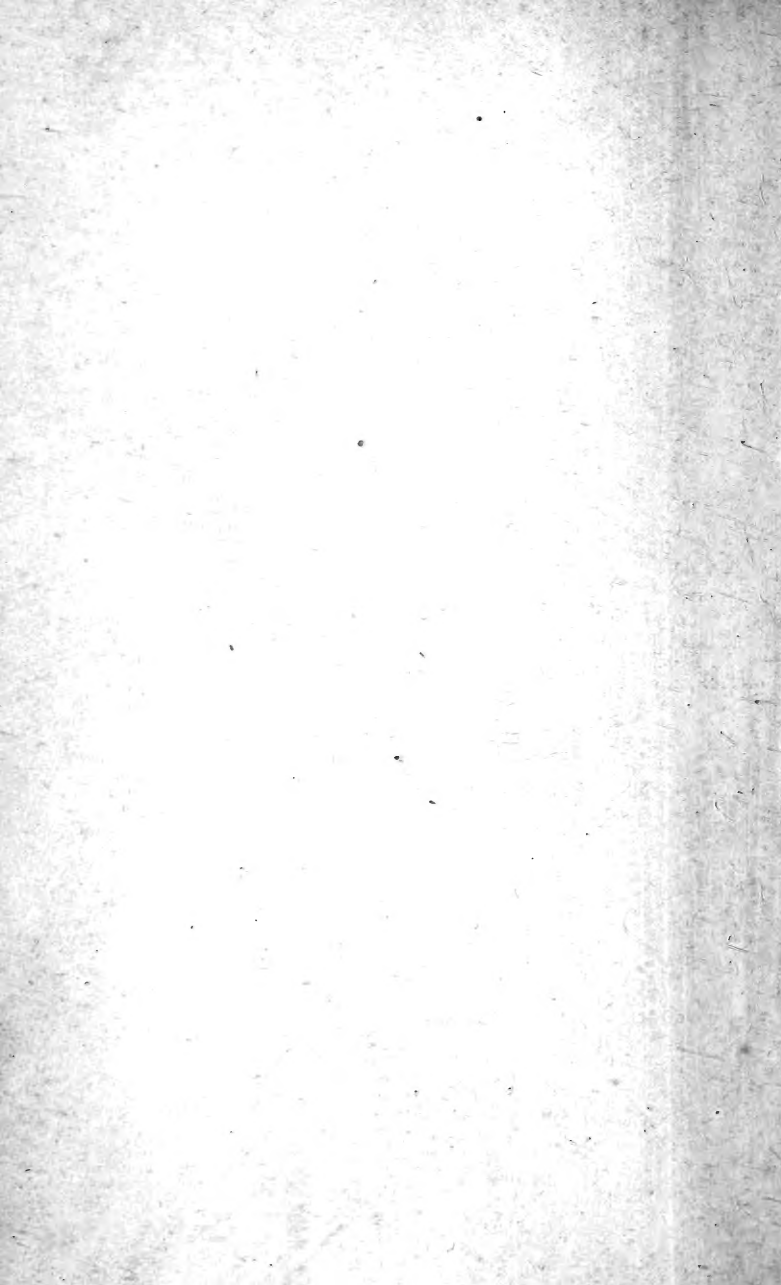
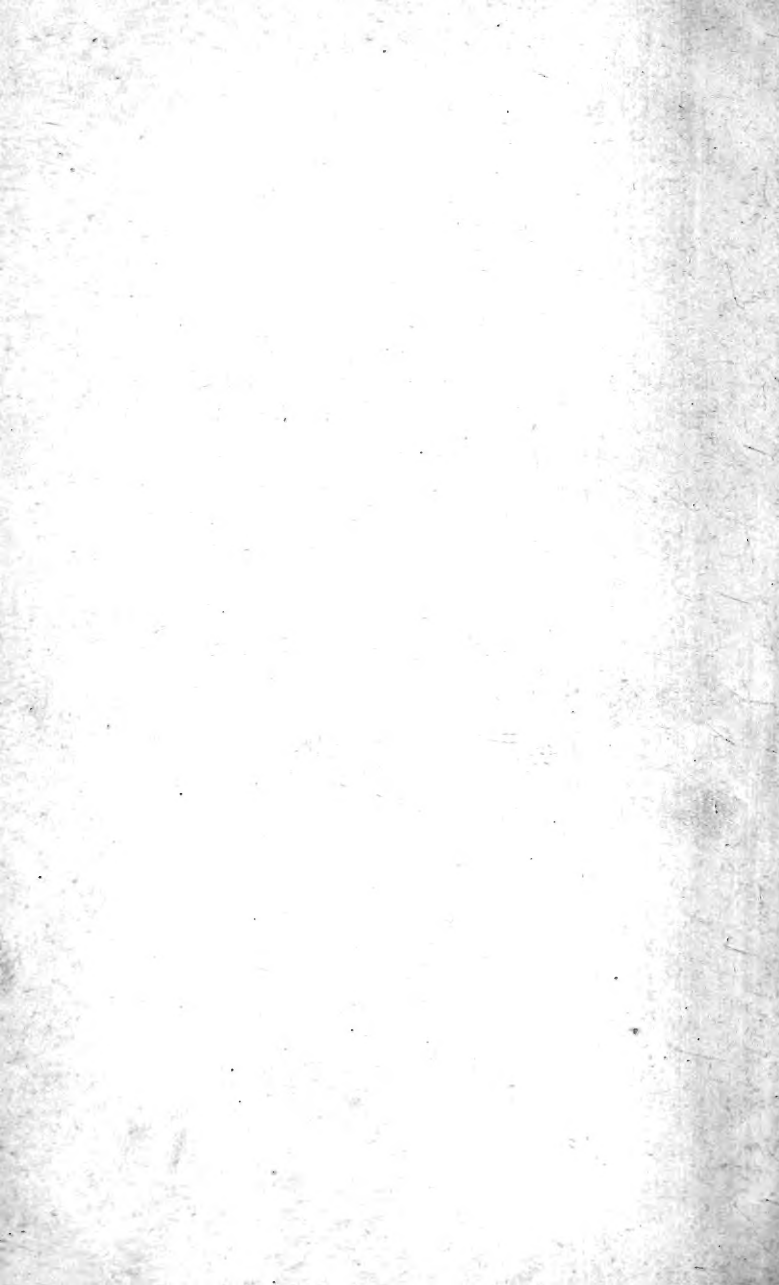


L. R. 1.









THE  
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CONDUCTED BY

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RICHARD TAYLOR, F.L.S. G.S. Astr. S. Nat. H. Mosc. &c.  
SIR ROBERT KANE, M.D. M.R.I.A.  
WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.  
JOHN TYNDALL, PH.D. F.R.S. &c.

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“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster  
vilior quia ex alienis libamus ut apes.” *Jusr. Lips. Polit. lib. i. cap. 1. Not.*

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VOL. IX.—FOURTH SERIES.

JANUARY—JUNE, 1855.

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

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

*J. B. Pinelli ad Mazonium.*





# CONTENTS OF VOL. IX.

(FOURTH SERIES.)

---

## NUMBER LVI.—JANUARY 1855.

	Page
Mr. W. R. Grove on a Method of increasing certain Effects of Induced Electricity .....	1
M. V. Regnault on the Elastic Forces of Vapours <i>in vacuo</i> and in Gases, at different Temperatures; and on the Tensions of the Vapours furnished by mixed or superposed Liquids ( <i>concluded</i> ) .....	4
Sir J. W. Lubbock on the Heat of Vapours .....	25
Mr. A. B. Northcote on the Brine-springs of Worcestershire..	27
Prof. W. Thomson on the Possible Density of the Luminiferous Medium and on the Mechanical Value of a Cubic Mile of Sunlight. ....	36
The Rev. J. A. Galbraith on the Composition of the Felspars of the Granite of the Dublin and Wicklow Mountains ....	40
Prof. Magnus on the Elastic Forces of Vapours of Mixtures of two Liquids .....	44
Sir W. R. Hamilton on some Extensions of Quaternions ( <i>continued</i> ) .....	46
The Rev. A. Thacker on a case of disturbed Elliptic Motion..	51
Notices respecting New Books:—Mr. T. Hopkins on the Atmospheric Changes which produce Rain and Wind, and the Fluctuations of the Barometer .....	54
Proceedings of the Royal Society .....	56
On a peculiar Phænomenon in the Electro-deposition of Antimony, by Mr. G. Gore .....	73
On the Composition and Properties of Fats, by D. Heintz. ...	74
On Phyllyrine, by C. Bertagnini .....	78
Meteorological Observations for November 1854 .....	79
Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall at Boston; and by the Rev. C. Clouston at Sandwick Manse, Orkney.....	80

---

## NUMBER LVII.—FEBRUARY.

Prof. Faraday on some Points of Magnetic Philosophy .....	81
M. W. Eisenlohr on the Action of the Violet and Ultra-violet Invisible Light .....	114

	Page
M. R. Bunsen on the Law of Absorption of Gases. (With a Plate.) . . . . .	116
Prof. Challis on the Eccentricity of the Moon's Orbit. . . . .	130
W. H. M. on the Adjustment of Crystals for Measurement with the Reflective Goniometer. . . . .	138
Prof. Buff on Galvanic Circuits in which Perchloride of Iron enters as an Element. . . . .	139
Prof. Connell on the new Hygrometer or Dew-point Instrument. . . . .	143
Dr. Anderson on the Products of the Destructive Distillation of Animal Substances.—Part III. . . . .	145
Notices respecting New Books:—M. P. T. Riess's Treatise on Frictional Electricity. . . . .	150
Proceedings of the Royal Society. . . . .	152
On the Limit of Vaporization of Mercury, by C. Brame. . . . .	157
Meteorological Observations for December 1854. . . . .	159
Table. . . . .	160

---

#### NUMBER LVIII.—MARCH.

Prof. Faraday's Further Observations on Associated cases, in Electric Induction, of Current and Static Effects. . . . .	161
Prof. Boole on certain Propositions in Algebra connected with the Theory of Probabilities . . . . .	165
Dr. Heddle's Analysis of the Mineral "Edingtonite" . . . . .	179
M. R. Bunsen on the Law of Absorption of Gases ( <i>concluded</i> ). . . . .	181
Prof. Challis on the Theory of the Moon's Motion.—Explanation of the occurrence of non-periodic Factors in the Development of the Radius-vector. . . . .	201
Dr. Tyndall on the Existence of a Magnetic Medium in Space. . . . .	205
The Rev. R. Carmichael's Theorems on the Quadrature of Surfaces and the Rectification of Curves . . . . .	209
Dr. Anderson on the Products of the Destructive Distillation of Animal Substances.—Part III. ( <i>concluded</i> ) . . . . .	214
Mr. W. Crookes on a more convenient form of applying Gallic Acid as a Developing Agent in Photography . . . . .	225
On the Gold Fish of Franklin . . . . .	227
Proceedings of the Royal Society. . . . .	229
On the Object of Salt in the Sea, by Professor Chapman, of University College, Toronto . . . . .	236
On an Appearance seen in the Moon, by Robert Hart, Esq. . . . .	238
Meteorological Observations for January 1855 . . . . .	239
Table. . . . .	240

---

#### NUMBER LIX.—APRIL.

Prof. W. Thomson's Elementary Demonstrations of Propositions in the Theory of Magnetic Force . . . . .	241
--	-----

	Page
Dr. Heddle and Mr. R. P. Greg on British Pectolites . . . . .	248
Prof. Faraday's Magnetic Remarks . . . . .	253
Mr. A. H. Church on the Benzole Series.—Determination of Boiling-points . . . . .	256
The Rev. Prof. Callan on a New Single Fluid Galvanic Battery, more powerful, and less expensive in construction and use, than any of the Nitric Acid Batteries . . . . .	260
The Rev. S. Haughton's Notes on Mineralogy.—No. I. On the Chemical Composition and Optical Properties of the Mica of the Dublin, Wicklow, and Carlow Granites . . . . .	272
M. Melloni on a new Electroscopé . . . . .	276
Sir W. R. Hamilton on some Extensions of Quaternions ( <i>continued</i> ) . . . . .	280
Prof. W. Thomson's Observations on the "Magnetic Medium," and on the Effects of Compression . . . . .	290
Proceedings of the Royal Society . . . . .	293
————— Royal Institution . . . . .	309
On the Artificial Production of Minerals belonging to the family of Silicates and Aluminates by the Reaction of Vapours upon Rocks, by M. Daubrée . . . . .	315
Meteorological Observations for February 1855 . . . . .	319
————— Table. . . . .	320

---

NUMBER LX.—MAY.

Prof. Potter on the Interference of Light near a Caustic, and the Phænomena of the Rainbow . . . . .	321
M. A. J. Angström's Optical Researches. (With a Plate.) . . . .	327
Mr. J. Bridge on the Oblique Aberration of Lenses . . . . .	342
Mr. T. S. Hunt's Examinations of some Felspathic Rocks. . . . .	354
Mr. J. Ball on a doubtful point in Climatology . . . . .	363
Dr. Herapath's Further Researches into the Properties of the Sulphate of Iodo-quinine or Herapathite, more especially in regard to its Crystallography, with additional facts concerning its Optical relations. (With a Plate.) . . . . .	366
Prof. Challis on the Theory of the Moon's Motion.—Results of a Third Approximation . . . . .	370
Mr. T. S. Hunt on the Mineral Wilsonite . . . . .	382
Proceedings of the Royal Society . . . . .	383
On Sensitive Collodion, by Thomas Woods, M.D. . . . .	398
Meteorological Observations for March 1855 . . . . .	399
————— Table. . . . .	400

---

NUMBER LXI.—JUNE.

