, however, to which this theorem is the key, is still everywhere called that of M. Clapeyron, and, as it appears to the writer, justly so, as it was an immediate and simple result from his investigations, with which M. Bertot was well acquainted.

The next important advance was made in 1861, when M. Bresse, Professeur de Mécanique appliquée à l’École Impériale des Ponts et Chaussées, completed the matter of the third volume of his course, which is exclusively devoted to this subject*. M. Bresse explains and demonstrates the theorem of the three moments, at the knowledge of which he had himself arrived from M. Clapeyron’s investigations, independently of M. Bertot. He then goes on to the investigation of an equation of much greater generality, in which what is termed by English writers "imperfect continuity" is taken into account, being, however, there replaced by the precisely equivalent notion of original differences of level in the supports, the beam being always supposed primitively straight; besides this the loads, instead of being taken as uniform for each span, are considered as distributed in any given manner.

Having obtained this fundamental equation, M. Bresse proceeds to investigate the nature of the curves which are the envelopes of the greatest bending moments produced at each point by the most unfavourable distribution of the load in reference to it, and, finally, gives tables for the ready calculation of results in a great variety of cases, comprising most of those likely to occur in practice.

During the time that M. Bresse was engaged in these researches, an Imperial Commission was formed, of which he was a member, for the purpose of devising rules applicable to practice; and the results of his labours have been the basis of legislative enactments equivalent to our Board-of-Trade regulations, prescribing the methods to be followed in determining the stresses in the various parts of the structure.

About the same time that M. Bresse turned his attention to this

* This was communicated to the Academy of Sciences in 1862, though the volume was not published till 1865.
subject, it appears also to have engaged that of M. Bélanger, who in
his work entitled 'Théorie de la Résistance et de la Flexion Plane
des Solides &c.,' Paris, 1862, gives a very complete demonstration—
resulting in an equation which in one point of view is slightly more
general than that of M. Bresse, as it takes in variation of the moment
of inertia of the section from one span to another. In another point
of view its generality is slightly less, as it deals only with loads
distributed over each separate span uniformly, whereas M. Bresse
replaces the simple algebraical terms expressing these by definite
integrals expressing the load as a function of the distance from one
of the points of support.
As far as the writer is informed, little has been done in France
to advance this theory beyond the point to which it was brought
by the writers last mentioned, and especially by M. Bresse; but
valuable contributions to its development in reference to application
to practice are to be found in the work of MM. Mollinos and Pronnier
above referred to, as well as in various papers by MM. Renaudot,
Albaret, Colignon, Piarron de Mondesir, &c.
In England little or no attention appears to have been paid to this
subject by writers on mechanics till 1843, when the Rev. Henry
Moseley, Professor of Natural Philosophy and Astronomy at King's
College, London, published his work on the Mechanical Principles
of Engineering and Architecture. In part 5 of this work, which
treats of the strength of materials, four cases of continuous beams
are fully investigated, and the general case is to a certain extent
discussed, the method of M. Navier being perhaps rather indicated
than fully developed.
Prof. Moseley's work was altogether a most valuable contribution
to engineering science, and, as far as the present subject is con-
cerned, no doubt furnished the groundwork of the method applied
by Mr. Pole to the solution of other particular but more complex and
difficult cases.
The first case which engaged the attention of Mr. Pole appears to
have been that of the bridge over the Trent at Torksey, consisting
of two spans of continuous tubular beams, resting on abutments
and a central pier. For special reasons it had become necessary
that the real conditions of equilibrium of this bridge should be in-
vestigated with more than ordinary precision; and this Mr. Pole did
by a method virtually identical with that of M. Navier, though it
does not appear that he had any previous knowledge of that method,
except through the medium of Moseley's work. Throughout Mose-
ley's cases, however, the load on the beam is considered as distributed
uniformly over its entire length, whereas Mr. Pole had to deal with
the case of different loads on the two spans, and, no doubt, had to
device the method of analysis necessary for its treatment. Mr.
Pole's paper on this subject is published in vol. ix. of the 'Minutes
As far as this went, however, it could hardly be considered to
have advanced the theory of the subject, as M. Navier's method
included this case, and much more; but about the same time Mr.
Pole had to investigate the case of a much larger work, the Britannia Bridge, where he had to deal with some new conditions, which, as far as the writer is aware, were then for the first time successfully treated.

These were that, besides variation of load on the different spans, their cross sections also varied, and there was imperfect continuity over the centre pier—that is to say, the points of support being supposed to range in a straight line, the beam if relieved from all weight would cease to remain in contact with them all, and would consist of two equal straight portions, forming an angle pointing upwards. The process which, for distinction, may be called that of M. Navier was skilfully extended by Mr. Pole so as to include these new circumstances; and by its means results were obtained certainly true within a very small limit, and as near the absolute truth as any existing means of treating the subject would produce.

Mr. Pole’s researches on this subject are published in Mr. Edwin Clark’s work on the Britannia and Conway Bridges, 1850. Both from the clear and accurate treatment of the case and the record of the numerous and delicate observations by which the theoretical conclusions were continually verified and kept in check, they are most strongly to be recommended to the attention of engineers having to deal with works of this character.

The sequence of events now compels the writer to advert to some studies of his own. In 1858–59, being then Chief Engineer of the Madras Railway, he had occasion to investigate the conditions of a bridge of five continuous spans over the river Palar. Having in India no books to refer to but those of Moseley and Edwin Clark, he found himself unable to extend the treatment of the cases there given to that of a beam with an increased number of openings and varying loads. After many attempts and failures, the same idea occurred to him which appears to have struck M. Clapeyron nine or ten years before—that if the bending-moments over the supports were known, the whole conditions would become known.

Following this clue, he was fortunate enough to succeed in at once eliminating the other unknown quantities, which M. Clapeyron had been obliged to retain in his equations for many years after his original discovery of the method, and thus to arrive at an equation precisely identical with that which had been first published in France by M. Bertot in 1856, and was known as the “Theorem of the three Moments.”

This was sufficient for the immediate purpose, as the beams in question were straight and of uniform section throughout, conditions to which this theorem is strictly applicable without any modification whatever.

As, however, the writer was at this time under the impression that he was using an entirely new mode of analysis, he was naturally anxious to check its results by comparison with those obtained in some well-known case by other means. Fortunately he had at hand that of the Britannia Bridge, perhaps the best that could have been selected; but for this purpose it became necessary to import into
the fundamental equation the conditions of varying sections in the
different spans and imperfect continuity. This, however, presented
no great difficulty; and by means of an equation thus modified, he
had the satisfaction of reproducing all Mr. Pole's results, and thus
convincing himself of the trustworthiness of the method in question.
The equation thus generalized is absolutely identical with that
arrived at by M. Bélanger in the work above referred to*.
It would appear, then, that the theory of this subject was indepen-
dently advanced to about the same state of perfection in France and
in England, though as regards the development of its application to
practice no doubt very much the more has been done in the former
country.