M. P. Riess on the Action of Non-conducting Bodies in Electric Induction . . . . .	401
--	-----

	Page
Mr. J. Wilson's Researches into the Alum Manufacture. . . . .	413
Prof. Tyndall on the Polymagnet. (With a Plate.) . . . . .	425
Prof. Challis on the Aberration of Light . . . . .	430
M. A. Secchi on the Periodical Variations of Terrestrial Magnetism ( <i>concluded</i> ) . . . . .	432
Dr. Heddle on Table Spar from the Morne Mountains . . . . .	452
Mr. A. H. Church on the Benzole Series.—Part II. Note on some Derivatives of Xylole . . . . .	453
Mr. G. J. Knox on the Existence of an Electrical Æther through Space . . . . .	457
Mr. T. H. Henry on a New Compound of Gold and Mercury. . . . .	458
Proceedings of the Royal Society. . . . .	459
Researches upon the Magnetic Power of Oxygen, by M. E. Becquerel . . . . .	474
On the Alteration of the Volume of some Substances by Heating and Fusion, by Hermann Kopp . . . . .	477
Recent Appointments . . . . .	479
Meteorological Observations for April 1855. . . . .	479
————— Table. . . . .	480

---

#### NUMBER LXII.—SUPPLEMENT TO VOL. IX.

M. Verdet on the Optical Properties developed in Transparent Bodies by the action of Magnetism. (With a Plate.) . . . . .	481
Mr. R. P. Greg on the Crystalline Form of Leucophane . . . . .	510
Mr. F. M. Jennings on an Analysis of the Potash and Soda Felspars of one of the Dublin Mountains. . . . .	511
Mr. A. Cayley on the Porism of the In-and-circumscribed Triangle, and on an Irrational Transformation of two Ternary Quadratic Forms each into itself . . . . .	513
Prof. Plücker's Contributions to our Knowledge of the Nature of the so-called Coercive Force. . . . .	518
Prof. W. Thomson on the Dynamical Theory of Heat.—Part V. On the Quantities of Mechanical Energy contained in a Fluid in Different States, as to Temperature and Density . . . . .	523
Prof. W. Thomson on the Electro-statical Capacity of a Leyden Phial and of a Telegraph Wire insulated in the axis of a cylindrical Conducting Sheath . . . . .	531
Proceedings of the Royal Society. . . . .	535
Experiments relating to Endosmose, by M. Lhermite. . . . .	544
On the Luminous Coat of the Induction Sparks of Ruhmkorff's Apparatus, by T. Du Moncel . . . . .	546
On Complementary Colours, by H. Meyer . . . . .	547
Index . . . . .	548

## PLATES.

- I. Illustrative of Prof. Bunsen's Paper on the Law of Absorption of Gases.
- II. Illustrative of M. A. J. Angström's Optical Researches.
- III. Illustrative of Dr. Herapath's Paper on the Properties of the Sulphate of Iodo-quinine or Herapathite.
- IV. Illustrative of Prof. Tyndall's Paper on the Polymagnet.
- V. Illustrative of M. Verdet's Paper on the Optical Properties developed in Transparent Bodies by the action of Magnetism.

## ERRATUM IN VOL. VIII.

In Table I., p. 421, in the second line of the column headed "Masses,"  
for  $20^{21}$  read  $10^{21}$ .

## ERRATA IN VOL. IX.

- Page 243, foot note, last line, *for against read with*; and *for DC read AB*.  
— — last line but two, *for with read against*.  
— — — *for AB read DC*.  
— — line 6 from foot, *for A read D*.  
— — — *for B read C*.  
Page 354, line 23, *for  $\frac{1}{3}$ th read  $\frac{1}{2}$ th*.



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

[FOURTH SERIES.]

JANUARY 1855.

I. *On a Method of increasing certain Effects of Induced Electricity.* By W. R. GROVE, Esq.\*

IN the course of last year I observed that, by connecting the coatings of a Leyden phial with the extremities of the secondary coil of Ruhmkorff's apparatus, a great increase in the brilliancy of the discharge could be obtained. Circumstances diverted my attention from it at the time, and I did not publish the experiment, though I believe Mr. Gassiot mentioned it in one of his papers. I have since heard that M. Sinsteden in France had made the same observation, though I do not know when, nor whether he has published his experiments.

The point which I now think may be worth insertion in the *Philosophical Magazine*, is the conversion, by means of a Ruhmkorff coil, of an indefinite amount of voltaic power into static electricity. If a small Leyden phial have its coatings connected respectively with the extremities of the secondary wire of a Ruhmkorff coil (the primary being, as usual, connected with the condenser of M. Fizeau, and two wires being attached to the terminals and brought within striking distance), the noise and brilliancy of the discharges are greatly increased, with generally a slight, but a very slight, diminution in their length. If now the voltaic battery be increased, the coil and Leyden phial remaining the same, but little increase in the length or brilliancy of the sparks will ensue, that is, provided the battery was in the first instance sufficiently powerful to give the maximum effect of the coil without the phial. For instance, if with a Ruhmkorff coil of the size now usually made, 10 inches long by 4 diameter,

\* Communicated by the Author.

four cells of 2 inches by 4 of the nitric acid battery be used, and a pint Leyden phial, but little increase of effect will be obtained by using eight or more cells, and the platinum at the contact breaker would be rapidly destroyed by the sparks.

But substitute for the pint Leyden phial one of double the capacity, and it will be found, that though this second phial was inferior to the first with a battery of four cells (giving shorter sparks, and fewer in a given time, though somewhat denser), yet it is far superior to the first with the battery of eight cells, and the sparks at the contact breaker are no longer injurious.

By adding more coated surface, for instance, another phial, four more cells may be added, and increased effects will be obtained, and thus with *the same coil* the brilliancy of the discharge may be increased to an extent to which I have not yet found a limit. I obtained this result some months back; but not having a large battery, I did not go beyond ten cells, which I found would well bear a jar of one square foot coated surface.

By the aid of Mr. Gassiot's more powerful apparatus, I have, with him, used thirty cells of the nitric acid battery, 2 inches by 4, and five square feet of coated surface; the effects were very striking,—a roar of voluminous discharge of 0·6 of an inch long, increased to 1·5 inch when the flame of a spirit-lamp was placed between the terminals. I have never witnessed such a torrent of electrical discharges; the noise could not be borne long without great discomfort.

With the same voltaic battery, and an additional square foot of coated surface, the effect was somewhat diminished. Mr. Gassiot had not more than thirty cells available at the time of our experiments, so that I have not yet ascertained the limit to which this increase of power can be carried. I presume, however, there is a limit, for reasons which will be presently apparent.

The following precautions are essential to the success of the experiment:—

1st. The wire proceeding from the outer extremity of the secondary coil must be connected with the inside or insulated coating of the Leyden battery, if the battery is not wholly insulated. The reason of this is, that the outer extremity of the coil is the better insulated portion, and also that to which electricity of tension flies off; a good spark can, under ordinary circumstances, be obtained from the outer, but scarcely any from the inner terminal of the coil.

2nd. The distance between the hammer of the contact breaker and the soft iron core should be made as great as practicable, at least one-eighth of an inch; this is an important point as to the theory and experimental results of the Ruhmkorff coil. Time, as is well known, is necessary for the development of



electro-magnetism; and M. Matteucci, in his recent valuable book on induced electricity, has shown some remarkable results flowing from this fact. If the hammer be too near the core, the former is raised before the latter has time to be fully magnetized; and when a Leyden condenser is used, further time is required for this to be charged. This demand of time indicates the probable limit to the increase of power to which I have above alluded.

It is very curious to see the *absorption*, so to speak, of voltaic power by the Leyden battery: when the maximum effect for a given Leyden jar has been passed, the contact breaker shows by its sparks the unabsorbed induced electricity which now appears in the primary wire; an additional jar acts as a safety-valve to the contact breaker and utilizes the voltaic power, and so on.

It is a question of some interest why a jar charged in the ordinary way by temporary contact of the terminals of a secondary coil will only receive a very slight charge, and give a discharge of scarcely measurable length, yet when permanently connected with the terminals, will give a long and powerful discharge. The following is the best theory I can offer. At the moment of the inductive action or wave of electricity, the same wire which is affected by the electric impulse is unable to conduct it back again, and thus to discharge the jar; while, when the jar is attempted to be charged in the ordinary way, the contact, however apparently of short duration, lasts longer than the single impulse of electricity, and so the coil in great part discharges the jar. Some such state of the wire as that I have suggested must exist at the moment of an induced current, as otherwise the wire would discharge itself, or in other words, would never receive a charge or state of opposite electricity of great tension at its extremities. At one time I considered the explanation to be, that at the moment of breaking contact a portion of the induced electricity flies off across the discharging interval in the form of a spark, and thus enables the jar to discharge itself just as the voltaic arc will pass across the path of an electric spark, though it will not pass through a measurable distance of interposed air without the spark. This theory, however, does not satisfactorily explain the great increase in the charge of the Leyden phial, as compared with the charge by contact.

3rd. It must be borne in mind that each coating of the Leyden phial must be connected with each terminal; the jar is not, as many have tried the experiment, to be interposed in the secondary circuit.

The number of discharges in a given time will depend upon the intensity of the battery, and its relation to the amount of coated surface; the eye cannot estimate this, but a rough mea-

surement of the rapidity of succession may be made in the following way. Move across the line of discharge with a steady hand a strip of writing-paper, it will be punctured with a row of holes, which will be the more closely approximated in proportion as the succession of discharges is more rapid. By a disc of paper attached to an axis moving with a given rate of revolution, this measurement may be made very accurate and useful.

Those who possess the coil apparatus will find it very convenient to have a plate of glass, coated on each side with tinfoil, placed on the base of the machine, and having strips leading from each coating to binding-screws, with which the terminals of the secondary coil can be corrected at will.

December 7, 1854.

II. *On the Elastic Forces of Vapours in vacuo and in Gases, at different Temperatures; and on the Tensions of the Vapours furnished by mixed or superposed Liquids.* By V. REGNAULT\*.

[Concluded from vol. viii. p. 280.]

PART III.—*On the Elastic Force of Vapours in Gases.*

PHYSICISTS in general admit that vapours behave in the same manner in gases as *in vacuo*, with this difference only, that in gases the equilibrium of tension is slowly established, whilst *in vacuo* it is produced almost instantaneously. An apparatus invented by Gay-Lussac is usually employed in lectures on physics, to show that the elastic force of vapours is exactly the same in gases as *in vacuo*. Nevertheless there is no record of any exact experiments having been made for the establishment of this law, which is of great importance, especially in meteorology.

During my hygrometric investigations (published in the fifteenth volume of the *Annales de Chimie et de Physique*†), I very carefully determined the weight of the aqueous vapour existing in the atmosphere at its different states of saturation. I ascertained that, even during long-continued rains, which must have maintained the air in a state of saturation, the quantity of water found by experiment was always less than that determined by my calculations, based on the one hand upon the elastic force which I had found for aqueous vapour *in vacuo*, and on the other upon the theoretical density of this vapour.

This circumstance might arise from two causes:—

1. The elastic force of vapour of water might be exactly the same in the air as *in vacuo*;

\* From the *Comptes Rendus* for August 21 and 28, 1854.

† Taylor's Scientific Memoirs, Part XVI.

2. The actual density of aqueous vapour in the air might differ from that obtained by calculations founded upon Mariotte's law and the theoretical density; for this density was only verified by Gay-Lussac at a temperature of  $100^{\circ}$  C., and under pressures which differed but little from the ordinary pressure of the atmosphere.

I wished first of all to determine as exactly as possible the weight of vapour which might be contained in air saturated with humidity at different temperatures; for it might be objected, that in the experiments in which I derived the saturated air directly from the atmosphere, there remained some uncertainty as to the exact determination of the temperature. My new experiments were free from this objection, for I drew the air, by means of an aspirator, into a series of tubes filled with wet sponge, and kept at a strictly invariable temperature during the whole course of the experiment. Sixty-eight determinations effected in this manner between limits of temperature from  $0^{\circ}$  to  $27^{\circ}$  C. all gave the weight of vapour lower than that deduced from calculation. The differences were, however, inconsiderable, rarely rising to  $\frac{1}{30}$ th of the total weight.

The fact being thus completely proved, its cause remained to be discovered, and for this purpose I undertook experiments to determine directly the density of aqueous vapour within limits of temperature analogous to those existing in my first experiments, as well as the elastic force of this vapour in the air. Unfortunately the direct determination of aqueous vapour in gases at low temperatures presents difficulties which are almost insurmountable, the ponderable quantity of this vapour being too small in proportion to that of the whole elastic fluid. I was therefore compelled to confine myself to making experiments *in vacuo*. As long as the fraction of saturation does not attain  $\frac{8}{10}$ ths, I found the density of aqueous vapour to be equal to that deduced from the theoretical density, by the application of Mariotte's law. But this density increases rapidly in proportion as it approaches saturation. I think, nevertheless, that this rapid increase is produced principally by a condensation of liquid water upon the walls of the balloon in which my experiments were made; and this condensation commences long before saturation.

I could find no means of getting rid of this cause of error, and I believe that the most exact method of determining the density of a vapour in a gas still consists in condensing, by means of absorbing substances, the vapour existing in a given volume of gas, under known conditions of temperature and pressure. But in this case the law of elastic forces must be thoroughly settled.

I have detailed (at page 134 of my memoir) \* the experiments which I made in order to determine directly the elastic force of aqueous vapour at saturation in air and nitrogen gas at different temperatures. The ninety-one determinations which I made between  $0^{\circ}$  and  $40^{\circ}$  C. gave, without exception, the elastic force of aqueous vapour as weaker in the air than *in vacuo*. The differences are of the same kind as those previously found between the weight of water which saturates a given volume of air, and the weight deduced from calculation. Nevertheless, in these experiments I endeavoured to vary the circumstances as far as possible, and especially to realize those conditions in which it might be expected that saturation would be obtained.

Aqueous vapour having but a feeble tension at low temperatures, it became important to extend these experiments to more volatile fluids. I accordingly made use of æther, sulphuret of carbon, and benzine. The apparatus employed was that described in the *Annales de Chimie et de Physique*, 3 sér. vol. xv. p. 131 †. Into a balloon, of the capacity of 600 to 700 cubic centims., a small hermetically-closed capsule was introduced, containing the liquid to be operated upon. This balloon, to the neck of which a tube of considerable diameter had been previously attached, communicated with a mercurial manometer. The entire apparatus was placed in a large tub filled with water, which was kept at a constant temperature. A piece of glass was let into one part of the side of the tub, through which the manometer might be observed. The apparatus being arranged, a vacuum was repeatedly produced in the balloon, the air admitted after each exhaustion being perfectly dry. Lastly, the apparatus was hermetically sealed, containing air under the pressure of the atmosphere.

The elastic force acquired by this dry air, kept constantly at the same volume, at different temperatures, having been determined, the capsule was ruptured by the action of heat, and the series of observations was recommenced on the air saturated with vapour. The liquid enclosed in the capsule had previously been kept from contact with the air, in order that it should not have a tendency to absorb the air of the balloon. The volume of the capsule in proportion to that of the balloon may also be varied in different experiments, so as to make sure that the absorption or disengagement of air by the liquid has not caused any sensible perturbation. The following are some of the results that I have obtained :—

\* Scientific Memoirs, vol. iv. p. 609.

† Ibid. p. 706.

*First Series.*

Capacity of the balloon, 668 cub. centims. Capsule containing 7.4 cub. centims. of liquid. Elastic force of the air at 0°C., 731.89 millims.

Temperatures.	Elastic force of the vapour in the gas.	Elastic force of the vapour <i>in vacuo</i> .	Difference.
			mm
5.17	225.94	232.5	6.6
14.42	340.15	345.3	5.1
14.38	336.48	344.5	8.0
20.78	439.50	445.6	6.1
20.78	439.78	445.6	5.8
11.09	297.10	300.2	3.1
11.11	296.78	300.6	3.8
19.37	414.02	423.1	9.1
12.22	311.30	315.0	3.7

*Second Series.*

Capacity of the balloon, 690 cub. centims. Weight of liquid contained in the capsule, 7.7 grms. Elastic force of the air alone at 0°C., 724.22 millims.

In this series the operations were carried on with a descending temperature, so as to realize saturation with greater certainty.

33.51	702.69	722.8	20.1
33.62	705.09	726.0	20.9
30.97	645.62	659.0	13.4
26.52	552.67	559.2	6.5
22.83	479.63	484.0	4.4
20.05	429.69	433.9	4.2
19.99	428.88	433.0	4.1
14.26	337.71	341.0	3.3

These tables, in which I have only inserted the greatest tensions observed at given temperatures, show that the elastic force of vapour of æther in the air is constantly less than that obtained *in vacuo*; the difference becomes greater in the same proportion as the elastic forces themselves.

Fearing that in the preceding experiments the tube by which a communication was established between the balloon and the manometer might be too small to allow of the necessary diffusion of the vapour, I replaced it by one of the same diameter as the manometric tube itself, that is to say, 22 millims. Under these conditions the following series of experiments was made. A small quantity of the liquid was always distilled over into the manometric tube before the commencement of the observations.

*Third Series.*

Elastic force of the air at 0° C., 706·91 millims.

Temperatures.	Elastic force of the vapour in the air.	Elastic force in <i>vacuo</i> .	Difference.
16 <sup>h</sup> ·73	368·81	379·7	mm 10·9
16·75	369·39	379·8	10·4
20·78	435·21	445·8	10·6
20·79	435·26	445·9	10·7
29·31	583·81	619·0	36·2
29·31	583·68	619·0	30·3
29·31	593·03	619·0	26·0
22·84	470·13	483·8	13·7
22·86	466·03	483·9	17·9
22·84	467·54	483·8	16·3

It will be observed that this series presents still greater differences than the preceding, but this arises from the maximum determinations only having been inserted. The observations at the same temperature were made at intervals of half an hour.

It would take up too much time to indicate here the different circumstances under which the partial determinations for these three series were effected,—these circumstances exercise a great influence upon the values found for the elastic force of the vapour. I shall content myself with saying, that very often a determination was not made until after the bath had been kept for several hours at a constant temperature and in continual agitation. In other cases the bath was allowed to cool very slowly, keeping it constantly agitated, and the observations were made during this period of descent in the temperature, which was very favourable to saturation.

The greatest value was obtained for the elastic force at a given temperature, by suddenly cooling the balloon so as to induce an abundant condensation of the vapour, and then rapidly re-establishing the original temperature. By making observations afterwards every quarter of an hour, the temperature of the bath remaining exactly the same, the elastic force will be found to diminish gradually during several hours. If, on the contrary, the temperature of the bath be raised gradually up to the stationary point, and observations be then made every quarter of an hour, it will be seen that the tension of the vapour constantly increases, without, however, attaining, not only the tension which it presents *in vacuo*, but even that observed at the same temperature in the experiments by refrigeration immediately after the condensation of the excess of vapour. I shall give the explanation of these facts hereafter.

The general tendency of these experiments is to show that the elastic force of vapour of æther in the air is constantly weaker than *in vacuo*. The absolute difference is greater than with water, and increases with the elastic force itself.

The experiments which I have made with sulphuret of carbon and benzine lead to the same result. The difference between the elastic force presented by the same vapour in the air and *in vacuo*, diminishes in proportion to the volatility of the liquid, as may be seen from the following tables:—

Tensions of the vapour of sulphuret of carbon in the air.			
Temperatures.	Elastic force of the vapour in the air.	Elastic force of the vapour <i>in vacuo</i> .	Difference.
13·45	227·38	229·3	mm 2·0
22·79	328·66	332·8	4·1
22·81	328·90	333·0	4·1
30·65	441·41	445·7	4·3
Tensions of the vapour of benzine in the air.			
10·64	46·80	48·2	1·4
10·67	46·98	48·3	1·4
10·68	46·92	48·3	1·4
11·37	47·96	50·3	2·4
11·38	48·57	50·3	1·7

In order to investigate this same subject, I have employed a method which presents many advantages over the preceding. It allows us to study with precision the influence exerted upon the elastic force of the vapour by the total pressure of the gaseous atmosphere which acts upon the volatile liquid, and the quantity of liquid in excess by which the walls of the vessel are moistened. For this purpose I made use of the eudiometer which I applied to the analysis of gases, and which is described in the *Annales de Chimie et de Physique*, vol. xxvi. p. 333.

I replaced the measuring tube of this apparatus, which usually consists of a glass tube traversed by two platinum wires, by another glass tube which was not furnished with wires, and which was graduated in centimetres so as to indicate the volume of gas. Into this tube the gas in which the volatilization was to be effected was introduced, with the precautions indicated in the memoir just quoted; then, keeping the bath which surrounded the apparatus constantly at a temperature of 7°·7 C., which was very near that of the surrounding atmosphere, the elastic force presented by this dry gas was determined by the

cathetometer, the surface of the mercury in the measuring tube being brought successively to each of the divisions marked upon the tube.

This done, a certain quantity of the volatile liquid was introduced into a test tube, which had been previously filled with dry mercury, exhausted *in vacuo*; then, after the communication between the two tubes had been established, the gas was passed from the measuring tube into the other, under a pressure sufficiently low to allow the whole of the liquid introduced to volatilize in the gaseous space thus afforded it. Lastly, the gas mixed with vapour was repressed into the measuring tube.

A new series of determinations as to the elastic force of the gaseous mixture was then commenced, by causing this to occupy successively the same volumes that had been occupied by the dry gas in the first series. It is clear that by deducting from the numbers of the second series the corresponding numbers of the first, we get the elastic forces of the vapour at a constant temperature, but under variable pressures and different volumes, and consequently in variable states of saturation.

From these different observations it may be deduced:—

1. From the first series, whether the dry gas follows the law of Mariotte. I shall give in another memoir the observations which I have made in this manner upon the compressibility of a great number of gases. The method, when executed with the necessary care, allows us to prove that atmospheric air itself departs sensibly from this law under the slightest variations of volume. The differences belong to the order of those deduced from the curve which I have already published, and which includes a great interval of pressure. By the same method I have determined the law of compressibility followed by mixtures, in known proportions, of atmospheric air with a gas, which, like carbonic acid, departs greatly from Mariotte's law.

2. By combining the two series, we may ascertain how far the gas, more or less charged with vapour, departs from Mariotte's law, when it is more or less distant from the state of saturation.

3. We may determine how the elastic force of the vapour varies from the moment when the liquid begins to be deposited in drops upon the walls of the tube, and consequently ascertain whether the elastic force of this vapour varies in proportion to the quantity of liquid condensed.

I give here the series of experiments which I have made upon vapour of æther in atmospheric air and hydrogen gas. I have subjoined other experiments by which I have determined the elastic force of the same vapour in carbonic acid gas, although I do not think that the latter can be employed with confidence in the determination of the tension of the vapour, in consequence



of the great solubility of carbonic acid gas in æther, which in this case produces a considerable perturbation.

On the tension of the vapour of æther in the air.

(Tension of the vapour *in vacuo* at 7°·7 C. = 260 millims.)

Numbers of the divisions.	Elastic force of dry air.	Elastic force of air with the vapour.	Elastic force of the vapour of æther.
54	611·03	775·93	164·90
52	630·93	801·33	170·40
50	652·13	828·18	176·05
48	675·03	857·08	182·05
46	690·23	887·58	188·35
44	725·58	921·08	195·50
42	753·68	956·68	203·00
40	784·53	995·18	210·65
38	817·83	1036·98	219·15
36	853·73	1082·08	228·35
34	893·13	1131·48	238·35
32	936·68	1182·13	245·45
30	984·58	1232·28	247·70
28	1037·13	1287·58	250·45
26	1097·03	1348·73	251·70
24	1163·83	1416·23	252·40
22	1238·13	.....	.....
54	.....	775·83	.....

A fresh quantity of æther was introduced into the laboratory tube and made to dissolve in the air.

54	.....	862·63	251·60
52	.....	883·63	252·70
50	.....	905·63	253·50
48	.....	929·38	254·35
48	} on the following day.	929·28	254·25
46		954·23	255·00
44	.....	981·08	255·50
42	.....	1010·18	256·50
40	.....	1041·28	256·75
38	.....	1075·18	257·35
36	.....	1111·58	257·85
34	.....	1151·31	258·18
32	.....	1195·68	259·00
30	.....	1243·98	259·40
28	.....	1297·28	260·15
26	.....	1357·03	260·00
24	.....	1423·93	260·10

The æther was in excess from the commencement of this second series.

Tensions of the vapour of æther in hydrogen gas.

(The measures were all made at a temperature of  $7^{\circ}\cdot7$  C.

Numbers of the divisions.	Hydrogen alone.	Hydrogen with æther.	Tensions of the vapour of æther.
48	570·73	813·58	242·85
	after $\frac{1}{4}$ h.	816·18	245·45
46	591·63		
44	613·68	864·13	250·45
42	637·78		
40	663·68	916·48	252·80
38	691·33		
36	721·93	977·08	255·15
34	755·88		
32	787·38	1048·38	261·00
30	832·68		
28	877·88	1135·08	257·20
26	927·68		
24	984·38	1242·98	258·60
22	1048·33		
20	1121·13	1380·88	259·75
18	1154·13		
48	570·73		

The volume of gas is augmented, the temperature raised  $1^{\circ}\cdot5$  C., the apparatus left for two hours and then brought again to  $7^{\circ}\cdot7$  C.

48	.....	816·88	246·15
24	.....	1243·68	259·30
24	.....	1243·38	259·00 aft. $\frac{1}{4}$ h.
20	.....	1381·73	260·60

*Second Series.*

Numbers of the divisions.	Hydrogen alone.	Gas with vapour.	Elastic force of the vapour.
60	633·63	885·28	251·65
56	672·43	926·33	253·90
52	716·13	971·43	255·30
48	766·28	1022·43	256·15
44	823·88	1081·08	257·20
40	890·48	1149·58	259·10
36	959·63	1228·63	259·10
32	1063·48	1324·08	260·60
28	1178·33	1439·98	261·65

The æther had remained in the gas for fifteen hours before the commencement of observations.

Tensions of the vapour of æther in carbonic acid gas.  
Temperature 7°·7 C.

Numbers of the divisions.	Gas alone.	With æther.	Elastic force of the vapour.
59	535·38	674·18	138·80
57	551·38		
56	560·28	704·68	144·40
52	596·38	749·98	153·60
48	637·98	801·40	163·52
44	685·38	860·88	175·50
40	740·38	929·48	189·10
36	805·88	1009·88	204·10
32	883·38	1105·83	222·45
28	977·58	1213·83	236·25
24	1095·68	1333·93	238·25
22	1165·78	1404·08	238·30
20	1245·98		
18	1337·93		
A fresh quantity of æther introduced.			
59	.....	769·78	234·40
56	.....	799·33	239·05
52	.....	839·33	242·95
48	.....	882·33	244·35
44	.....	930·36	244·98
40	.....	986·18	245·80
36	.....	1050·28	244·40
32	.....	1126·53	243·15
28	.....	1219·21	241·63
24	.....	1333·53	237·85
22	.....	1401·21	235·43

In these experiments I have noted the precise moment when the vapour began to be deposited in drops on the walls of the tube. I have constantly found that the vapour is then still far from possessing the elastic force of 260 millims., which it presents *in vacuo*. If the compression of the gas be continued, the condensed liquid becomes more abundant and the tension of the vapour increases, approaching more and more to that observed *in vacuo*. But an equality is not attained until a thick stratum of liquid has collected on the surface of the mercury. Moreover this equality is only to be observed immediately after the reduction in volume, for the tension afterwards diminishes, and this decrease continues to progress for several hours, although the temperature may be carefully maintained.

The explanation of these various facts appears to me to be very simple. When a volatile liquid in a limited space is in contact at the same time with a gas which fills this space and with the substance of which its walls are composed, it has a tendency to produce vapour until the tension of the latter shall be equal to that acquired by the vapour *in vacuo* at the same temperature.

But this vapour comes in contact with the walls of the vessel, which by their hygroscopic affinity condense a portion of it on their surface. The space is consequently below saturation as long as its walls have not condensed the quantity of vapour necessary to satisfy their attractive action upon the vapour. This quantity may be valued by the thickness of the liquid stratum. On the vertical portions of the wall the liquid stratum cannot increase indefinitely in thickness, as its weight tends to make it run towards the bottom. It will be understood, therefore, that if the stratum which remains adherent to the wall be too thin to neutralize the attractive action of the wall upon the vapour, the space must lose vapour; and if the lost vapour be not supplied with the same rapidity by the excess of liquid, the elastic force must always appear less than that observed *in vacuo*.

In this manner is explained why the greatest elastic forces corresponding with a given temperature are always observed immediately after a diminution of volume, or after refrigeration, because then the liquid which is just condensed has saturated the wall, and has not had time to run down by its own weight. This drainage gradually takes place, and the elastic force of the vapour is diminished in consequence. A similar effect cannot be produced *in vacuo*, because in this case the vaporization of the liquid is very rapid, and immediately replaces the vapour which has condensed upon the walls.

Many facts observed by physicists may also be explained in this manner. The air saturated with vapour by an abundant rain, descends below saturation soon after the cessation of the rain, because the bodies which are immersed in it deprive it of vapour by a hygroscopic action. If air be drawn by an aspirator from a space where it is saturated with vapour, and passed through a dry glass tube kept at the same temperature as the air, the walls of the tube will become covered with an abundance of drops, &c.

To sum up, I believe we may conclude from my observations in general, that Dalton's law regarding mixtures of gases and vapours may be regarded as a theoretical law, which would probably be verified most strictly if we could enclose the gas in a vessel, of which the walls might be formed of a certain thickness of the volatile fluid itself. But this law can only be very imperfectly realized in our apparatus; the hygroscopic affinity of their walls reducing the vapour to a variable tension, which is always lower than that corresponding with saturation.

PART IV.—*On the Elastic Force of the Vapours emitted by volatile Liquids mixed by mutual solution, or superposed.*

It is generally admitted in works on physics, that a mixture of several volatile substances which do not combine chemically, evolves complex vapours, of which the total elastic force in a state of saturation is equal to the sum of the tensions which would be produced by each of the liquids separately. This proposition, moreover, is nothing but a particular case of the general law known as *Dalton's law*, and which would apply to every mixture of volatile fluids, permanent gases, or vapours. This law is said to have been established by Dalton, as the consequence of direct experiments; and M. Biot, in his *Traité de Physique* (vol. i. p. 308), has described the process employed for this purpose. This process is very imperfect, and it is difficult to imagine how Dalton can have obtained exact results from it, especially in a question which requires exact measurements, like that at present under consideration.

Gay-Lussac is also said to have verified Dalton's law upon the elastic forces of mixed vapours, but I have nowhere found traces of the experiments of this celebrated physicist. M. Biot (*op. cit.* vol. i. p. 297) cites the experiments made by Gay-Lussac to determine the density possessed by the complex vapour, furnished by mixtures of variable proportions of alcohol and water, which were completely vaporized in the same space. Gay-Lussac found that the weight of the complex vapour is exactly the same as if the vapours of the liquids were isolated; and he concluded from this, that the two substances dissolved in the first instance undergo a molecular separation in acquiring the form of vapour, and that they then behave as though each of them were separate, without any mutual influence. But it must be observed, that in Gay-Lussac's experiments the vapours were not in a state of saturation, for they were at a temperature of  $212^{\circ}$  F., and under a pressure less than that of the atmosphere; and besides, they were not in contact with an excess of the liquid from which they had been produced. Gay-Lussac's experiments, as transmitted to us by M. Biot, have consequently no bearing upon the present question.