The writer will now advert to some inherent defects of this theory,
the cure of which is the principal object of the investigation which
follows.
The chief one, which is admitted by all writers on the subject,
is the necessity for supposing the moment of inertia of the section
constant throughout each span; any more general hypothesis, it is
said, would render the calculation inextricable. Still it is certain that
the conclusions arrived at on the hypothesis of a constant section
cease to be true if a variation of section is introduced; and the amount
of error thereby induced, though considered to be probably small, is
still a matter of uncertainty.
The next defect is the assumption of uniformity of load through-
out each span; for although as far as rolling load is concerned no
more correct hypothesis could be made, the weight of the bridge
itself, if a large one, usually varies considerably in the different parts
of the same span.
The equation given by M. Bresle, as has been stated, provides for
certain kinds of variable loads by the use of integrals; but the writer
is not aware that they have been applied, even by that author him-
self, to the purposes of calculation, and it seems to him that in most
cases the attempt to make such an application would be beset with
difficulties.
It will, however, it is hoped, be seen from what follows, that the
dealing with variations of the above elements does not in fact pre-
sent any very formidable difficulty, though no doubt the labour of
calculation is greater; but what the writer regards as most satisfac-
tory is the very small difference in the principal results in the case of
the Britannia (where these variations greatly exceed in amount those
usually occurring), whether obtained by the approximate method
hitherto followed, or by the more rigorous one to be explained, afford-
ing a strong presumption that in all ordinary cases the former method
may be confidently employed without risk of any important error.
Should the following treatment of the case be deemed successful,
the author would remark that its success is mainly due to the use
of an abbreviated functional notation, by which a great degree of
clearness and symmetry is preserved in expressions which would
otherwise have become inextricably complex.

* A paper on this subject by the writer was published in the Minutes of Pro-
ceedings of Inst. C. E. vol. xix. 1859-60.
Mr. J. M. Heppel on the Theory of Continuous Beams. 451

General Investigation of the Bending-Moments and Deflections of Continuous Beams.

Let 1 2 represent any span of a continuous beam, the length of the span being \( l \).

\( x, y \) the coordinates of the deflection-curve, the origin being at the point 1.

\( a \) and \( b \) particular values of \( x \).

\( \epsilon_1, \epsilon_2, \epsilon_3 \) reciprocals of the products of the moments of inertia of the sections in the spaces \( 1, a, a, b, b, 2 \), about their neutral axes, by the modulus of elasticity of the material \( \left( \frac{1}{E_I} \right) \).

\( \mu_1, \mu_2, \mu_3 \) loads per unit of length in the same spaces.

\( T \) tangent of inclination of deflection-curve at 1, to straight line joining 1 and 2, its positive value being taken upwards.

\( \phi_1, \phi_2 \) bending-moments at 1 and 2.

\( P \) shearing force at 1.

Now let the bending-moment at any point \((x, y)\)

between 1 and \( a \) be called \( F_1''(x) \),

between \( a \) and \( b \) be called \( F_2''(x) \),

between \( b \) and 2 be called \( F_3''(x) \);

and let the part of this bending-moment which results alone from the load on the beam between 1 and \( x \) be called,

between 1 and \( a \), \( f_1''(x) \),

between \( a \) and \( b \), \( f_2''(x) \),

between \( b \) and 2, \( f_3''(x) \);

and let the first and second integrals of these functions, as of \( F_1''(x) \), \( f_1''(x) \), be denoted by \( F_1(x) \), \( f_1'(x) \), and \( F_1(x), f_1(x) \), and the value of any one, as \( F_1(x) \), for a particular value of \( x \), as \( a \), by \( F_1(a) \); then

\[
\begin{align*}
  f_1''(x) & = \mu_1 \frac{a^2}{2}, \\
  f_2''(x) & = \mu_2 \left( x - \frac{a}{2} \right) + \mu_3 \frac{(x-a)^2}{2}, \\
  f_3''(x) & = \mu_4 \left( x - \frac{a}{2} \right) + \mu_5 \frac{(x-a)^2}{2} \quad \text{.. (3)}
\end{align*}
\]

Also, from equality of moments about the point \((x, y)\),

\[
\begin{align*}
  F_1''(x) & = \phi_1 - Px + f_1''(x), \\
  F_2''(x) & = \phi_2 - Px + f_2''(x), \\
  F_3''(x) & = \phi_3 - Px + f_3''(x) \quad \text{.. (4)}
\end{align*}
\]

and, from equality of moments about the point 2,

\[
\begin{align*}
  Pl & = \frac{1}{2} \left( \phi_1 - \phi_2 + f_3''(l) \right), \\
  P & = \frac{1}{2} \left( \phi_1 - \phi_2 + f_3''(l) \right) \quad \text{.. (7)}
\end{align*}
\]
Substituting for \( P \) in (4), (5) and (6),

\[
F_1''(x) = \left( 1 - \frac{\alpha}{l} \right) \phi_1 + \frac{\alpha}{l} \phi_2 - \frac{\alpha}{l} f_3''(l) + F_1''(x),
\]

\[
F_2''(x) = \left( 1 - \frac{\alpha}{l} \right) \phi_1 + \frac{\alpha}{l} \phi_2 - \frac{\alpha}{l} f_3''(l) + F_2''(x),
\]

\[
F_3''(x) = \left( 1 - \frac{\alpha}{l} \right) \phi_1 + \frac{\alpha}{l} \phi_2 - \frac{\alpha}{l} f_3''(l) + f_3''(x),
\]

equations from which for a given value of \( \alpha \), \( F_1''(x) \), \( F_2''(x) \), \( F_3''(x) \) may be determined if \( \phi_1 \) and \( \phi_2 \) are known.

From the nature of the deflection-curve,

from 1 to \( a \),

\[
\frac{d^2y}{dx^2} = \epsilon_1 F_1''(x);
\]

from \( a \) to \( b \),

\[
\frac{d^2y}{dx^2} = \epsilon_2 F_2''(x);
\]

from \( b \) to \( 2 \),

\[
\frac{d^2y}{dx^2} = \epsilon_3 F_3''(x);
\]

\[
\therefore \frac{dy}{dx} = \epsilon_1 F_1'(x) + C; \quad \text{when } x=0, F_1'(x)=0, \frac{dy}{dx} = -T;
\]

\[
\therefore \frac{dy}{dx} = \epsilon_1 F_1'(x) - T;
\]

from \( a \) to \( b \),

\[
\frac{dy}{dx} = \epsilon_2 F_2'(x) + C;
\]

making \( x=a \) in (14) and (15), and transposing,

\[
C = \epsilon_1 F_1'(a) - \epsilon_2 F_2'(b) - T;
\]

\[
\therefore \frac{dy}{dx} = \epsilon_1 F_1'(a) - \epsilon_2 \left( F_2'(x) - F_2'(a) \right) - T;
\]

from \( b \) to \( 2 \),

\[
\frac{dy}{dx} = \epsilon_3 F_3'(x) + C;
\]

making \( x=b \) in (16) and (17), and transposing,

\[
C = \epsilon_1 F_1'(a) + \epsilon_2 \left( F_2'(b) - F_2'(a) \right) - \epsilon_3 F_3'(b);
\]

\[
\therefore \frac{dy}{dx} = \epsilon_1 F_1'(a) + \epsilon_2 \left( F_2'(b) - F_2'(a) \right) + \epsilon_3 \left( F_3'(a) - F_3'b \right) - T; \quad (18)
\]

\[
\therefore \text{from } 1 \text{ to } a,
\]

\[
y = \epsilon_1 F_1'(x) - T a; \quad \text{no constant; for if } x=0, F_1' = 0, y=0; \quad (19)
\]