It may be said that Dalton's law has been accepted, because it appeared to be a natural consequence of the ideas already formed upon the constitution of the aëriiform fluids, and which have been developed by Laplace, Poisson, and other mathematicians. Physicists have not thought it necessary to verify this law by direct experiment.

I have made my experiments only upon mixtures of two liquids and *in vacuo*. The case of three, or a greater number of liquids

in the mixture, would have given too much complication to the question. I have, of course, only operated upon liquids which do not combine chemically by simple mixture. But we must commence by defining exactly what must be understood by substances which react, or do not react chemically upon each other. By substances which react chemically, we must understand those which, when mixed, give rise to a new substance, endowed with properties distinct from those of the primitive substances, and constituted according to the rules of definite proportions. When bodies are mutually soluble, they do not give rise to new compounds; nevertheless, in the phenomenon of solution, particular molecular actions take place, which distinctly modify certain individual physical properties.

I shall divide the binary mixtures of volatile substances upon which I have operated into three classes:—

*First class.* Binary mixtures of volatile substances which are not mutually soluble.

*Second class.* Binary mixtures of volatile substances which are mutually soluble in larger or smaller proportions, but which are employed in such quantities, that, after reciprocal saturation, there remain two separate liquids.

*Third class.* Binary mixtures of volatile substances which are mutually soluble in all proportions, and therefore always furnish a single fluid.

The process employed in these experiments is the same as that applied to the determination of the elastic force of the vapours furnished by isolated substances; the results are therefore perfectly comparable.

When the substances exercised little or no dissolving action upon each other, a single capsule was filled with nearly equal volumes of the two liquids, and then hermetically closed by the lamp. To deprive the liquids of air, they were placed separately for a few moments under the receiver of the air-pump, so as to expel the air by ebullition under a low pressure. The capsule was put into the balloon of the apparatus, and the operation was then carried on exactly as in the determination of the elastic force of the vapour of a homogeneous substance.

In other cases a much larger quantity of the two liquids was introduced into the balloon, and the air was completely expelled from the apparatus by boiling the liquids under a low pressure by the aid of the air-pump. However, these two processes gave the same results, so that they may be employed indifferently. Care was taken also, before making an observation, to keep the bath at a constant temperature for a quarter of an hour, and often longer still. This rendered it certain that a state of equilibrium was obtained.

FIRST CLASS.—*Binary mixtures of volatile substances which exercise no sensible solvent action upon each other.*

The number of binary mixtures of this class which may be experimented upon with certainty is much more restricted than might be supposed at the first glance, since one essential condition in experiments of this kind is, that the substances employed must be perfectly pure, and they must also both be sufficiently volatile to enable their individual tensions and that of their mixture to be determined with the necessary exactness. Lastly, it is probable that there are in reality no two volatile substances which are absolutely exempt from those reciprocal molecular actions which produce the phenomenon of solution. Everyone knows that water which has remained for a considerable time in contact with essential oils, which are considered insoluble, acquires a very distinct odour of these substances, without its being possible to detect the mechanical interposition of the smallest particle of essence.

The only mixtures of this class upon which I have operated are,—

The mixture of water and sulphuret of carbon.

The mixture of water and chloride of carbon,  $C^2 Cl^6$ .

The mixture of water and benzine.

The following tables give the results which I have obtained. With regard to each temperature at which an observation has been made, we find the elastic force of the vapour given by the mixture; the elastic forces of the vapours of each of the substances separately, taken upon the curves of which I have previously given the elements, or which I have determined directly for this purpose; and lastly, the sum of the two individual elastic forces.

Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of water.	Elastic force of the vapour of sulphuret of carbon.	Sum of the elastic forces.	Differences.
8·85	196·81	8·48	189·2	197·7	mm 0·9
12·07	225·93	10·51	216·7	227·2	1·3
18·85	299·52	16·20	285·2	301·4	1·9
26·87	412·28	26·32	388·7	415·0	2·7
14·10	247·43	11·99	236·0	248·0	0·6
22·43	347·17	20·17	328·5	348·7	1·5
38·35	634·60	50·26	584·9	635·2	0·6
31·80	498·74	34·96	464·8	499·8	1·1

<i>Mixture of chloride of carbon, C<sup>2</sup> Cl<sup>6</sup>, and water.</i>					
Tempera- tures.	Elastic force of the vapour of the mix- ture.	Elastic force of the vapour of water.	Elastic force of the vapour of sulphuret of carbon.	Sum of the elastic forces.	Differences.
7·79	63·49	7·90	52·5	60·4	mm 3·1
11·39	75·37	10·05	62·5	72·5	2·8
16·75	97·25	14·19	80·4	94·6	2·6
20·49	115·69	17·92	95·2	113·1	2·6
25·66	146·58	24·49	119·7	144·2	2·4
29·12	170·77	30·00	138·6	168·6	2·2
34·42	214·67	40·50	172·2	212·7	2·0
38·59	256·42	50·92	203·7	254·6	1·8
44·59	328·38	69·91	256·6	326·5	1·9
<i>Mixture of water and benzine.</i>					
10·10	54·92	9·23	47·0	56·2	1·3
12·38	61·93	10·72	52·4	63·1	1·2
15·26	72·34	12·91	60·5	73·4	1·1
18·01	83·00	15·36	69·2	84·6	1·6
19·88	91·49	17·26	75·7	93·0	1·5
22·53	104·28	20·30	86·0	106·3	2·0
10·53	56·03	9·49	48·2	57·7	1·7

It will be seen from these tables, that the elastic forces observed for the mixtures are very nearly equal to the sum of the elastic forces given by the separate substances. With sulphuret of carbon and benzine the tension is constantly a little weaker than the sum of the tensions. The opposite result is presented by the mixture of water and chloride of carbon. I nevertheless regard this latter fact as an anomaly, which I attribute to the circumstance, that, notwithstanding all the care which had been taken, the chloride of carbon was not absolutely pure. I have already said, that a very small quantity of a foreign substance, inappreciable by our ordinary chemical reagents, exerts a very sensible influence upon the elastic force of the vapour.

From these experiments I think we may conclude, as a theoretical law, that *two volatile substances, which are not capable of mutual solution, give a tension of vapour equal to the sum of the tensions presented by these substances separately*, although it is probable that this will never be verified absolutely upon natural substances, because there certainly exist no two substances entirely deprived of all reciprocal solvent action.

This is consequently a verification of Dalton's law; but we shall see from what follows, that this is the only case to which Dalton's law applies.



SECOND CLASS.—*Binary mixtures of volatile substances which dissolve each other in larger or smaller, but not unlimited proportions.*

It is very difficult to make mixtures of substances which come in this category. I have only operated upon mixtures of æther and water. They gave the following results:—

Temperatures.	Elastic force of the mixture.	Elastic force of pure water.	Elastic force of æther.
15·56	362·95	13·16	361·4
20·40	440·32	17·83	440·0
26·73	562·79	26·09	563·6
33·08	710·02	27·58	711·6
27·99	589·38	28·08	590·0
24·21	510·08	25·30	510·0

In this case the mixture, far from giving a vapour possessing a tension equal to the sum of the individual elastic forces of the separate substances, scarcely presents that of the æther alone.

THIRD CLASS.—*Binary mixtures of liquids mutually soluble in every proportion.*

This class includes the most numerous binary mixtures. I have operated upon three mixtures in various proportions of sulphuret of carbon and æther, two liquids which are particularly adapted to investigations of this nature, as they both possess vapours of considerable tensions at low temperatures:—

Upon two mixtures of chloride of carbon,  $C^2Cl^6$ , and sulphuret of carbon; and

Upon a mixture of benzine and alcohol.

<i>Mixture of nearly equal weights of æther and sulphuret of carbon.</i>			
Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of æther.	Elastic force of the vapour of sulphuret of carbon.
0	182·92	182·3	126·5
-16·71	80·59	80·3	56·3
-11·36	105·26	106·0	74·0
-11·02	107·67	108·0	75·2
- 8·94	119·18	119·6	83·4
- 8·53	121·77	122·2	84·8
- 8·44	122·73	122·5	85·2
- 7·15	130·40	131·4	90·8
- 7·14	130·82	131·3	90·8
- 4·01	151·76	152·3	106·0
+ 8·93	271·38	274·0	190·2
+ 8·94	271·26	274·1	190·3
+ 8·96	270·92	274·4	190·5
+ 9·07	274·02	275·7	191·3

<i>Mixture of 62 volumes of sulphuret of carbon and 38 volumes of æther.</i>			
Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of æther.	Elastic force of the vapour of sulphuret of carbon.
4.72	207.58	228.2	158.8
9.31	252.33	278.8	193.8
12.60	288.96	319.5	221.8
17.00	344.14	384.0	264.5
20.54	395.52	442.3	305.0
24.07	451.79	508.7	349.5
27.19	506.63	573.2	392.8
30.79	575.90	654.3	448.3
33.28	627.82	717.2	489.0
36.01	688.73	786.5	539.5
39.44	772.49	927.0	607.2
<i>Mixture of 56 volumes of æther and 41 volumes of sulphuret of carbon.</i>			
8.01	253.76	263.0	183.0
9.46	268.81	280.0	195.0
11.17	288.55	301.4	209.2
12.88	309.50	323.3	224.0
16.22	253.33	371.8	257.0
18.62	388.42	410.8	282.6
21.12	434.88	452.3	312.0
23.68	478.38	500.0	344.0
26.89	538.77	566.7	388.8
30.09	603.93	638.0	436.2
32.65	659.45	699.3	478.8
35.61	730.33	777.2	531.6
38.18	795.80	856.2	581.5

These tables show that mixtures of sulphuret of carbon and æther, far from giving vapours of a tension equal to the sum of the tensions of the two separate vapours, exert an elastic force which is generally less than that which would be produced by the most volatile of the two liquids. The difference increases in proportion to the volume of the less volatile liquid.

As it was my intention only to study the phenomenon in its general bearings, I did not take great care in the determination of the quantities of liquids mixed, particularly as their proportions change by the mere fact of vaporization. I have had an apparatus constructed by means of which I proposed to continue these experiments, operating with exactly known and suitably graduated mixtures, and which would allow the determination, at any moment, of the constitution of the atmosphere of vapour. Time has not hitherto permitted me to make use of it.

<i>Mixture of equal volumes of chloride of carbon, C<sup>2</sup> Cl<sup>6</sup>, and sulphuret of carbon.</i>			
Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of sulphuret of carbon.	Elastic force of the vapour of chloride of carbon.
8·75	151·24	189·3	55·4
13·32	183·32	228·6	68·7
18·84	229·15	285·0	88·3
22·81	267·53	333·3	105·8
26·44	307·41	382·2	123·7
30·64	358·81	445·4	147·3
33·78	400·47	497·6	168·0
36·53	444·34	549·5	187·3
42·35	542·21	668·8	236·0
48·43	661·95	814·0	296·2
<i>Mixture of 60 parts of sulphuret of carbon and 145 parts of chloride of carbon, C<sup>2</sup> Cl<sup>6</sup>.</i>			
11·98	143·82	216·1	64·4
21·09	206·71	311·0	98·2
35·12	349·23	522·3	177·6
41·50	436·52	649·5	228·5
The second day 18·70	188·39	283·6	87·8
The third day 13·16	149·97	227·0	68·2

The two mixtures of sulphuret and chloride of carbon consequently present the same fact that we have already seen to occur with mixtures of æther and sulphuret of carbon; the tensions of their vapours are weaker than those of the most volatile liquid, and intermediate between the tensions of the two liquids composing the mixture.

*Mixture of benzine and alcohol.*

Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of benzine.	Elastic force of the vapour of alcohol.
7·22	43·17	40·4	20·1
9·98	50·22	46·8	24·2
13·11	59·66	54·4	29·2
16·05	69·43	62·7	35·0
18·59	79·35	71·0	41·0

The mixture of benzine and alcohol consequently gave vapours of greater tension than those of the most volatile liquid, which was not observed with the preceding mixtures in the proportions of which they were composed.

The preceding experiments relate to the elastic force of the vapours furnished by mixtures of volatile liquids *in the static condition*, that is to say, when the space containing the liquid and the vapour is kept at a constant temperature. It became interesting to study them in the dynamic state, that is to say, to determine the temperatures presented by the vapour and the liquid when the mixture is boiled under different pressures. My experiments have hitherto been made only upon liquid mixtures of the first class, *i. e.* upon those composed of liquids which exert no sensible solvent action upon each other, and which consequently remain superposed.

Here we must distinguish two cases; that in which the more volatile liquid forms the upper stratum, and that in which it forms the lower. It is to the latter that I have principally directed my attention, as it is for this that we may hope to obtain the most constant results; the vapour of the more volatile liquid is then compelled to traverse the less volatile supernatant liquid, and is therefore placed in favourable conditions for becoming saturated with it. Nevertheless the phænomenon appears to be very complex, because it depends upon the greater or less vivacity of the ebullition. When the ebullition is weak, its temperature is found to be that at which the sum of the tensions of the two vapours is in equilibrium with the pressure opposed to the ebullition; but if the fire be rendered more active, the ebullition becomes very violent, the temperature rises, and finally attains the point at which the more volatile liquid would boil under the same pressure if it were alone. It is probable that passages are then formed in the upper stratum of liquid, and that the vapour passing without obstacle does not sensibly carry up vapour from the less volatile liquid. These irregularities are particularly remarkable when ebullition takes place under low pressures.

PART V.—*Researches undertaken to determine whether the solid or liquid state of bodies exerts any influence upon the elastic force of the vapours which they emit at the same temperature in vacuo.*

In my preceding researches upon the elastic force of aqueous vapour, I have endeavoured to ascertain whether the solidification which water undergoes when its temperature descends below  $0^{\circ}$  C. exerts any influence upon the tension of its vapour. For this purpose I made a great number of determinations of the elastic force of the vapour emitted by ice between  $-32^{\circ}$  and  $0^{\circ}$  C. I have ascertained that the curve constructed upon these experiments presented a perfect continuity with that given by the elastic forces of the vapours furnished by liquid water at temperatures above  $0^{\circ}$  C.

I have since made similar experiments upon two volatile liquids which solidified at a readily obtainable temperature, whilst still presenting a tension sufficiently strong to be measured with precision. These are hydrocarburet of bromine and benzine.

We may therefore admit, as proved by experiment, that *the molecular forces which determine the solidification of a substance exert no sensible influence upon the tension of its vapour in vacuo.*

But I attached particular interest to similar researches made upon monohydrated acetic acid. This acid is solid up to a temperature of  $+16^{\circ}$  C.; but when once liquefied, its congelation presents great difficulty. It may be cooled sometimes to  $-8^{\circ}$  or  $-10^{\circ}$  C. without solidifying, even when the flask containing it is briskly shaken. The solidification takes place immediately when the surface of the liquid is touched with a glass point, or when a crystal of solid acid is thrown into it. Monohydrated acetic acid consequently presented me with an instance of a substance, the tensions of the vapour emitted by which, both in a solid and liquid state, might be determined through a considerable extent of the thermometric scale.

The acetic acid employed in the two first series of experiments was taken from a mass of 1 kilogramme, which to all appearance solidified completely; but, for more certainty, only the last solid fragments which remained after the liquefaction of the greater part of it, were taken.

*First Series, January 12, 1844.*

Temperatures.	Elastic forces.
	mm
6.55	6.37
4.36	5.63
7.62	6.83
10.09	7.80
14.43	10.02
17.09	11.61
19.91	13.56

The acid remained liquid through all these temperatures. To make it crystallize, the water was taken out of the trough and the balloon was made to vibrate strongly, but without success. These vibrations at last fractured the mastic which established the communication with the manometer, and the experiment was obliged to be stopped.

*Second series, January 18.*

In this second series the balloon was surrounded by a freezing mixture to cause the congelation of the acid.

Temperatures.	Elastic forces.	Temperatures.	Elastic forces.		
	mm		mm		
Liquid acid.	1 <sup>o</sup> 36	5 <sup>o</sup> 23	Liquid acid. { -0 <sup>o</sup> 69	4 <sup>o</sup> 27	
	3 <sup>o</sup> 81	5 <sup>o</sup> 99	{ -2 <sup>o</sup> 40	3 <sup>o</sup> 90	
	9 <sup>o</sup> 09	7 <sup>o</sup> 81	{ -5 <sup>o</sup> 11	3 <sup>o</sup> 35	
	10 <sup>o</sup> 95	8 <sup>o</sup> 48	Solid acid. {	3 <sup>o</sup> 25	
	13 <sup>o</sup> 13	9 <sup>o</sup> 29		-7 <sup>o</sup> 55	3 <sup>o</sup> 56
	14 <sup>o</sup> 74	10 <sup>o</sup> 23		-5 <sup>o</sup> 83	3 <sup>o</sup> 93
	18 <sup>o</sup> 23	12 <sup>o</sup> 34		-4 <sup>o</sup> 24	4 <sup>o</sup> 26
	7 <sup>o</sup> 21	6 <sup>o</sup> 64		-2 <sup>o</sup> 56	4 <sup>o</sup> 71
	5 <sup>o</sup> 52	5 <sup>o</sup> 97		-0 <sup>o</sup> 82	4 <sup>o</sup> 89
	8 <sup>o</sup> 19	7 <sup>o</sup> 01		-0	
	10 <sup>o</sup> 91	8 <sup>o</sup> 12		The solidification of the acid only took place towards -7° C. At the moment when it took place, the tension suddenly increased considerably, which must be attributed to the instantaneous disengagement of the latent heat of fusion.	
	15 <sup>o</sup> 92	10 <sup>o</sup> 87			
	3 <sup>o</sup> 09	5 <sup>o</sup> 27			
	1 <sup>o</sup> 53	4 <sup>o</sup> 83			

If the curve of the elastic forces be constructed from these elements, it appears that the solid and liquid acid give two separate curves which probably unite at the point of fusion; the curve of the solid acid is constantly above that of the liquid acid, the tensions of which are always weaker at equal temperatures.

*Third Series.*

As it was feared that the acid employed in the two former series might have contained a little water, it was distilled over anhydrous phosphoric acid. But it was found that a certain quantity of acetone was then always formed. To separate this the substance was again distilled, only the last quarter of the product being collected; upon this the experiments were made.

Dates, 1844.	Temperatures.	Elastic forces.	Observations.
February 1.	7 <sup>o</sup> 40	mm 6 <sup>o</sup> 22	On the 1st of February the acid resisted a temperature of -5 <sup>o</sup> .38 C. without solidifying; whilst on the 2nd of February it solidified in melting ice.
	10 <sup>o</sup> 33	7 <sup>o</sup> 28	
	12 <sup>o</sup> 25	8 <sup>o</sup> 05	
	15 <sup>o</sup> 18	9 <sup>o</sup> 39	
	18 <sup>o</sup> 79	11 <sup>o</sup> 37	
	21 <sup>o</sup> 57	13 <sup>o</sup> 26	
	12 <sup>o</sup> 24	8 <sup>o</sup> 03	
February 2.	- 5 <sup>o</sup> 38	3 <sup>o</sup> 21	
	11 <sup>o</sup> 09	7 <sup>o</sup> 52	
	8 <sup>o</sup> 61	6 <sup>o</sup> 57	
	7 <sup>o</sup> 01	6 <sup>o</sup> 06	
	5 <sup>o</sup> 35	5 <sup>o</sup> 59	
	3 <sup>o</sup> 03	4 <sup>o</sup> 96	
	0 <sup>o</sup> 00	3 <sup>o</sup> 78	
	1 <sup>o</sup> 23	4 <sup>o</sup> 02	
3 <sup>o</sup> 51	4 <sup>o</sup> 59	The experiments could not be continued further, because one of the tubes of communication was broken accidentally.	
5 <sup>o</sup> 52	5 <sup>o</sup> 18		

The solid and liquid acid still present two curves, but the curve of the solid acid is constantly below that of the liquid acid.

*Fourth Series, February 3.*

The fourth series was made with acetic acid distilled a second time over anhydrous phosphoric acid. The same fact was observed as in the third series; that is to say, two separate curves, that of the liquid acid having the larger ordinates.

Temperatures.	Elastic forces.	Temperatures.	Elastic forces.		
	mm		mm		
Liquid acid.	7·04	5·61	Liquid acid.	1·32	3·96
	7·09	5·53		3·54	4·50
	7·17	5·57		5·77	5·14
	9·71	6·42	Solid acid.	0·00	3·23
	12·12	7·33		3·57	4·06
	14·87	8·59		6·92	5·08
	17·23	9·85		9·96	6·28
	22·37	13·15		11·49	6·97
	25·28	15·36		12·43	7·48
	19·84	11·47	13·14	7·86	
	19·84	11·44	Liquid acid.	14·33	8·42
	8·07	5·79			

I think that the anomalies presented by monohydrated acetic acid may be explained by the presence of very small quantities of foreign substances. The acid obtained by simple crystallization probably contained a very small quantity of water in excess, whilst the distilled acid contained a little acetone. As long as the acid is liquid, the foreign matter is disseminated through its whole mass, and does not exert any sensible influence upon the tension of the vapour. But this is by no means the case when the acid becomes solid; then the foreign matter separates, in combination with a smaller quantity of acetic acid, and its influence upon the tension of the vapour thus becomes much more remarkable.

### III. On the Heat of Vapours.

By Sir J. W. LUBBOCK, *Bart.*, F.R.S.\*

IN my Treatise on the Heat of Vapours, p. 2, I gave the following expression:

$$1 + \alpha\theta' = (1 + \alpha\theta) \frac{(p^{\frac{1-\gamma}{\gamma}} - E)}{(p'^{\frac{1-\gamma}{\gamma}} - E)}$$

\* Communicated by the Author.

In this expression  $\alpha$ ,  $\gamma$ , and  $E$  are constants,  $\theta$  and  $p$  are corresponding temperatures and pressures.

If  $\frac{1-\gamma}{\alpha} = \beta$ , and if  $\frac{1}{\alpha} + \theta = \tau$ ,  $\tau$  being the absolute temperature, the following equation is easily obtained :

$$p^\beta = E \left\{ 1 + \frac{(p'^\beta - E)\tau'}{E\tau} \right\};$$

if  $p'$  be taken equal to unity at the boiling-point of the fluid,  $\tau'$  is the absolute temperature at that boiling-point,

$$p^\beta = E \left\{ 1 + \frac{(1-E)\tau'}{E\tau} \right\};$$

and if

$$\frac{(1-E)\tau'}{E} = F,$$

$$p^\beta = E \left\{ 1 + \frac{F}{\tau} \right\},$$

$$\log p = \frac{1}{\beta} \log E + \frac{1}{\beta} \log \left\{ 1 + \frac{F}{\tau} \right\}.$$

This formula resembles that given by Mr. Rankine in the last Number of this Journal, inasmuch as it expresses the logarithm of the pressure in a series proceeding according to the negative powers of  $\tau$ .

In my treatise above referred to, the following numerical values are given for water :

$$E = 1.17602, \quad \beta = .0134;$$

but having gone over the work again, I find the slightly different values

$$E = 1.18028, \quad \beta = .01372;$$

$\tau = 448^\circ +$  number of degrees of Fahrenheit,  $\tau' = 660$ , and the pressure being reckoned in inches of mercury,

$$\log p = 6.72106 + [1.8626459] \log \left\{ 1 - \frac{[2.0035047]}{\tau} \right\},$$

or the pressure being expressed in millimetres of mercury, and the temperatures in Centigrade degrees,

$$\log p = 8.12766 + [1.8626459] \log \left\{ 1 - \frac{[1.7482322]}{\tau} \right\}.$$

From the same three observations of M. Regnault that Mr. Rankine used in his paper in the Edinburgh Philosophical Journal, vol. xlvii. p. 31, to determine the constants in his formula, I found



$$\beta = \cdot 02191,$$

$$E = 1 \cdot 29226 \text{ (the pressure reckoned in atmospheres),}$$

$$\log p = 7 \cdot 96295 + [1 \cdot 6593576] \log \left\{ 1 - \frac{[1 \cdot 9272914]}{\tau} \right\};$$

the pressure being expressed in millimetres of mercury, and the temperature in Centigrade degrees, the absolute zero being supposed to be  $274^{\circ}$  Cent. below the temperature of melting ice; the quantities in square brackets throughout being the logarithms of the numbers whose places they occupy. But although this expression represents all Regnault's observations very well, it deviates in places more than Mr. Rankine's formula.

These expressions appear to have nothing in common with those given by Mr. Waterston in the Philosophical Transactions, part 1, 1852.

IV. *On the Brine-springs of Worcestershire.* By A. BEAUCHAMP NORTHCOTE, Senior Assistant in the Royal College of Chemistry\*.

THE deposits of rock-salt which occur in the earth's crust, and the brine-springs which at various places rise to its surface, appear to be confined to no particular series of strata, but to be distributed somewhat indiscriminately. Thus, the inexhaustible mines of Wielitzka in Galicia, the deposits at the base of the Carpathian mountains, as well as those in many other places in central Europe, are found in the Tertiary: in some parts of Germany, again, salt is obtained from strata equivalent to our New Red Sandstone; in the Austrian Alps, an impure deposit mixed with clay and gypsum occurs in oolitic limestone; whilst in many countries, and even in England, salt-springs burst out of the carboniferous and older rocks †.

The great English deposits, however, exist in the upper members of the New Red Sandstone, in the red and green marls of that system; which has occasioned the application of the term "saliferous" to the whole series of rocks of which it is composed; a term which Sir R. Murchison, in his 'Siluria,' shows to be incorrect, inasmuch as the middle and upper members of that group are destitute both of rock-salt and saline waters. These salt-producing strata of the New Red Sandstone lie both in Cheshire and Worcestershire in a basin of Lias: at Northwich in Cheshire, masses of rock-salt exist, 60 feet in thickness, 1400 yards in breadth, and a mile and a half long; and from a bed still lower, salt has already been removed to a depth of 110 feet ‡:

\* Communicated by the Author.

† Murchison's Silurian System, vol. i. p. 31.

‡ Geological Transactions, Old Series, vol. i. p. 46.

in Worcestershire, the deposits, although large, do not appear to be on so gigantic a scale; the whole depth of the rocks constituting the matrix has been estimated at upwards of 600 feet; and from some observations published by Sir Charles Hastings some years since, on the sections disclosed by the sinkings at Stoke Prior, the layers of salt in this region vary from mere veins to deposits of more than 30 feet in thickness\*.

The brine-springs of Droitwich are stated by Sir C. Hastings to have been in use in the time of the Romans; but the existence of deposits of rock-salt does not appear to have been suspected until about thirty years ago, when a person acquainted with the features of the salt district of Cheshire, upon examining the geological peculiarities of the marls at Stoke Prior, a place nearly four miles to the N.E. of Droitwich, gave it as his opinion that rock-salt would be found there in sufficient quantity to make the working profitable. Shafts were accordingly sunk, and both salt and brine found: of the latter they speedily availed themselves, but I am not aware that the rock-salt has at any period been quarried, as in Cheshire; probably the extent of the deposits found on sinking the shafts did not warrant such an undertaking, or perhaps the abundance of the issuing salt-spring presented a more immediately available and less expensive source of the desired substance. At both places (Stoke and Droitwich) a shaft of considerable diameter is sunk and lined with an iron casing; in this the pumps which remove the brine and convey it to the reservoirs are fitted, and from the bottom of this upper tube a narrower pipe is carried down to a considerable depth, until, in fact, it reaches the subterranean brine chamber. The depth of the iron casing at Droitwich is 150 feet, that of the boring below 25 feet; and so abundant is the supply of brine, that if the pumps cease working, the shaft is speedily filled to within 9 feet of the surface. At Stoke the supply appears now to be equally inexhaustible, although I believe that in former years it failed; but the present proprietor has bored deeper than his predecessors, and has by means of a shaft 225 feet deep, and a boring of 348 feet more, succeeded in breaking through the drier and more superficial deposits, and has no doubt penetrated a larger and better supplied reservoir than had previously been opened.

The brine, thus pumped out of the pits as fast as it is supplied, is treated at both places in a precisely similar manner; it is conveyed by pipes directly from the pumps into large but shallow reservoirs, which act as feeders to the numerous pans in which the evaporation is carried on. These pans vary in length from 60 to 100 feet, and are about 20 feet broad and 18 inches

\* Murchison's *Silurian System*, vol. i. p. 31.

deep: the evaporation is conducted rather rapidly in the manufacture of ordinary salt, to prevent its deposition in crystals of too large a size; in the case of bay salt, the crystallization is allowed to be slower; in the large pans, however, such is the difference of temperature in different parts of the pan, from the furnace being placed at one end, that while the fine-grained salt is produced at the hottest end, bay salt is the result of the diminished temperature at the other. The salt as it crystallizes out sinks to the bottom, and is continually removed by the workmen, who place it in tall boxes having their bottoms pierced; in these the salt is allowed to drain and settle, and is shortly afterwards removed in the form of a consolidated block and placed in the drying-room. The fine salt, which is not sent to the market in blocks, is drained and dried in conical wicker baskets, from which it is easily shaken out as a rough powder, whilst the bay salt is merely removed from the pans by the shovel.

Formerly the brine reservoir beneath Droitwich was drawn upon in several places, each proprietor sinking a shaft for himself; but by degrees these shafts have all, with one exception, fallen into the hands of a single firm, that of Messrs. Clay and Newman, who finding the supply afforded by one shaft quite sufficient for their purpose, have discontinued working the others. This brine I have examined, and the results obtained will be found below: the brine yielded by the other shaft to which I have alluded I have not analysed, since as it is sunk within 50 feet of that belonging to Messrs. Clay and Newman, the probability is that the composition of the two waters is identical. What has been said of the sinkings at Droitwich applies in a great measure also to those at Stoke Prior; at this latter place three or four shafts are in action, all within a very short distance of each other, I therefore considered it useless to examine the water from more than one, thinking them to be not sufficiently far removed from one another to furnish brines having any material difference in their composition. The water which I took at this place for the purpose of analysis was that obtained at the works of Mr. J. Corbett. It will be seen by reference to the tables which follow, that two analyses of the Droitwich spring have been made: the water for the first analysis was kindly sent me by Sir C. Hastings in the January of the present year, that for the second was taken at the same time as the Stoke brine, in August last. This second examination was made chiefly for the more accurate comparison of the two waters, but it also serves to show that the composition of this saline spring is almost invariable, the analyses having been made at the most opposite periods of the year.