from \( a \) to \( b \),

\[
y = \epsilon_1 F_1'(a)x + \epsilon_2 \left( F_2'(x) - F_2'(a)x \right) - T a + C; \quad \ldots \ldots (20)\]
making \( x = a \) in (19) and (20), and transposing,
\[
C = \epsilon_1 (F_1(a) - F_1'(a)a) + \epsilon_2 (F_2(a) - F_2'(a)a);
\]
\[
\therefore y = \epsilon_1(F_1(a) + F_1'(a)(x-a)) + \epsilon_2(F_2(x) - (F_2(a) + F_2'(a)(x-a))) - Tx; \quad (21)
\]
from \( b \) to 2,
\[
y = \epsilon_1 F_1'(a)x + \epsilon_2 (F_2'(b)x - F_2'(a)x) + \epsilon_3 (F_3(x) - F_3'(b)x) - Tx + C; \quad \ldots \quad (22)
\]
making \( x = b \) in (21) and (22), and transposing,
\[
y = \epsilon_1(F_1(a) + F_1'(a)(x-a)) + \epsilon_2 \left[ (F_2(b) + F_2'(b)(x-b)) - (F_2(a) + F_2'(a)(x-a)) \right]
+ \epsilon_3 \left[ (F_3(x) - F_3'(b)x) - F_3'(b)(x-b) \right] - Tx. \quad \ldots \quad \ldots \quad \ldots \quad (23)
\]
From the way in which this last equation is formed, it is evident that if there were any number of particular values of \( x \) to be considered, as \( a, b, \&c., j, k, l \), the corresponding values of \( \frac{1}{EI} \) being \( \epsilon_1, \epsilon_2, \&c., \epsilon_{n-1}, \epsilon_n \), it might be written
\[
y = \left( F_1(a) + F_1'(a)(x-a) \right)
+ \left( F_2(b) + F_2'(b)(x-b) \right) - (F_2(a) + F_2'(a)(x-a)) - Tx; \quad (24)
\]
\[
+ \left( F_3(x) - (F_3(b) + F_3'(b)(x-b)) \right)
+ \&c.
\]
\[
+ \epsilon_{n-1} \left[ (F_{n-1}(k) + F_{n-1}'(k)(x-k)) - (F_{n-1}(j) + F_{n-1}'(j)(x-j)) \right]
+ \epsilon_n \left[ F_n(x) - (F_n(k) + F_n'(k)(x-k)) \right];
\]
if \( x = l \) in (24), \( y = 0 \);
\[
\therefore T = \frac{1}{l} \left[ \epsilon_1(F_1(a) + F_1'(a)(l-a))
+ \epsilon_2 \left[ (F_2(b) + F_2'(b)(l-b)) - (F_2(a) + F_2'(a)(l-a)) \right]
+ \epsilon_3 \left[ (F_3(c) + F_3'(c)(l-c)) - (F_3(b) + F_3'(b)(l-b)) \right]
+ \&c.
\]
\[
+ \epsilon_{n-1} \left[ (F_{n-1}(k) + F_{n-1}'(k)(l-k)) - (F_{n-1}(j) + F_{n-1}'(j)(l-j)) \right]
+ \epsilon_n \left[ F_n(l) - (F_n(k) + F_n'(k)(l-k)) \right].
\]
If, now, the formation of the functions \( F_1(a), F_1'(a) \&c. \) be examined, it is evident that this equation may be written
\[
T = A\phi_1 + B\phi_2 + C,
\]
where \( A \) and \( B \) are known functions of \( a, b, c, \&c. \) and \( \epsilon_1, \epsilon_2, \epsilon_3, \&c., \)
and \( C \) is a known function of the same and \( \mu_1, \mu_2, \mu_3, \&c. \).
If the adjacent span to the left be now considered, it is evident that a precisely similar equation may be obtained, which may be written
\[
T' = A'\phi_1 + B'\phi_2 + C';
\]
adding these, and writing \( t \) for \( T + T' \), which is known, as it is the tangent of the small angle which the neutral lines of the two spans would make at the point \( 1 \) if relieved from all load,

\[
t = (A + A')\phi_1 + B\phi_2 + B'\phi_0 + C + C',
\]

which may be written

\[
\Psi_1(\phi_0, \phi_1, \phi_2) = 0;
\]
similarly for the other bearing-points in succession,

\[
\Psi_2(\phi_1, \phi_2, \phi_3) = 0,
\]

\[
\Psi_3(\phi_2, \phi_3, \phi_4) = 0, \text{ &c.,}
\]

where the number of equations is two less than that of the quantities \( \phi_0, \phi_1, \phi_2, \text{ &c.,} \) so that if two of these are known the rest may be determined. But the first and last are always known, being usually each \( = 0 \). Therefore they may all be determined.

This being so, the bending-moment at any point \((x, y)\) may be found from equations (8), (9), (10) and others of the same form; and the deflection may be found from equations (19), (21), (23), and others of the same form, regard being had to the interval of the beam in which the point under examination lies.

If, now, we suppose that \( a = b = c = \&c. = l \), equation (25) reduces to

\[
T = \frac{1}{EI} (F_1(l));
\]
similarly,

\[
T' = \frac{1}{EI} (F_1'(l));
\]

\[
\therefore t = \frac{1}{EI} F_1(l) + \frac{1}{EI} F_1'(l),
\]

\[
\frac{EI}{F_1(l)} + i(F_1'(l)), \text{ writing } i \text{ for } \frac{1}{i}.
\]

Clearing of fractions and transposing,

\[
S(l + il')\phi_1 + 4l\phi_2 + 4il'\phi_0 = \ell'\mu + il'^3\mu' + 24EI\ell, \quad \ldots \quad (26)
\]
an equation which was given by the author in his paper before referred to, and which is nearly identical with the general equation of M. Bresse, and, allowing for difference of notation, precisely so with that of M. Bélanger.

If \( i = 1 \) and \( \ell = 0 \), which is the case of a straight beam of uniform section throughout,

\[
S(l + il')\phi_1 + 4l\phi_2 + 4il'\phi_0 = \ell'\mu + il'^3\mu' + 24EI\ell, \quad \ldots \quad (27)
\]

which is the equation generally known as the theorem of the three moments.

If in equation (25) we put \( l = a \), it becomes

\[
T = a\epsilon_1 \left( \frac{1}{3} \phi_1 + \frac{1}{6} \phi_2 - \frac{1}{24} a^2 \mu_1 \right); \quad \ldots \quad (28)
\]

and for the central deflection equation (19) becomes

\[
Y = a^2 \epsilon_1 \left( -\frac{1}{16} (\phi_1 + \phi_2) + \frac{5}{384} a^2 \mu_1 \right). \quad \ldots \quad (29)
\]
Mr. J. M. Heppel on the Theory of Continuous Beams. 455

If we put \( b = 2a, \ l = 3a, \)

\[
T = a \left[ \epsilon_1 \left( \frac{19}{27} \phi_1 + \frac{7}{54} \phi_2 - a^2 \left( \frac{43}{216} \mu_1 + \frac{7}{36} \mu_2 + \frac{7}{108} \mu_3 \right) \right) + \epsilon_2 \left( \frac{7}{27} \phi_1 + \frac{13}{54} \phi_2 - a^2 \left( \frac{7}{54} \mu_1 + \frac{7}{24} \mu_2 + \frac{13}{108} \mu_3 \right) \right) + \epsilon_3 \left( \frac{1}{27} \phi_1 + \frac{7}{54} \phi_2 - a^2 \left( \frac{1}{54} \mu_1 + \frac{1}{18} \mu_2 + \frac{11}{216} \mu_3 \right) \right) \right], \quad (30)
\]

and central deflection from equation (21),

\[
Y = a^2 \left[ \epsilon_1 \left( -\frac{7}{36} \phi_1 - \frac{1}{18} \phi_2 + a^2 \left( \frac{11}{144} \mu_1 + \frac{1}{12} \mu_2 + \frac{1}{36} \mu_3 \right) \right) + \epsilon_2 \left( -\frac{5}{16} \phi_1 - \frac{5}{16} \phi_2 + a^2 \left( \frac{5}{32} \mu_1 + \frac{1}{12} \mu_2 + \frac{5}{32} \mu_3 \right) \right) + \epsilon_3 \left( -\frac{1}{18} \phi_1 + \frac{7}{36} \phi_2 + a^2 \left( \frac{1}{36} \mu_1 + \frac{1}{12} \mu_2 + \frac{11}{144} \mu_3 \right) \right) \right], \quad (31)
\]

If we put \( b = 2a, \ c = 3a, \ d = 4a, \ l = 5a, \)

\[
Y = a \left[ \epsilon_1 \left( \frac{61}{75} \phi_1 + \frac{13}{150} \phi_2 - a^2 \left( \frac{149}{600} \mu_1 + \frac{91}{300} \mu_2 + \frac{13}{60} \mu_3 + \frac{13}{100} \mu_4 + \frac{13}{300} \mu_5 \right) \right) + \epsilon_2 \left( \frac{37}{75} \phi_1 + \frac{31}{150} \phi_2 - a^2 \left( \frac{37}{150} \mu_1 + \frac{123}{200} \mu_2 + \frac{31}{60} \mu_3 + \frac{31}{100} \mu_4 + \frac{31}{300} \mu_5 \right) \right) + \epsilon_3 \left( \frac{19}{75} \phi_1 + \frac{37}{150} \phi_2 - a^2 \left( \frac{19}{150} \mu_1 + \frac{19}{50} \mu_2 + \frac{13}{24} \mu_3 + \frac{37}{100} \mu_4 + \frac{37}{300} \mu_5 \right) \right) + \epsilon_4 \left( \frac{7}{75} \phi_1 + \frac{31}{150} \phi_2 - a^2 \left( \frac{7}{150} \mu_1 + \frac{7}{50} \mu_2 + \frac{7}{30} \mu_3 + \frac{161}{600} \mu_4 + \frac{31}{300} \mu_5 \right) \right) + \epsilon_5 \left( \frac{1}{75} \phi_1 + \frac{13}{150} \phi_2 - a^2 \left( \frac{1}{150} \mu_1 + \frac{1}{50} \mu_2 + \frac{1}{30} \mu_3 + \frac{7}{150} \mu_4 + \frac{7}{200} \mu_5 \right) \right) \right], \quad (32)
\]

and central deflection from equation (23),

\[
Y = a^2 \left[ \epsilon_1 \left( -\frac{13}{60} \phi_1 - \frac{1}{30} \phi_2 + a^2 \left( \frac{7}{80} \mu_1 + \frac{7}{60} \mu_2 + \frac{1}{12} \mu_3 + \frac{1}{20} \mu_4 + \frac{1}{60} \mu_5 \right) \right) + \epsilon_2 \left( -\frac{31}{60} \phi_1 - \frac{7}{30} \phi_2 + a^2 \left( \frac{31}{120} \mu_1 + \frac{161}{240} \mu_2 + \frac{7}{12} \mu_3 + \frac{1}{20} \mu_4 + \frac{7}{60} \mu_5 \right) \right) + \epsilon_3 \left( -\frac{9}{16} \phi_1 - \frac{9}{16} \phi_2 + a^2 \left( \frac{9}{32} \mu_1 + \frac{27}{32} \mu_2 + \frac{469}{384} \mu_3 + \frac{27}{32} \mu_4 + \frac{9}{32} \mu_5 \right) \right) + \epsilon_4 \left( -\frac{7}{30} \phi_1 - \frac{31}{60} \phi_2 + a^2 \left( \frac{7}{60} \mu_1 + \frac{7}{20} \mu_2 + \frac{7}{12} \mu_3 + \frac{161}{240} \mu_4 + \frac{31}{120} \mu_5 \right) \right) + \epsilon_5 \left( -\frac{1}{30} \phi_1 - \frac{13}{60} \phi_2 + a^2 \left( \frac{1}{60} \mu_1 + \frac{1}{20} \mu_2 + \frac{1}{12} \mu_3 + \frac{7}{60} \mu_4 + \frac{7}{80} \mu_5 \right) \right) \right], \quad (33)
\]
As an example of the application of the foregoing method to the purposes of calculation, let the case of the Britannia Bridge be taken, and let the large span be supposed to be divided into five, and the small span into three equal parts, and let the moments of inertia of the sections and loads per unit of length be supposed constant within each part and equal to their mean values.

We have then the following data:—

In spans (1 2) and (1 0),

\[
\begin{array}{cccccc}
\lambda & b' & a' & \Lambda & a & b & c & d & a \\
0 & 1 & 1 & a &=& b &=& c &=& d \\
1 & 1 & 1 & \lambda &=& e &=& f &=& g &
\end{array}
\]

\[
\begin{align*}
\epsilon &= \frac{1}{1132\text{E}} & \epsilon' &= \frac{1}{1520\text{E}} & \epsilon_3 &= \frac{1}{1746\text{E}} & \epsilon_4 &= \frac{1}{1664\text{E}} & \epsilon_5 &= \frac{1}{1857\text{E}} \\
\mu_1 &= 2.89 & \mu_2 &= 3.31 & \mu_3 &= 3.57 & \mu_4 &= 3.49 & \mu_5 &= 3.65
\end{align*}
\]

and, from symmetry of loading, \( T + T' = 0 \).

Applying equations (30) and (32) to spans (1.0) and (1.2) respectively and eliminating \( T \) and \( T' \) by adding them, we obtain

\[
0.1888\phi_1 + 0.04827\phi_2 - 10481 = 0
\]

and applying equation 32 to span (2 1),

\[
0.04827\phi_1 + 0.08765\phi_2 = 5420 = 0
\]

whence

\[
\phi_1 = 46206, \quad \phi_2 = 36387.
\]

Taking these values of \( \phi_1 \) and \( \phi_2 \), and \( Y \) be calculated from equations (26) and (19), on the supposition that the moments of inertia of the section and the loads are constant throughout each span and equal to their mean values, they are

\[
\phi_1 = 47030, \quad \phi_2 = 35610, \quad Y = 4.62,
\]

which are almost identical with the values ascertained by Mr. Pole.
If the variation of section alone be considered, the load being taken at its mean value,

\[ \phi_1 = 46382, \quad \phi_2 = 34465, \quad Y = 4.52. \]

It therefore appears that the amount of variation in the section and load which occurs in each span of the Britannia Bridge, when taken strictly into account, produces scarcely any effect on the values of the bending-moments and deflections, which are practically the same as those resulting from their mean values considered as constant; and it may be considered demonstrated that, for most ordinary cases of large bridges, calculations founded on equation (26) may be confidently relied on. It need scarcely be remarked that these are much more simple and easy than those founded on the more exact but complex equations above given.

In smaller bridges, however, the error of the approximate process will be more considerable, and the process above given may be applied with advantage to its correction.

In concluding this paper, the author desires to record his thanks to his young friend, Mr. Henry Reilly, for the patience and skill with which he made, in detail, all the intricate calculations of the numerical values of the various functions involved in the above demonstration.

"Remarks on Mr. Heppel's Theory of Continuous Beams." By W. J. Macquorn Rankine, C.E., LL.D., F.R.S.

1. Condensed form of stating the Theory.—The advantages possessed by Mr. Heppel's method of treating the mathematical problem of the state of stress in a continuous beam will probably cause it to be used both in practice and in scientific study.

The manner in which the theory is set forth in Mr. Heppel's paper is remarkably clear and satisfactory, especially as the several steps of the algebraical investigation correspond closely with the steps of the arithmetical calculations which will have to be performed in applying the method to practice.