It was soon found by the qualitative examination of both

waters, that the only bodies existing in quantity sufficient to admit of their amount being determined, were soda, lime and magnesia, chlorine and sulphuric acid; free carbonic acid was indeed present, but it was impossible to take the brine from the pits, and as they issued from the delivery pipes in a state of foam, any attempt at the determination of this gaseous constituent was rendered useless: the existence of other substances was also ascertained, but their amount was so small as effectually to elude detection, unless a considerable quantity of water was operated upon. These waters contain 25 per cent. of solid matter; the evaporation of a comparatively small bulk of brine therefore yielded a residue which soon became cumbersome; nearly two gallons of the Droitwich brine, and rather more than one of that obtained at Stoke, were, however, evaporated. The plan of proceeding was to evaporate to a small bulk, and to test portions of the mother-liquor poured off from the deposited salts; first, for bromine and iodine, by evaporation to dryness and exhaustion with alcohol, repeating the process until a small saline residue was obtained; this was then examined in the usual manner by mixing thoroughly with starch-paste, and adding a single drop of nitric acid. Secondly and thirdly, for potassa and lithia, by the appropriate and well-known methods: the residue separated from the mother-liquor was exhausted with nitric acid, and the acid liquid examined; for iron, by converting the nitric into an acetic solution, and testing with ferrocyanide of potassium; for manganese, by Mr. W. Crum's method, the reaction of binoxide of lead; for phosphoric acid, by the action of molybdate of ammonia; and for alumina, by igniting the solid produced by evaporating the acid solution with nitrate of cobalt. The residue insoluble in nitric acid was further tested for fluorine and strontia by first submitting it to the process for producing hydrofluosilicic acid, and then examining the insoluble residue resulting from that process for strontia. Nitric acid and ammonia, arsenic, antimony, and tin were tested for in separate portions of each water.

As these brines closely resemble each other in their largely-occurring constituents, so do they also in the traces of substances which they contain. Careful examination of a portion of the mother-liquor, equivalent to half a gallon of the water, has shown in the case of each the presence of a minute quantity of bromine, but iodine appears to be totally absent: another portion of the mother-liquor, representing about a quart of brine, has in both instances yielded a distinct trace of potassa, whilst no evidence of the presence of lithia has been obtained. In the insoluble residues a distinct trace of iron has been found; manganese, however, as might have been expected when so small a quantity

of iron was present, was not to be recognized. Unmistakeable evidence of the presence of phosphoric acid was obtained in both waters, upon the addition of a small quantity of the nitric acid solution to a quite colourless and nearly dry mixture of nitric acid and molybdate of ammonia. The search for fluorine and strontia has been unsuccessful; nor have any traces of nitric acid or ammonia, antimony, arsenic, or tin been discovered; but on passing sulphuretted hydrogen through portions of the brines acidified with hydrochloric acid, a slight but decided brownish tinge was observed in both cases, such as is produced by the same reagent in a solution containing about  $\frac{1}{800,000}$  dth of its weight of lead: this I would suggest might be due to the presence of a minute trace of chloride of silver dissolved in the chloride of sodium, since that metal has been found in rock-salt in small quantities\*; so exceedingly trifling is its amount, however, that my attempts to isolate it have been fruitless. The following are the preliminary observations made upon the waters, and the direct results of their analysis:—

	Droitwich brine.		Stoke brine.
	January 1854.	August 1854.	August 1854.
Specific gravity	1·1893	1·1792	1·2044
Temp. of air . . .	...	76°·5 F.	75° F.
Temp. of brine . . .	...	54°·5 F.	55° F.
Reaction . . . . .	...	slightly alkaline. slightly alkaline.	

Table of direct results of analysis calculated to 100 parts.

	Droitwich brine.						Stoke brine.		
	January.			August.			August.		
	I.	II.	Mean.	I.	II.	Mean.	I.	II.	Mean.
Potassa .....						trace			trace
Soda.....	12·5798	12·5929	12·5864	12·1501	12·1217	12·1359	13·7804	13·7754	13·7779
Lime .....	·1508	·1530	·1519	·1581	·1612	·1596	·1102	·1049	·1075
Magnesia .....	·0196	·0204	·0200	·0167	·0159	·0163	·0187	·0143	·0165
Sesquioxide of iron } .....			trace			trace			trace
Chlorine .....	14·1344	14·1360	14·1352	13·6167	13·6329	13·6248	15·4479	15·4916	15·4697
Bromine .....			trace			trace			trace
Sulphuric acid. . .	·4256	·4269	·4262	·4886	·4876	·4881	·4896	·4880	·4888
Phosphoric acid } .....						trace			trace
Silicic acid ... } .....			trace			trace			trace
Residue on direct evaporation... } .....	24·2272	24·1856	24·2064	23·4205	23·4205	23·4205	26·4632	26·4866	26·4749

\* *Annales de Chimie et de Physique*, 3 sér. vol. xxviii. p. 129.

Since gypsum is an invariable constituent of rock-salt strata, it is reasonable to suppose that it dissolves as such in the brine; and as its solubility is increased by the presence of sulphate of soda and chloride of sodium\*, I have ventured in the arrangement of results which follows, to calculate all the lime as sulphate, although were it not for the presence of those salts, it would be a larger per-centage than the water would dissolve.

Table of constituents calculated in 100 parts.

	Droitwich brine.		Stoke brine.
	January.	August.	August.
Chloride of sodium .....	23·2932	22·4521	25·4923
Bromide of sodium .....	trace	trace	trace
Sulphate of potassa .....	.....	trace	trace
Sulphate of soda .....	·3712	·3905	·5948
Sulphate of lime .....	·3690	·3878	·2612
Carbonate of soda .....	·1382	·1157	·0161
Carbonate of magnesia .....	·0420	·0343	·0347
Phosphate of lime .....	.....	trace	trace
Phosphate of sesquioxide of iron .	.....	trace	trace
Silica .....	trace	trace	trace
	24·2136	23·3804	26·3991
Solid residue by direct evapo- ration .....	24·2064	23·4205	26·4749

In the table which follows next, I have given some results obtained by Dr. Daubeny in an examination which he made of the Droitwich brine in 1830†. In his paper he states the constituents in a pint, and in that quantity was unable to detect the presence of bromine or iodine; there is no doubt, however, as he has observed, that if far larger quantities were operated upon, both these elements would be found; of the former of them, indeed, I have shown a slight but still distinctly recognizable trace to exist in about half a gallon of the brine; and there is every probability that iodine also would be found by working on a larger scale. I have also introduced the results of an analysis made by Mr. Horner in the year 1812‡; at that time there were no fewer than five pits in Droitwich from which brine was drawn. The specific gravity of the different specimens he states to vary from 1·20611 to 1·17471, and the amount of salts in a pint to range between 2289·75 and 1922·97 grains; the results calculated in his paper to the pint, I have (as also in the case of Dr. Daubeny's experiments) recalculated to the im-

\* Gmelin's Handbook, vol. iii. p. 202.

† Transactions of the Royal Society for 1830.

‡ Geological Transactions, Old Series, vol. ii. p. 94.

perial gallon. It will be seen that the analysis which he made coincides more nearly with mine of the Stoke brine; but it must be remembered that at that time no shafts had been sunk at Stoke, a circumstance which might materially influence the neighbouring brine-springs. Mr. Horner also had the choice of five brines, of which he naturally selected the strongest. Considering, however, the lapse of time (more than forty years) between the two analyses, and the change which the methods of manipulation have undergone in the interim, they seem to me to bear sufficient resemblance to each other to prove that no material alteration has taken place in the composition of the brine; and among many similarities, two are worthy of remark. The temperature which he gives as that of the brine is  $55^{\circ}$  F., whilst that which I observed, it will be remembered, was  $54^{\circ}5$  F.; and he also states in his qualitative examination, that by the addition of ammonia to the water, a slight turbidity was occasioned, due doubtless to the presence of a small quantity of the phosphates of lime or iron (alumina being absent, and silica virtually so). This is a circumstance which I also observed, and subsequently found enough phosphoric acid to account for its occurrence.

Table of constituents calculated to the gallon.

	Droitwich brine.				Stoke brine.
	Horner.	Daubeny.	January 1854.	August 1854.	August 1854.
Chloride of sodium .....	21585.5	16910	19392.570	18532.886	21492.048
Chloride of magnesium .....	16.8				
Sulphate of soda .....	407.0	402.5	309.097	322.334	501.539
Sulphate of lime .....	363.7	.....	307.282	320.113	220.246
Carbonate of soda .....	.....	.....	115.123	95.519	13.573
Carbonate of magnesia .....	.....	.....	33.501	28.353	29.263
Solid contents .....	.....	17460			
Total .....	22373.0	.....	20157.573	19299.205	22256.669

In that excellent paper of Dr. Holland's on the deposits of rock-salt in Cheshire, to which I have before referred\*, he states that the beds in that county have their longitudinal extension in a line drawn from the N.E. to the S.W., and that their greatest thickness is at the N.E. extremity, from whence they thin out as they approach the S.W. From what is known of the deposits in Worcestershire, it would appear that they also follow somewhat of the same method of arrangement. Stoke and Droitwich stand to each other in the relation of N.E. to S.W.; and from the fact of brine being found at both points, and that of very similar composition, there can be little doubt of a continuity of

\* Geological Transactions, Old Series, vol. i.

the strata from which it is produced between the two places ; at the same time, the possibility of the extension of the rock-salt is, at least on one side, confined to that direction, since at Forest Hill, near Hanbury\*, a place about two miles south of a line joining Stoke and Droitwich, outcroppings of Lias are found, which, as they indicate the termination of the New Red Sandstone, show also of necessity that of the rock-salt. Whether as great a similarity exists between these two great English deposits in their shape as in their direction, cannot so readily be inferred, since it is of course doubtful whether at either of the explored parts the lowest limit of the salt has hitherto been reached ; yet since the level of the ground at Stoke is from 300 to 400 feet higher than that at Droitwich, and the productive salt deposits there do not appear to begin until the borings have been sunk to about the level of the former place, it would seem that in the Worcestershire salt beds no such thinning out in a south-westerly direction takes place.

These brines doubtless spring from large subterranean reservoirs, produced probably in the first instance by the solvent action exerted on the rock-salt by spring- or rain-water. That this is so at Droitwich is clear from what Mr. Horner states as having occurred on the first sinking of a shaft at that place ; he says, that after penetrating for 150 feet, the borer suddenly descended 22 inches † ; the roof of the brine chamber had evidently been broken through, and the water instantly burst into the tube and gushed from the aperture with great force. This account is confirmed by observations of the same kind made at Stoke Prior ; but owing to the greater elevation of the land, the brine when "tapped" rose only to within 90 feet of the surface, but its ascension in this case also was very rapid. The idea of extensive cavities filled with brine existing beneath these places, also receives additional support from the distinct sinking of the ground around the salt-works at Droitwich ; at that place, *i. e.* at which the subterranean reservoir would be nearer the surface, and where consequently the intervening strata would present a lesser obstacle to the filling up by a more solid material of the space rendered vacuous by the removal of the brine. We can readily conceive the incessant action of the pumps to gain in the course of years upon the supply by which the chamber is replenished, and an occurrence of this sort is exactly that which might have been predicted.

It is thought by persons on the spot, that a stream of brine passes from Stoke to Droitwich, and is at once the source from which the supply at both places is derived ; and although at first

\* Murchison's *Silurian System*, vol. i. p. 32.

† *Geological Transactions*, Old Series, vol. ii.



this might appear most probable, when we remember that (taking into consideration the difference in the level of the localities) the same depth has to be reached before the brine can be obtained, yet I cannot but think that one circumstance tends to show that this view is not the correct one; for if such were the case, the brine should in each instance rise to the same level in the different shafts when left undisturbed by the action of the pumps. Now under these conditions, the height to which it ascends at Droitwich is about 166 feet; whilst at Stoke, starting from very nearly the same level, the column of water rises to a height of 483 feet; this of itself is sufficient to show that no direct communication can take place between these two springs without supposing a violation of one of the first principles of hydrostatics. It might then be thought that a reference to the analyses before given might decide this question; but on comparing the constituents of the two waters, we can easily see that, taken alone, they prove nothing, since the differences are such as might readily be found in the same water when passing over slightly differing deposits. If the stream ran from Stoke to Droitwich, we might suppose that in its course it met with strata containing more sulphate of lime than it had as yet encountered, and at the same time was somewhat diluted with streams poorer in chloride of sodium; but I rather incline to think, that even from the composition of the brines, there is no reason to suppose that such a direct communication exists; their great similarity may easily be accounted for by supposing an equal similarity in the composition of the entire strata of rock-salt from which they are derived. Numerous freshwater streams occur in the higher parts of these deposits; in fact, no less than three are encountered, and have to be guarded against in the Stoke shafts, and these doubtless fed by rain-water permeate the strata in various directions, and dissolve the rock-salt with which they come in contact. Mr. Corbett informs me that the salt from which his brine is drawn is perfectly transparent, which clearly accounts for the fact of the Stoke brine containing less sulphate of lime, while the somewhat diminished strength of the Droitwich brine is probably due to the less perfect exclusion of the freshwater springs of the upper strata from the shaft which reaches the reservoir. Their striking similarity in other respects, the identity of their temperature, and of the traces of substances which they contain, point to an identity also in the composition of the deposit from which they are formed, of the solvent which renders them available, and of the manner in which they are produced.

I cannot conclude without expressing my thanks to Sir Charles Hastings for the assistance which he has afforded me in obtaining the Droitwich water, and also to Messrs. Clay and Corbett for the valuable information which they have given me on the subject.

V. *Note on the Possible Density of the Luminiferous Medium and on the Mechanical Value of a Cubic Mile of Sunlight.*  
By PROFESSOR WILLIAM THOMSON\*.

THAT there must be a medium forming a continuous material communication throughout space to the remotest visible body is a fundamental assumption in the undulatory theory of light. Whether or not this medium is (as appears to me most probable) a continuation of our own atmosphere, its existence is a fact that cannot be questioned, when the overwhelming evidence in favour of the undulatory theory is considered; and the investigation of its properties in every possible way becomes an object of the greatest interest. A first question would naturally occur, What is the absolute density of the luminiferous æther in any part of space? I am not aware of any attempt having hitherto been made to answer this question, and the present state of science does not in fact afford sufficient data. It has, however, occurred to me that we may assign an inferior limit to the density of the luminiferous medium in interplanetary space by considering the mechanical value of sunlight as deduced in preceding communications to the Royal Society from Pouillet's data on solar radiation, and Joule's mechanical equivalent of the thermal unit. Thus the value of solar radiation per second per square foot at the earth's distance from the sun, estimated at  $\cdot 06$  of a thermal unit Centigrade, or 83 foot-pounds, is the same as the mechanical value of sunlight in the luminiferous medium through a space of as many cubic feet as the number of linear feet of propagation of light per second. Hence the mechanical value of the whole energy, actual and potential, of the disturbance kept up in the space of a cubic foot at the earth's distance from the sun†, is  $\frac{83}{192000 \times 5280}$ , or  $\frac{\cdot 819}{10^7}$  of a foot-pound. The mechanical value of a cubic mile of sunlight is consequently 12050 foot-pounds, equivalent to the work of one-horse power for a third of a minute. This result may give some idea of the actual amount of mechanical energy of the luminiferous motions and forces within our own atmosphere. Merely to commence the illumination of three cubic miles, requires an

\* From the Transactions of the Royal Society of Edinburgh, vol. xxi. part 1; read May 1, 1854.