Still it appears to me that, for the scientific study of the principles of the method, and for the instruction of students in engineering science, it may be desirable to have those principles expressed in a condensed form; and with that view I have drawn up the following statement of them, which is virtually not a new investigation, but Mr. Heppel's investigation abridged.

Let \((x = 0, y = 0)\) and \((x = l, y = 0)\) be the coordinates of two adjacent points of support of a continuous beam, \(x\) being horizontal. Let \(y\) and the vertical forces be positive downwards.

At a given point \(x\) in the span between those points let \(\mu\) be the load per unit of span, and EI the stiffness of the cross section, each of which functions may be uniform or variable, continuous or discontinuous.

let the lower limits be $x=0$.

$$
\begin{align*}
\int \mu dx^2 &= m; & \int \frac{dx^2}{EI} &= n; \\
\int \frac{x dx^2}{EI} &= q; & \int \frac{dx^2}{EI} \int \mu dx^2 &= F.
\end{align*}
$$

When the integrations extend over the whole span $l$, that will be denoted by suffixing 1; for example, $m_1$, $n_1$, &c.

Let $-P$ be the upward shearing-force exerted close to the point of support ($x=0$), $\Phi_0$ the bending-moment, and $T$ the tangent of the inclination, positive downwards, at the same point. Then, by the general theory of deflection, we have, at any point $x$ of the span $l$, the following equations:

- moment, $\Phi = \Phi_0 - Px + m$; \hspace{1cm} (2)
- deflection, $y = Tx - Pq + \Phi_0 + F$. \hspace{1cm} (3)

Let $\Phi_1$ be the moment at the further end of the span $l$, and suppose it given. This gives the following values for the shearing-force $P$ and slope $T$ at the point ($x=0$):

$$
P = \frac{\Phi_0 - \Phi_1 + m}{l}; \hspace{1cm} \text{(4)}$$

and because $y_1 = 0$,

$$
T = \frac{Pq_1 - \Phi_0 n_1 - F_1}{l} = \Phi_0 \left( \frac{q_1 - n_1}{l^2} \right) - \frac{\Phi_0 q_1}{l^2} + \frac{m_1 q_1}{l} - \frac{F_1}{l}. \hspace{1cm} \text{(5)}
$$

Consider, now, an adjacent span extending from the point of support ($x=0$) to a distance ($x=l'$) in the opposite direction, and let the definite integrals expressed by the formulæ (1), with their lower limits still at the same point ($x=0$), be taken for this new span, being distinguished by the suffix $-1$ instead of $1$. Let $-T'$ be the slope at the point of support ($x=0$). Then we have for the value of that slope,

$$-T' = \Phi_0 \left( \frac{q_1 - n_1}{l^2} \right) - \frac{\Phi_1 q_1}{l'^2} + \frac{m_1 q_1}{l'} - \frac{F_1}{l'} \hspace{1cm} \text{(5A)}$$

Add together the equations (5) and (5A), and let $t = T - T'$ denote the tangent of the small angle made by the neutral layers of the two spans with each other in order to give imperfect continuity. Then, after clearing fractions, we have the following equation, which expresses the theorem of the three moments in Mr. Heppel's theory:

$$0 = \Phi_0 (q_1 l'^2 + q_{-1} l'^2 - n_1 l'^2 - n_{-1} l'^2) - \Phi_1 q_1 l'^2 - \frac{\Phi_1 q_1}{l'^2} - \Phi_{-1} q_{-1} l'^2 \hspace{1cm} \text{(6)}$$

That equation gives a linear relation between the bending moments $\Phi_{-1}, \Phi_0, \Phi_1$ at any three consecutive points of support, and certain known functions of known quantities. In a continuous girder of $N$ spans there are $N-1$ such equations and $N-1$ unknown moments; for the moments at the end of most supports are each $=0$. The moments at the intermediate points of support are to be found by elimination; which having been done, the remaining quantities
required may be computed for any particular span as follows:—
The inclination T at a point of support by equation (5); the shearing-force P at the same point by equation (4); the deflection y and moment φ at any point in that span by equations (3) and (2). Points of maximum and minimum bending-moment are of course found by making \( \frac{d\phi}{dx} = 0 \); and points of inflection by making \( \phi = 0 \).

2. Case of a uniform girder with an indefinite number of equal spans, uniformly loaded; loads alternately light and heavy.—The supposition just described forms the basis of the formulæ given in a treatise called ‘A Manual of Civil Engineering,’ page 288; and it therefore seems to me desirable to test those formulæ by means of Mr. Heppel’s method.

The cross section of the whole girder and the load on a given span being uniform, the definite integrals of the formulæ (1) take the following values:—

\[
m = \frac{\mu x^2}{2}; \quad n = \frac{x^3}{2EI}; \quad q = \frac{x^3}{6EI}; \quad F = \frac{\mu x^4}{24EI} = \frac{mq}{2w} \quad \ldots \quad (7)
\]

The values of those integrals for the complete span are expressed by making \( x = l \).

The values of \( n \) and \( q \) are the same for every span. In the values of \( m \) and \( F \), the load \( \mu \) per unit of span has a greater and a less value alternately. Let \( w_0 \) be the weight per unit of span of the girder with its fixed load, \( w_1 \) that of the travelling load (increased, if necessary, to allow for the additional straining effect of motion); then the alternate values of \( \mu \) are

\[
\mu = w_0; \quad \mu' = w_0 + w_1. \quad \ldots \quad \ldots \quad (8)
\]

The moments at the points of support are all equal; that is, \( \Phi_0 = \Phi_1 = \Phi_{-1} \).

Equation (6) now becomes the following (the common factor \( l^2 \) having been cancelled):—

\[
0 = -2\Phi_0 n_1 + F_1 + F_{-1} - tl;
\]
giving for the bending-moment at each point of support

\[
\phi_0 = \frac{F_1 + F_{-1} - tl}{2n_1} = \frac{2w_0 + w_1}{24} \cdot l - \frac{t}{l} EI. \quad \ldots \quad (9)
\]

If \( t \) be made = 0, so that the continuity is perfect, this equation exactly agrees with the formula at page 289 of the treatise just referred to; and the same is the case with the following formulæ for the shearing-forces and slopes close to a point of support, and for the moments and deflections at other points:—

Shearing-force, light load, \( P = \frac{w_0 l}{2} \); \]

Shearing-force, heavy load, \( P_1 = \frac{w_0 + w_1}{2} \cdot l \).

\[
\begin{align*}
2 & H 2
\end{align*}
\]
Intelligence and Miscellaneous Articles.

Slope, light load, \( T = \frac{Pq - \Phi_s n - F}{l} = \frac{t}{2} \frac{w_0 l^3}{48EI} \); \( \text{(11)} \)

Slope, heavy load, \( -T' = \frac{t}{2} + \frac{w_0 l^3}{48EI} \).

Moment, light load, \( \Phi = \Phi_0 - Px + m = -\frac{t}{l} EI + \frac{2w_0 + w_1 p}{2} - \frac{w_2}{2} + \frac{w_0 w_0^2}{2} \); \( \text{(12)} \)

Moment, heavy load, \( \Phi' = -\frac{t}{l} EI + \frac{2w_0 + w_1 p}{2} - \frac{w_2}{2} + \frac{w_0 + w_1}{2}. \)

Central moment, light load, \( \Phi\left( x = \frac{l}{2} \right) = -\frac{t}{l} EI + \frac{w_1 - w_0 l^2}{24} \); \( \text{(13)} \)

Central moment, heavy load, \( \Phi'\left( x = \frac{l}{2} \right) = -\frac{t}{l} EI + \frac{w_1 + w_0 l^2}{24} \).

Central deflection, light load, \( y = T x - P q + \Phi_s n + F \) (with \( x = \frac{l}{2} \)) = \( \frac{tl}{8} + \frac{w_0 - 2w_1}{384EI}. \)

Central deflection, heavy load, \( y = -T' x - P' q + \Phi_s n + F \) (with \( x = \frac{l}{2} \)) = \( \frac{tl}{8} + \frac{w_0 + 3w_1}{384EI}. \)

---

LVIII. Intelligence and Miscellaneous Articles.

LECLANCHE'S MANGANESE ELEMENTS. BY J. MÜLLER.

Leclanché's voltaic elements have recently been extensively recommended, although no statements as to their constants have been published. I have thus been led to make a few experiments with them.

The arrangement of the elements is as follows:—A plate, or rather a rod of gas-coke is placed in a porous clay cylinder, and the rest of the space filled with a mixture of equal volumes of manganese (pyrolusite, Braunite) and of gas-coke in pieces the size of a pea. The clay cylinder thus prepared is placed in a wide glass vessel filled with solution of sal-ammoniac, in which is placed an amalgamated zinc rod.

To determine the constants of this combination I used Ohm's method. Three of Leclanché's cells, connected so as to form one pair of plates, produced a deflection of 13° on a tangent-compass the reduction-factor of which was 74 when this was connected with the rheometer by only short thick copper wires. This deflection was diminished to 5°.1 by the insertion of a Siemens's unit. Hence for the electromotive force of a Leclanché's element we get the value \( e = 10.76 \),

and for the essential resistance of one cell \( r = 1.89 \),

if, according to Waltenhofen's proposal, we assume as unit that current which furnishes one cubic centimetre of explosive gas in a minute, and as unit of resistance a Siemens's unit (the resistance of
Intelligence and Miscellaneous Articles. 461
da column of mercury a metre in length and a millimetre square in section). Taking the same unit as a basis, the electromotive force of a Bunsen's element is equal to 21, and of a Daniell's equal to 12. Hence the electromotive force of a Leclanché's element is only 0.896 that of a Daniell's, while from Leclanché's determinations* it is said to be 1.38 as great as that of a Daniell's.

This difference (1.38 against 0.896) may be easily explained. Without galvanic polarization the electromotive force of such an element should be equal to that of a Bunsen's cell (compare my Lehrbuch der Physik, 7th edit. vol. ii. p. 263). But the degree of polarization depends on the strength of the current which the cell furnishes, and therefore on the magnitude of the resistance which is interposed in the circuit. In my experiments the resistance was very small, and hence there was a powerful polarization; while in Leclanché's experiments the current was not so powerful, and the electromotive force was therefore not so greatly weakened as in my experiments.

Leclanché found the mean resistance of a manganese-cell of mean size (porous cell 15 centims. in height and 6 centims. in diameter) to be equal to 550, taking as unit of resistance an iron wire 4 millims. in diameter and a metre in length. Referred to Siemens's unit, this resistance is

\[ r = n \frac{l}{\pi r^2} = 1.4, \]

where \( n \) is the resistance of iron as compared with mercury (that is, 0.12), and \( l = 550, r = 4 \); while I found \( r = 1.89 \). The cells with which Leclanché experimented were doubtless somewhat larger than mine.

I was concerned to ascertain, if possible, the part which the manganese plays in this. Leclanché's statements on this point are inadequate; for he says (Dingler's Journal) that the manganese rapidly and uniformly absorbs the hydrogen. If by this it is meant that the hydrogen liberated at the negative pole is immediately oxidized, the statement is manifestly incorrect; for then the galvanic polarization would not exist, and the electromotive force would be 21 (that is, equal to that of a Bunsen's cell). But whether the manganese does generally exert an influence on the electromotive force can only be decided by investigating a cell which has just the same structure as a Leclanché's, but with the difference that the mixture of manganese and carbon is replaced by pieces of carbon (without manganese). For such a cell I found the electromotive force

\[ e' = 6.16, \]

considerably less, therefore, than the electromotive force of a manganese-cell. Hence the voltaic polarization is not entirely removed by the carbon being partly surrounded by manganese, although it is materially lessened. The manganese manifestly gives up some oxygen, although it is not sufficient to oxidize all the liberated hydrogen. With this agrees the experience, that in Leclanché's cells which had been for some time in use the manganese had lost its activity.—Poggendorff's Annalen, June 1870.

ON THE MELTING OF LEADEN PROJECTILES BY THEIR IMPACT UPON AN IRON PLATE. BY EDUARD HAGENBACH.

At the commencement of the present year experiments were made at Basle with the view of using targets of iron instead of wood in practice with firearms. Strong plates of iron were, on this occasion, fired at from the short distance of 100 paces. Conical bullets by their impact against the iron plate produced a scarcely perceptible indentation, and fell down near the target; at the same time the lead projectile was melted to a very considerable extent. This could be recognized by the fact that, around the point where the ball had struck, the plate was spattered with lead in the form of a white star, that, moreover, the melted lead was found in the vicinity, and that, of the original bullet, which weighed 40 grammes, only the comparatively small portion of 13 grammes remained. This residual part exhibited a very peculiar kind of deformation and inversion, as may be seen in figs. 1 and 2. Fig. 1 gives the section of

![Fig. 1](image)

the original projectile, and fig. 2 the section of the residue; the concave surface $a b c d$, in consequence of the pressure resulting from the impact, was transformed into the convex surface $a_i b_i c_i d_i$. The phenomenon in question is obviously interesting with regard to the mechanical theory of heat, inasmuch as we have here a very distinct example of the transformation of the impetus of the motion of a body into the impetus of molecular motion. We will inquire how far, with the help of this theory, we are in a position to account for the matter in question.

According to the statement of a competent military man, the velocity of the projectile, under the circumstances in question, may be assumed to be equal to 320 metres; hence the impetus, $\frac{mv^2}{2}$, of the movement of the body is equal to 209 kilogrammetres*. Assuming 424 kilogrammetres as the mechanical equivalent of the heat, this gives us 0.49 thermal unit. Let us now inquire how much heat is necessary to produce the melting described. The entire projectile (40 grammes) had to be raised to the temperature of the melting-point of lead, or near it; and then 27 grammes had to be melted. Assuming 100° for the initial temperature of the projectile, which must have been somewhat warmed by the heat of combustion of the powder and by

* In this we neglect (what must in any case be very small) the impetus due to the velocity of rotation of the projectile.
the friction against the barrel, taking the melting-point of lead as 335°, its specific heat as 0·031, and its latent heat of fusion as 5·37, we find necessary

For the heating....... 0·29 thermal unit.
,, fusion ........ 0·15 ,, 
For both together....... 0·44 ,, 

From this calculation we see:—
(1) That the mechanical theory of heat sufficiently accounts for the operation.
(2) Almost all the impetus of the motion of the body is transformed into heat,—a result which was indeed to be expected, seeing that the iron plate was very slightly deformed, and the projectile rebounded but little.
(3) By far the greater part of the heat was used in heating and in melting the lead. This also is readily intelligible; for the short time within which the entire process was effected could give rise to but little loss by conduction and radiation.—Poggendorff's Annalen, No. 7, 1870.

AN EXPERIMENT ON THE BOILING IN CONJUNCTION OF TWO LIQUIDS WHICH DO NOT MIX. BY AUGUST KUNDT.

Magnus *, and after him Regnault †, have shown that the vapours of liquids which do not mix obey Dalton's law of diffusion. The common tension of the vapours of two non-miscible liquids (e. g. bisulphide of carbon and water) in a state of saturation is equal to the sum of the tensions which would correspond to the state of saturation of the individual vapours for the temperature in question. Two such liquids boil, therefore, when together, at a temperature which is lower than the boiling-point of the most volatile. Magnus, however, in describing his experiments, remarks that the temperature of the boiling liquid is somewhat higher than that of the most volatile when the latter is underneath the less volatile one.