† The mechanical value of sunlight in any space near the sun's surface must be greater than in an equal space at the earth's distance, in the ratio of the square of the earth's distance to the square of the sun's radius, that is, in the ratio of 46,400 to 1 nearly. The mechanical value of a cubic foot of sunlight near the sun must, therefore, be about  $\cdot 0038$  of a foot-pound, and that of a cubic mile 560,000,000 foot-pounds.

amount of work equal to that of a horse-power for a minute ; the same amount of energy exists in that space as long as light continues to traverse it ; and, if the source of light be suddenly stopped, must be emitted from it before the illumination ceases\*. The matter which possesses this energy is the luminiferous medium. If, then, we knew the velocities of the vibratory motions, we might ascertain the density of the luminiferous medium ; or, conversely, if we knew the density of the medium, we might determine the average velocity of the moving particles. Without any such definite knowledge, we may assign a superior limit to the velocities, and deduce an inferior limit to the quantity of matter, by considering the nature of the motions which constitute waves of light. For it appears certain that the amplitudes of the vibrations constituting radiant heat and light must be but small fractions of the wave lengths, and that the greatest velocities of the vibrating particles must be very small in comparison with the velocity of propagation of the waves. Let us consider, for instance, plane-polarized light, and let the greatest velocity of vibration be denoted by  $v$  ; the distance to which a particle vibrates on each side of its position of equilibrium, by  $A$  ; and the wave length, by  $\lambda$ . Then if  $V$  denote the velocity of propagation of light or radiant heat, we have

$$\frac{v}{V} = 2\pi \frac{A}{\lambda} ;$$

and therefore if  $A$  be a small fraction of  $\lambda$ ,  $v$  must also be a small fraction ( $2\pi$  times as great) of  $V$ . The same relation holds for circularly-polarized light, since in the time during which a particle revolves once round in a circle of radius  $A$ , the wave has been propagated over a space equal to  $\lambda$ . Now the whole mechanical value of homogeneous plane-polarized light in any infinitely small space containing only particles sensibly in the same phase of vibration, which consists entirely of potential energy at the instants when the particles are at rest at the extremities of their excursions, partly of potential and partly of actual energy when they are moving to or from their positions of equilibrium, and wholly of actual energy when they are passing through these positions, is of constant amount, and must therefore be at every instant equal to half the mass multiplied by the square of the velocity the particles have in the last-mentioned case. But the velocity of any particle passing through its position of equilibrium is the greatest velocity of vibration, which has been denoted by  $v$  ; and, therefore, if  $\rho$  denote the

\* Similarly we find 15000 horse-power for a minute as the amount of work required to generate the energy existing in a cubic mile of light near the sun.

quantity of vibrating matter contained in a certain space, a space of unit volume for instance, the whole mechanical value of all the energy, both actual and potential, of the disturbance within that space at any time is  $\frac{1}{2}\rho v^2$ . The mechanical energy of circularly-polarized light at every instance is (as has been pointed out to me by Professor Stokes) half actual energy of the revolving particles and half potential energy of the distortion kept up in the luminiferous medium; and, therefore,  $v$  being now taken to denote the constant velocity of motion of each particle, double the preceding expression gives the mechanical value of the whole disturbance in a unit of volume in the present case. Hence it is clear, that for any elliptically-polarized light the mechanical value of the disturbance in a unit of volume will be between  $\frac{1}{2}\rho v^2$  and  $\rho v^2$ , if  $v$  still denote the greatest velocity of the vibrating particles. The mechanical value of the disturbance kept up by a number of coexisting series of waves of different periods, polarized in the same plane, is the sum of the mechanical values due to each homogeneous series separately, and the greatest velocity that can possibly be acquired by any vibrating particle is the sum of the separate velocities due to the different series. Exactly the same remark applies to coexistent series of circularly-polarized waves of different periods. Hence the mechanical value is certainly less than *half* the mass multiplied into the square of the greatest velocity acquired by a particle, when the disturbance consists in the superposition of different series of plane-polarized waves; and we may conclude, for every kind of radiation of light or heat except a series of homogeneous circularly-polarized waves, that *the mechanical value of the disturbance kept up in any space is less than the product of the mass into the square of the greatest velocity acquired by a vibrating particle in the varying phases of its motion.* How much less in such a complex radiation as that of sunlight and heat we cannot tell, because we do not know how much the velocity of a particle may mount up, perhaps even to a considerable value in comparison with the velocity of propagation, at some instant by the superposition of different motions chancing to agree; but we may be sure that the product of the mass into the square of an ordinary maximum velocity, or of the mean of a great many successive maximum velocities of a vibrating particle, cannot exceed in any great ratio the true mechanical value of the disturbance. Recurring, however, to the definite expression for the mechanical value of the disturbance in the case of homogeneous circularly-polarized light, the only case in which the velocities of all particles are constant and the same, we may define the mean velocity of vibration in any case as such a velocity that the product of its square into the mass of the vibrating particles is equal to the whole

mechanical value, in actual and potential energy, of the disturbance in a certain space traversed by it; and from all we know of the mechanical theory of undulations, it seems certain that this velocity must be a very small fraction of the velocity of propagation in the most intense light or radiant heat which is propagated according to known laws. Denoting this velocity for the case of sunlight at the earth's distance from the sun by  $v$ , and calling  $W$  the mass in pounds of any volume of the luminiferous æther, we have for the mechanical value of the disturbance in the same space,

$$\frac{W}{g} v^2,$$

where  $g$  is the number 32.2, measuring in absolute units of force, the force of gravity on a pound. Now we found above, from observation,  $\frac{83}{V}$  for the mechanical value, in foot-pounds, of a cubic foot of sunlight; and therefore the mass, in pounds, of a cubic foot of the æther, must be given by the equation,

$$W = \frac{32.2 \times 83}{v^2 V}.$$

If we assume  $v = \frac{1}{n} V$ , this becomes

$$W = \frac{32.2 \times 83}{V^3} \times n^2 = \frac{32.2 \times 83}{(192000 \times 5280)^3} \times n^2 = \frac{n^2}{3899 \times 10^{20}};$$

and for the mass, in pounds, of a cubic mile we have

$$\frac{32.2 \times 83}{(192000)^3} \times n^2 = \frac{n^2}{2649 \times 10^9}.$$

It is quite impossible to fix a definite limit to the ratio which  $v$  may bear to  $V$ ; but it appears improbable that it could be more, for instance, than  $\frac{1}{50}$ , for any kind of light following the observed laws. We may conclude that probably a cubic foot of the luminiferous medium in the space traversed by the earth contains not

less than  $\frac{1}{1560 \times 10^{17}}$  of a pound of matter, and a cubic mile not

less than  $\frac{1}{1060 \times 10^6}$ .

If the mean velocity of the vibrations of light within a spherical surface concentric with the sun and passing through the earth were equal to the earth's velocity—a very tolerable supposition—since this is  $\frac{1}{10170}$  of the velocity of light, the whole mass of the luminiferous medium within that space would be  $\frac{1}{300000}$  of the earth's mass, since the mechanical value of the light within it, being as much as the sun radiates in about

eight minutes, is about  $\frac{1}{15000}$  of the mechanical value of the earth's motion. As the mean velocity of the vibrations might be many times greater than has been supposed in this case, the mass of the medium might be considerably less than this; but we may be sure it is not incomparably less, not 100,000 times as small for instance. On the other hand, it is worth remarking that the preceding estimate shows that what we know of the mechanical value of light renders it in no way probable that the masses of luminiferous medium in interplanetary spaces, or all round the sun in volumes of which the linear dimensions are comparable with the dimensions of the planets' orbits, are otherwise than excessively small in comparison with the masses of the planets.

But it is also worth observing that the luminiferous medium is enormously denser than the continuation of the terrestrial atmosphere would be in interplanetary space, if rarified according to Boyle's law always, and if the earth were at rest in a space of constant temperature with an atmosphere of the actual density at its surface\*. Thus the mass of air in a cubic foot of distant space several times the earth's radius off, on this hypothesis, would be  $\frac{1 \text{ lb.}}{442 \times 10^{345}}$ ; while there cannot, according to

the preceding estimate, be in reality less than  $\frac{1 \text{ lb.}}{1560 \times 10^{17}}$ , which is  $9 \times 10^{327}$  times as much, of matter in every cubic foot of space traversed by the earth.

VI. *On the Composition of the Felspars of the Granite of the Dublin and Wicklow Mountains.* By the Rev. JOSEPH A. GALBRAITH, *Erasmus Smith's Professor of Natural and Experimental Philosophy in the University of Dublin.*

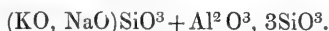
*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

**I**N the Mourne Mountains of the county Down we have a well-known case of the occurrence of albite as a constituent

\* "Newton has calculated (Princ. iii. p. 512) that a globe of ordinary density at the earth's surface, of 1 inch in diameter, if reduced to the density due to the altitude above the surface of one radius of the earth, would occupy a sphere exceeding in radius the orbit of Saturn."—(Herschel's Astronomy, Note on § 559.) It would (on the hypothesis stated in the text) we may now say occupy a sphere exceeding in radius millions of millions of times the distances of any stars of which the parallaxes have been determined. A pound of the medium, in the space traversed by the earth, cannot occupy more than the bulk of a cube 1000 miles in side. The earth itself, in moving through it, cannot displace less than 250 pounds of matter.

of granite rock, that is to say, of a mineral occurring in the sixth system, whose rational formula is



I mention this because there has existed a good deal of confusion as to the use of the terms orthose and albite; the former having been commonly appropriated to minerals of the felspathic family in which the alkali potash exists in excess, the latter to those in which soda preponderates. The true distinction unquestionably rests on crystallographic form, as instances are to be found of minerals included under the formula already given, occurring in the fifth system, in which soda preponderates, and to which therefore we should give the name orthose. And *vice versá* of specimens containing potash in excess occurring in the sixth system, and which should therefore be called albite.

It was very commonly believed until lately that this case of the Mourne Mountains was a singular instance in Ireland. In the Proceedings of the Royal Irish Academy, vol. v. p. 351, I find that Sir Robert Kane announced as a fact, that the felspar of the Dublin and Wicklow granites "was almost exclusively an albitic or soda felspar, containing only in some cases a small quantity of replacing potash." This conclusion he derived from the analyses of some surface waters taken from the Three Rock Mountain in the neighbourhood of Dublin, in which he found a great preponderance of soda, but at the same time stated that "the fact was verified by a great number of analyses of specimens of granites taken from various parts of the great mass which extends from Dublin into the county Wicklow. In all the analyses made, which included both ordinary granites and elvan or granite porphyries, both potash and soda were found present; and the latter almost always so preponderant, as to lead to the conclusion that the potash should in most cases be considered to belong to the mica which the granite contained; and the felspar was almost exclusively an albitic or soda felspar, containing only in some cases a small quantity of replacing potash."

These analyses, which he stated had been made in the Museum of Irish Industry, he did not present to the Academy, nor have they ever, as far as I know, come before the public.

The statement of a fact so interesting and at the same time so novel excited much interest; and as it was suggested at the time by a very eminent authority, Dr. Apjohn, Professor of Chemistry and Mineralogy in the University of Dublin, that a direct investigation of the mineral in question would be of essential service in determining so important a geological fact, I undertook the task of making a systematic investigation of the felspars of the range of mountains which runs in a south-westerly direction from Dublin to within a few miles of New Ross. As a first

contribution I beg leave to send you the results of the analyses of seven specimens taken from different localities in these mountains, pretty evenly distributed over that part of the range which extends from Dalkey to the flanks of Lug-na-quilla, a mountain about thirty-six miles from Dublin. These analyses will, I think, not fail to engage the attention of such of your readers as feel an interest in chemico-geological investigations,

*Quarries of Dalkey, County Dublin.*

	Per cent.	Atomic quotients.	Proportionals.
SiO <sup>3</sup> . . . .	64.00	1.412	4.01
Al <sup>2</sup> O <sup>3</sup> . . . .	18.11	0.352	1.00
MgO . . . .	0.57	0.028	} 0.396
CaO . . . .	trace	0.000	
KO . . . .	12.73	0.271	
NaO . . . .	3.00	0.097	
Loss by ignition .	0.55		1.13
	<u>98.96</u>		

Specific gravity = 2.540.

*Three Rock Mountain, County Dublin.*

	Per cent.	Atomic quotients.	Proportionals.
SiO <sup>3</sup> . . . .	65.40	1.444	4.19
Al <sup>2</sup> O <sup>3</sup> . . . .	17.71	0.344	1.00
MgO . . . .	1.77	0.088	} 0.420
CaO . . . .	trace	0.000	
KO . . . .	10.68	0.227	
NaO . . . .	3.26	0.105	
Loss by ignition	0.69		1.22
	<u>99.51</u>		

Specific gravity = 2.562.

*Lough Bray, County Dublin.*

	Per cent.	Atomic quotients.	Proportionals.
SiO <sup>3</sup> . . . .	65.44	1.444	4.04
Al <sup>2</sup> O <sup>3</sup> . . . .	18.36	0.357	1.00
MgO . . . .	0.00	0.000	} 0.388
CaO . . . .	0.80	0.038	
KO . . . .	12.34	0.262	
NaO . . . .	2.73	0.088	
Loss by ignition	0.52		1.08
	<u>100.19</u>		

Specific gravity = 2.554.



*Lough Dan, County Wicklow.*

	Per cent.	Atomic quotients.	Proportionals.
SiO <sup>3</sup> . . . .	65.05	1.436	4.16
Al <sup>2</sup> O <sup>3</sup> . . . .	17.72	0.345	1.00
MgO . . . .	trace	0.000	} 0.381 1.10
CaO . . . .	0.23	0.008	
KO . . . .	13.42	0.285	
NaO . . . .	2.75	0.088	
Loss by ignition	0.36		
	<u>99.53</u>		

Specific gravity = 2.559.

*Glenmacanass, County Wicklow.*

	Per cent.	Atomic quotients.	Proportionals.
SiO <sup>3</sup> . . . .	64.19	1.417	3.96
Al <sup>2</sup> O <sup>3</sup> . . . .	18.39	0.358	1.00
MgO . . . .	0.34	0.017	} 0.379 1.05
CaO . . . .	0.70	0.025	
KO . . . .	11.39	0.242	
NaO . . . .	2.95	0.095	
Loss by ignition	0.58		
	<u>98.54</u>		

Specific gravity = 2.553.