Regnault remarks that in the boiling of two liquids which do not mix it is very difficult to preserve constant temperatures in the vapour and in the liquid; the temperature varied materially with the heating and with the formation of bubbles.

I have found that the anomaly observed by Magnus (that is, the difference in temperature of the liquid and of the vapour) may be completely avoided, and the experiment be so arranged that the liquid during boiling retains exactly, and without variations, the temperature which corresponds to Dalton's law. For this purpose I do not heat the liquids, such as bisulphide of carbon and water, together in a vessel by direct heat, but heat one by the vapour of the other. Magnus once used this method to show that a concentrated saline solution can be heated by vapour from pure water to the boiling-point of the solution in question ‡.

The method is applicable both to miscible and to non-miscible liquids. If into a vessel (and best of all a glass cylinder) which is

* Poggendorff's Annalen, vols. xxxviii. and xciii.
about one-third filled with CS₂, steam from a flask with distilled water be passed continuously by means of a tube which goes to the bottom, the liquid (that is, the mixture of bisulphide and water) which is traversed by aqueous vapour has the same temperature as the mixed vapours. Both the liquid and the vapour indicated a temperature of 42°6, a temperature which of course varies with the purity of the CS₂ used (boiling-temperature 46°6) and the barometric height. The temperature once obtained is kept perfectly constant as long as there is a small quantity of CS₂ in the cylinder. The same temperature of 42°6 is maintained constantly in the liquid and the vapour when the experiment is inverted, and water is poured into the cylinder, and the latter heated by having bisulphide vapour passed in.

I made the same experiments with water and benzole, with water and oil of cloves, and several other liquids, and in all cases with the same result. When, for instance, aqueous vapour was passed into oil of cloves, the mixed liquids and also the vapour showed very nearly 99°. More accurate numbers and a few remarks which naturally arise out of them will be published subsequently.

For the present it is my purpose to describe an experiment to which I have been led by that above related; for it elucidates in a very clear manner, and one especially suited for lectures, that two liquids which do not mix boil when together at a lower temperature than the most volatile. As far as I am aware, the experiment has not hitherto been described.

If CS₂ boils when alone at 46°6, and CS₂ and water when together at nearly 43°, it is clear that boiling must occur when both liquids are heated separately to a temperature between 43° and 46°6, about 45°, and are then brought together. Experiment confirms this completely.

Into a glass vessel about a foot in height and \( \frac{3}{4} \) foot in diameter let water be brought whose temperature is not quite 46°6, let a test-tube about \( \frac{3}{4} \) inch in diameter be half filled with CS₂ and immersed in the water until the temperature of the bisulphide has risen to about 45°.

If then the bisulphide be poured into the water, a brisk ebullition is set up, which, with an adequate quantity of water, is maintained for some time. If after a while the ebullition becomes weak or even entirely ceases, stirring with a glass rod starts it again and keeps it in fresh ebullition. By stirring, other particles of water are brought into contact with the CS₂, which have not yet been cooled down by parting with the heat necessary for evaporation.

Even when the entire mass has already been cooled below 42°, solitary bubbles rise, though there is no longer a continuous ebullition.

The tension of the bisulphide is then only sufficient between the bisulphide and the water, especially if the former does not cover the entire base, but forms detached drops, to form a bubble (as Quincke has also observed*), which, when it attains sufficient magnitude, can detach itself on shaking or stirring and ascends to the surface.

Proper continuous boiling only sets in at a temperature of about 43°.—Poggendorff’s Annalen, No. 7, 1870.

INDEX to VOL. XL.

AÉRIAL vibrations in pipes of various forms, on the, 211.
Aldis (T. S.) on spiral nebulae, 389.
Ammonia, on the estimation of, in atmospheric air, 54.
André (F.) on the velocity of the propagation of sound in water, 76.
Atmosphere, on the estimation of ammonia in the, 54.
Aurora borealis, on a possible cause of the bright line observed by M. Angström in the spectrum of the, 33.
Ball (J.) on the cause of the descent of glaciers, 1.
Barlow (W. H.) on the cause and theoretic value of the resistance of flexure in beams, 130.
Beams, on the theory of continuous, 446, 457.
Bezold (Prof. Von) on the electrical discharge, 42.
Bleekrode (Dr. L.) on the influence of heat on electromotive force, 310.
Böttger (R.) on the preparation of a liquid for producing Plateau's figures, 392.
Books, new:—Tyndall's Researches on Diamagnetism, 301; Goodev's Elements of Mechanism, 445.
Broughton (J.) on the chemical characteristics of the various parts of the Cinchona plant, 379.
Brown (H. T.) on the estimation of ammonia in atmospheric air, 54.
Caillélet (L.) on the compressibility of gases under high pressures, 146.
Calorimetry, on the use of the electric current in, 142.
Carbon, on the spectra of, 100.
Carbonic oxide, on the rapidity of the absorption of, by the lungs, 150.
Cayley (Prof.) on the geodesic lines on an oblate spheroid, 329.
Cazin (A.) on the duration of the electric spark, 78; on internal work in gases, 81, 197, 268.

Chemistry, on statical and dynamical ideas in, 259.
Cinchona, experiments on living, 379.
Clausius (Prof. R.) on a mechanical theorem applicable to heat, 122.
Cometary orbits, on the probable character of, 183.
Comets, on a theory of, 300.
Contour-lines, on the forms of, 421.
Corona, is the, a solar or terrestrial phenomenon? 117.
Croll (J.) on the cause of the motion of glaciers, 153; on the physical cause of ocean-currents, 233.
Davis (A. S.) on the bright line observed by M. Angström in the spectrum of the aurora borealis, 33; on the probable character of cometary orbits, 183; on a theory of nebulae and comets, 300.
Davis (J. E.) on deep-sea thermometers, 132.
Dawson (Dr. J. W.) on the structure and affinities of Sigillaria, Calamites, and Calamodendron, 384.
De la Rive (Prof. A.) on the magnetic rotatory polarization of liquids, 393.
De La Rue (Dr. W.) on solar physics, 53.
Delanay (M.) on the late Mr. Hopkins's method of determining the thickness of the earth's crust, 10.
Dinosauria, on the classification of the, 70.
Douglas (J. C.) on a new optometer, 340.
Duncan (Dr. P. M.) on the physical geography of Western Europe during the mesozoic and Cainozoic periods, 71.
Earth, on the method of determining the thickness of the crust of the, by the precession and nutation of the axis of the, 10; on supra-annual cycles of temperature in the surface-crust of the, 58.

INDEX.

Earths, on the spectra of some, 302.
Edlund (Prof. E.) on the path of electrical induction- and disjunction-currents through gases of various densities and between poles of different shapes, 14.
Electric current, on the use of the, in calorimetry, 142.
— spark, on the duration of the, 78.
Electrical discharge, researches on the, 42.
— induction- and disjunction-currents, on the path of, through gases, 14.
— resistance, on a simple method of constructing high, 41.
Electricity, on the molecular theory and laws of, 390.
Electrodynamic spirals, on the magnetism of, 264.
Electrolytes, on the extension of Ohm’s law to, 227.
Electromotive force, on the influence of heat on, 310.
Electroscopic experiments, on a cause of error in, 128.
Equations, on the solution of linear partial differential, 35, 149.
Equilibrium, experimental and theoretical researches into the figures of, of a liquid mass without weight, 355.
Erbia, on the spectrum of, 302.
Flexure, on the cause and theoretic value of the resistance of, in beams, 130.
Gases, on internal work in, 81, 197, 268; on the compressibility of, under high pressures, 146.
Geodesic lines on an oblate spheroid, on the, 329.
Gibbs (Dr. W.) on the measurement of wave-lengths by means of indices of refraction, 177; on liquids of high dispersive power, 229; on tests for the perfection and parallelism of plane surfaces of glass, 311.
Glaciers, on the cause of the descent of, 1, 153.
Glass, on tests for the perfection and parallelism of plane surfaces of, 311.
Gore (G.) on the molecular movements and magnetic changes in iron, 170; on the magnetism of electrodynamic spirals, 264.
Granites of Scotland, on the constituent minerals of the, 59.
Gréhant (N.) on the rapidity of the absorption of carbonic oxide by the lungs, 150.
Guthrie (Prof. F.) on approach caused by vibration, 345.
Hagenbach (E.) on the melting of leaden projectiles by their impact upon an iron plate, 462.
Haughton (Rev. S.) on the constituent minerals of the granites of Scotland, 59.
Heat, on the interchangeability of, and mechanical action, 51, 103, 218, 429; on a mechanical theorem applicable to, 122; on the influence of, on electromotive force, 310; on the radiation of, from the moon, 372.
Heights, on contour-lines and measurement of, 421.
Heppel (J. M.) on the theory of continuous beams, 446.
Hills and dales, on, 421.
Huggins (Dr. W.) on the spectra of erbia and some other earths, 302.
Huxley (Prof.) on a new genus of Dinosauria, 68; on the affinity between the Dinosaurian reptiles and birds, 69; on the Dinosauria of the trias, 70.
Iron, on the molecular movements and magnetic changes in, 170.
Jamin (J.) on the use of the electric current in calorimetry, 142.
Koenig (R.) on the fixed notes characteristic of the various vowels, 145.
Kohlrausch (F.) on the extension of Ohm’s law to electrolytes, and on the numerical determination of the resistance of dilute sulphuric acid, 227.
Kundt (A.) on the boiling in conjunction of two liquids which do not mix, 463.
Laplace’s coefficients, on the equation of, 434.
Leclanché’s manganese elements, observations on, 460.
INDEX.

Le Sueur (A.) on the great Melbourne telescope, 377.

Light, on the dispersion of, 105.

Lime, on the spectrum of, 303.

Liquids, on the adhesion between, and solids, 190; of high dispersive power, on, 229; on the magnetic rotatory polarization of, 393; on the boiling in conjunction of two, which do not mix, 463.

Lowey (B.) on solar physics, 53.

Lorenz (L.) on the molecular theory and laws of electricity, 390.

Lucas (M.) on the duration of the electric spark, 78.

Lungs, on the rapidity of the absorption of carbonic oxide by the, 150.

Luvini (Prof. G.) on the adhesion between solids and liquids, 190.

Magnesia, on the spectrum of, 303.

Magnetism of electrodynamic spirals, on the, 264.

Manganese elements, on Leclanché's, 460.

Maxwell (Dr. J. C.) on hills and dales, 421.

Merz (S.) on an object-glass spectral apparatus, 294.

Meteor of Nov. 19, 1870, observations on the, 440.

Mills (Dr. E. J.) on the chemical activity of nitrates, 134; on chemical substance and chemical functions, 259.

Moon, on the radiation of heat from the, 372.

Moon (R.) on the solution of linear partial differential equations, 35, 149; on the equation of Laplace's coefficients, 434.

Moseley (Canon) on the cause of the descent of glaciers, 1.

Miiller (J.) on Leclanché's manganese elements, 460.

Nebulæ, on a theory of, 300; note on spiral, 389.

Nippoldt (A.) on the extension of Ohm's law to electrolytes, and on the numerical determination of the resistance of dilute sulphuric acid, 227.

Nitrates, on the chemical activity of, 134.

Ocean-currents, on the physical cause of, 233.

Optometer, remarks on a new, 340.

Phillips (S. E.) on a simple method of constructing high electrical resistance, 41.

Plateau (Prof.) on the figures of equilibrium of a liquid mass without weight, 355.

Polarization, on the magnetic rotatory, of liquids, 393.

Pratt (Archdeacon) on the method of determining the thickness of the earth's crust by the precession and nutation of the earth's axis, 10.

Projectiles, on the melting of leaden, by their impact upon an iron plate, 462.

Rankine (Dr. W. J. M.) on thermodynamics, 103, 291; on the thermodynamic acceleration and retardation of streams, 288; on the meteor of November 19, 1870, 440; on Mr. Heppel's theory of continuous beams, 457.

Refractive indices and dispersion of opaque bodies, on the, 105.

Resistances, on a new method of determining, 441.

Roscoe (Prof. H. E.) on the relation between the sun's altitude and chemical intensity, 56; on vanadium, 62.

Rosse (Earl of) on the construction of thermopiles, 569; on the radiation of heat from the moon, 372.

Royal Society, proceedings of the, 53, 128, 221, 302, 369, 446.

Saline solutions, on supersaturated, 221; on the action of low temperatures on supersaturated, 295.

Seabroke (G. M.) on the nature of the corona, 117.

Seebeck (A.) on the propagation of sound in tubes, 231.

Smyth (Prof. C. P.) on supra-annual cycles of temperature in the earth's surface-crust, 58.

Solar physics, researches on, 53.

— protuberances, on, 427.

Solids and liquids, on the adhesion between, 190.

Sondhauss (Dr.) on the tones of heated tubes and aerial vibrations in pipes of various forms, 211.

Sound, on the velocity of the propagation of, in water, 76; on the propagation of, in tubes, 231.

Spectral apparatus, on an object-glass, 294.

Stewart (Dr. B.) on solar physics, 53.
INDEX.

Streams, on the thermodynamic acceleration and retardation of, 288.
Strutt (the Hon. J. W.) on the tones of heated tubes and aerial vibrations in pipes of various forms, 211.
Sulphuric acid, on the determination of the resistance of dilute, by means of alternate currents, 227.
Sun, on the relation between the altitude and chemical intensity of the, 56; on the temperature and physical constitution of the, 313.
Temperature, on supra-annual cycles of, in the earth’s surface-crust, 58.
Thermodynamics, on, 51, 103, 218, 429.
Thermometers, on deep-sea, 132.
Thermopiles, on the construction of, 369.
Thorpe (Prof. T. E.) on the relation between the sun’s altitude and chemical intensity, 56.
Tomlinson (C.) on supersaturated saline solutions, 221; on the action of low temperatures on supersaturated saline solutions, 295; on a salt that is invisible in its mother-liquor, 328.
Tones of heated tubes, on the, 211.
Vanadium, researches on, 62.
Vibration, on approach caused by, 345.
Vowels, on the fixed notes characteristic of the various, 145.
Watersheds and watercourses, on, 425.
Watts (Dr. W. M.) on the spectra of carbon, 100.
Wave-lengths, on the measurement of, by means of indices of refraction, 177.
Wernicke (W.) on the refractive indices and the dispersion of opaque bodies, 105.
Wheatstone (Sir C.) on a cause of error in electroscopic experiments, 128.
Zöllner (Prof. F.) on the temperature and physical constitution of the sun, 313; on solar protuberances, 427.

END OF THE FORTIETH VOLUME.
Forms of Eruptions & Cloud-like prominences as observed and drawn by Prof. Zollner.

Fig 1
Position. 160°
Time 10h 22m

1869 August 29

Fig 2
The same Protuberance
Time 11h 20m