*Glendalough, County Wicklow.*

	Per cent.	Atomic quotients.	Proportionals.
SiO <sup>3</sup> . . . .	63.60	1.404	3.83
Al <sup>2</sup> O <sup>3</sup> . . . .	18.84	0.366	1.00
MgO . . . .	0.40	0.020	} 0.385 1.05
CaO . . . .	trace	0.000	
KO . . . .	14.33	0.305	
NaO . . . .	1.92	0.060	
Loss by ignition	0.60		
	<u>99.69</u>		

Specific gravity = 2.453.

*Glenmalur, County Dublin.*

	Per cent.	Atomic quotients.	Proportionals.
SiO <sup>3</sup> . . . .	64.48	1.423	3.84
Al <sup>2</sup> O <sup>3</sup> . . . .	19.04	0.370	1.00
MgO . . . .	1.02	0.051	} 0.364 0.98
CaO . . . .	trace	0.000	
KO . . . .	10.74	0.228	
NaO . . . .	2.64	0.085	
Loss by ignition	0.78		
	<u>98.70</u>		

Specific gravity = 2.560.

I feel a great desire to see the rock analyses on which Sir Robert Kane has founded so important a statement, and sincerely hope that he will before long make them public, as I feel utterly at a loss to reconcile the statement with the results I have arrived at. The granite rock in question consists of quartz, mica and felspar: the quartz contributes no alkali, the mica is well known to contain potash in excess, the felspar I have shown contains potash in excess over soda in the ratio of 9:2; how, then, can a rock which is a mixture of the three be said to contain both potash and soda? "but the latter almost always so preponderant, as to lead to the conclusion that the potash should in most cases be considered to belong to the mica which the granite contained; and that the felspar was almost exclusively an albitic or soda felspar, containing only in some instances a small quantity of replacing potash."

I remain, Gentlemen,

Your obedient Servant,

Trinity College, Dublin,  
December 12, 1854.

JOSEPH A. GALBRAITH.

VII. *On the Elastic Forces of Vapours of Mixtures of two Liquids.* By Professor MAGNUS\*.

**I**N the communication of his interesting researches on the elastic forces of the vapours of different liquids, M. Regnault states, "It may be said that the law of Dalton has been accepted on sufferance, inasmuch as it seemed to be a natural consequence of the notions formed regarding the constitution of gaseous fluids, and which were developed by Laplace, Poisson, and other mathematicians. Physicists have not deemed it necessary to submit the law to an experimental examination."

This statement contains a complaint which is not well founded. In a memoir published in Poggendorff's *Annalen*, vol. xxxviii. p. 181, in the year 1836, "On the boiling of Mixtures of two Liquids, and the bumping of such mixtures," I have shown that the law of Dalton is not applicable to the vapours of mixed liquids; that is to say, of liquids which mutually dissolve each other by their mixture. It is there stated,—"If a liquid, æther for example, be brought into the vacuum of a barometer tube, and if, after the maximum tension of the vapour developed has been observed, another liquid, for instance alcohol, be introduced, whose vapour at the existing temperature possesses a smaller tension than that of æther; then the tension of the vapours of both liquids is less than that of the æther vapour alone, and is ever feebler the more alcohol is added, until finally, when a

\* From Poggendorff's *Annalen*; communicated by the Author.

large quantity of the latter, liquid in comparison with the æther, is present, the tension almost corresponds with that of the vapour of alcohol alone at the existing temperature. The same is the case if, instead of alcohol, oil of turpentine, or instead of æther, bisulphuret of carbon be used; or if alcohol be first introduced into the barometer tube and water added afterwards."

From these experiments it follows, that the elastic force of the vapours of a mixture of two liquids which mutually dissolve each other is smaller than the sum of the elastic forces of the component vapours at the same temperature; and that the elastic force of the vapours of a mixture is dependent on the proportions in which the liquids which produce them are present.

As regards the department of the vapours of a mixture of two liquids which do not mutually dissolve each other, but form separate strata, I also made experiments at the period referred to, and found, in their case, the elastic force of the vapours of the mixture to be equal to the sum of the elastic forces of the vapours of both liquids taken separately at the same temperature. That with such mixtures, therefore, the law of Dalton finds full application.

In regard to the boiling of such mixtures, I found that two cases are to be distinguished from each other, according as the more volatile liquid constitutes the upper or the under layer. When the more volatile liquid was uppermost, boiling took place within the latter alone, and at the temperature at which the liquid would have boiled under the existing pressure, had it alone been contained in the vessel. If, on the contrary, the more volatile liquid were below, the temperature of the boiling mixture was always somewhat higher than that at which the more volatile liquid would have boiled if alone. The vapours, on the contrary, which developed themselves from this mixture always possessed a temperature, which was not only lower than that of the liquid mixture, but also lower than that at which the more volatile liquid would have boiled under the existing pressure.

In a mixture, for example, of fresh rectified oil of turpentine and water, which boiled under the pressure of 749·6 millims., the thermometer in the liquid showed a temperature of 102° C., and in the vapour 94°·5 C. In like manner, in a mixture of bisulphuret of carbon and water, which boiled under a pressure of 752·2 millims., the thermometer in the liquid stood at 47° C., and that in the vapour at 43°·5 C.

This difference in the temperatures of the vapour and the liquid is not mentioned by M. Regnault. It is, however, an immediate consequence of the law of Dalton. For the lower liquid, which is the most volatile, must, in order to boil, possess a temperature

at which the elastic force of its vapour is capable of overcoming the pressure, not only of the air, but also that of the liquid above. This temperature is therefore a little higher than that which it would be if the liquid boiled under the pressure of the air alone. Further, as the bubbles of vapour from the lower liquid pass through the upper one, the temperature of the latter is nearly the same. But while the bubbles from below pass through the upper liquid, they carry vapour from the latter along with them, so that the escaping bubbles are formed of a mixture of the vapour of both liquids. Without doubt these bubbles at the moment of their escape from the liquid have the same temperature as the latter. But as the elastic force of the vapour of the more volatile liquid would alone be able to overcome the atmospheric pressure, the elastic force of the vapours of both liquids together is greater than the pressure of the atmosphere; hence these vapours expand until the sum of their tensions is just sufficient to overcome the pressure of the atmosphere, and by expansion are brought to the lower temperature observed.

With the exception of this latter phænomenon, namely, that the temperature of the vapour escaping from a mixture of two liquids which do not mutually act upon each other, is less than that of the liquid mixture, which phænomenon is not mentioned by M. Regnault, my results are corroborated by him. They were, however, made public by me eighteen years ago, and since that time it has been known to what extent the law of Dalton is applicable, and the deportment of the vapours of mixtures of two liquids has been understood.

VIII. *On some Extensions of Quaternions.* By Sir WILLIAM ROWAN HAMILTON, LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the French Institute, Hon. or Corr. Member of several other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from vol. viii. p. 269.]

#### Section IV.

[26.] **F**OR quines, the equations of condition between the 24 symbols  $l_1 \dots u_3$  amount (as has been already remarked) to 80 in all; namely to 8, 24, 12, 24, and 12 equations, included respectively in the five types last mentioned, and sufficiently developed above, by the formulæ (117) (118) (143) (145) (148) (152) (153) (155): which also enable us, with the help of (141) (144), to determine the values of the four symbols

$a_1 \dots a_4$ , and of the six other symbols  $b_1 \dots c_3$ , when values of  $l_1 \dots u_3$  have been found, which satisfy the eighty conditions. And then, if we denote the quine itself by the following expression (compare [1.]),

$$P = x_0 + \iota_1 x_1 + \iota_2 x_2 + \iota_3 x_3 + \iota_4 x_4, \quad \dots \quad (158)$$

which is a little more symmetric than the form (111), the laws of multiplication of any two such quines, P, P', will be sufficiently expressed by the formulæ

$$\left. \begin{aligned} \iota_1^2 = a_1, \quad \iota_4^2 = a_4, \quad S\iota_2\iota_3 = b_1, \quad S\iota_1\iota_4 = c_1, \\ V\iota_2\iota_3 = \iota_1 l_1 + \iota_2 m_3 + \iota_3 n_2 + \iota_4 p_1, \\ V\iota_1\iota_4 = \iota_1 r_1 + \iota_2 s_1 + \iota_3 t_1 + \iota_4 u_1; \end{aligned} \right\} \quad (159)$$

if we remember that 1, 2, 3 may still be cyclically permuted, and that the law of conjugation (32) gives

$$K'l = u', \quad S'l = Su', \quad V'l = -Vu'. \quad \dots \quad (160)$$

For in this manner, by (41), if  $\varpi$  denote, as in (14), the vector part of P, so that

$$\varpi = \iota_1 x_1 + \iota_2 x_2 + \iota_3 x_3 + \iota_4 x_4, \quad \dots \quad (161)$$

we shall have

$$\begin{aligned} S\varpi\varpi' &= a_1 x_1 x_1' + a_2 x_2 x_2' + a_3 x_3 x_3' + a_4 x_4 x_4' \\ &+ b_1 (x_2 x_3' + x_3 x_2') + \&c. + c_1 (x_1 x_4' + x_4 x_1') + \&c., \end{aligned} \quad (162)$$

$$\begin{aligned} V\varpi\varpi' &= (\iota_1 l_1 + \iota_2 m_3 + \iota_3 n_2 + \iota_4 p_1) (x_2 x_3' - x_3 x_2') + \&c. \\ &+ (\iota_1 r_1 + \iota_2 s_1 + \iota_3 t_1 + \iota_4 u_1) (x_1 x_4' - x_4 x_1') + \&c., \end{aligned} \quad (163)$$

each “&c.” representing terms obtained by the permutations already mentioned; and if the constants  $abclmnpqrstu$  have been chosen so as to fulfill the conditions above developed, we may then conclude (compare (51)) that the following EQUATIONS OF ASSOCIATION hold good, for the multiplication of any three such vector-units  $\iota$ , or quadrinomial vectors  $\varpi$ , or quinquinomial expressions P, whether equal or unequal among themselves:

$$\iota . \iota' \iota'' = \iota' . \iota'' ; \quad \varpi . \varpi' \varpi'' = \varpi \varpi' . \varpi'' ; \quad P . P' P'' = P P' . P'' ; \quad (164)$$

which it has been the main object of our recent investigations to establish.

[27.] Without pretending to do more, on the present occasion, than merely to exemplify the possibility of satisfying, for quines, the foregoing equations of association, I may here remark that if we restrict the question by assuming (with the usual permutations),

$$(A, B)^* \dots \quad n_1 = m_1, \quad p_1 = 0, \quad u_1 = 0, \quad \dots \quad (165)$$

\* This line is lettered thus, because it contains the conditions common to the two systems (A) and (B) of associative quines, which are deduced a little further on.

then numerous simplifications take place, and the 80 equations between the 24 symbols  $lmnrstu$  are found to reduce themselves to 44 equations between the 15 symbols  $lmrst$ , obtained from the five types I. to V. of recent paragraphs, which may be thus denoted and arranged :

$$\text{from type I. . . } m_1^2 = l_2 l_3, \quad r_2^2 - r_1^2 = s_3 t_1 - s_2 t_3; \quad . \quad (166)$$

$$\text{from II. and III. . . } m_1 l_1 = m_2 m_3, \quad m_1 r_1 = m_2 s_1 = m_3 t_1, \quad . \quad (167)$$

$$\text{and } m_1 r_2 = l_2 s_2, \quad m_1 r_3 = l_3 t_3, \quad m_1 (r_1 + r_2 + r_3) = 0; \quad (168)$$

$$\text{from IV. . . } s_2 (r_2 + r_3) = -t_1 t_2, \quad t_3 (r_2 + r_3) = -s_3 s_1, \quad (169)$$

$$\text{and } m_1 s_1 = l_2 t_1, \quad m_1 t_1 = l_3 s_1; \quad . \quad . \quad . \quad (170)$$

$$\text{and from V. . . } m_1 s_2 = l_3 r_2, \quad m_1 t_3 = l_2 r_3, \quad l_1 (r_1 + r_2 + r_3) = 0. \quad (171)$$

Now these conditions may all be satisfied in each of two principal ways, conducting to *two distinct systems of associative quines*, which may be called Systems (A) and (B), but which are *not the only possible systems* of such quines, because we need not have commenced by assuming the equations (165), although that assumption has simplified the problem. For first we may suppose that the constants  $l$  and  $m$  are different from zero, but that the constants  $r$  are connected by the relation

$$(A) \dots r_1 + r_2 + r_3 = 0; \quad . \quad . \quad . \quad (172)$$

or secondly, we may reject this relation between the constants  $r$ , and suppose instead that the six constants  $l$  and  $m$  all vanish, so that

$$(B) \dots l_1 = l_2 = l_3 = m_1 = m_2 = m_3 = 0. \quad . \quad . \quad (173)$$

With the first supposition, (172), we are to combine the nine relations between the fifteen constants  $lmrst$ , which are sufficiently expressed by the formula (167), or by the following :

$$(A_1) \dots l_1 = m_1^{-1} m_2 m_3, \quad s_1 = m_2^{-1} m_1 r_1, \quad t_1 = m_3^{-1} m_1 r_1; \quad (174)$$

and then all the other conditions of association will be found to be satisfied, if we equate each of the ten symbols  $abc$  to zero, or if we establish this other formula,

$$(A_2) \dots a_1 = 0, \quad b_1 = 0, \quad c_1 = 0, \quad a_4 = 0: \quad . \quad . \quad (175)$$

while there will still remain *five arbitrary constants* of the system, for instance  $r_1 r_2 m_1 m_2 m_3$ . With the second supposition, (173), we are to combine four distinct relations between the nine constants  $rst$ , contained in the formula (169), or in the following :

$$(B_1)^* \dots r_1 + r_2 = -s_1^{-1} t_1 t_3, \quad s_1 s_2 s_3 = t_1 t_2 t_3; \quad . \quad . \quad (176)$$

\* It must be observed that these equations (176), which are part of the *basis* of the system (B), are *true* in the system (A) also, as corollaries from (174) and (172), which last equation does not hold in (B); and which allows us to reduce, for (A) but not for (B), the relation (177) to the simpler form  $r_1 r_2 r_3 = s_1 s_2 s_3$ .

which give also, as a consequence, this other relation :

$$(r_1 + r_2)(r_2 + r_3)(r_3 + r_1) = -s_1 s_2 s_3; \quad \dots \quad (177)$$

and then the other conditions of association will all be satisfied, if we make, instead of (175),

$$(B_2) \dots a_1 = b_1 = c_1 = 0, \quad a_4 = (r_1 + r_2 + r_3)^2: \quad \dots \quad (178)$$

this system also involving five arbitrary constants, for example  $s_1 s_2 t_1 t_2 t_3$ . The assertion respecting quines, which was made near the end of [16.], has therefore been fully justified.

[28.] Finally, as regards the system (A) of quines, it may be observed,—1st, that in this system, by (162) and (175), we have generally,

$$(A_3) \dots S\omega\omega' = 0; \quad \dots \dots \dots (179)$$

or that “the product of any two quadrinomial vectors  $\omega, \omega'$ , reduces itself to a pure vector;” and 2nd, that, by (163) (165), “this vector product,  $\omega\omega'$ , is of trinomial form, involving no part with  $\iota_4$  for a factor.” This product is therefore already seen to be of the form

$$\omega\omega' = \iota_1 X_1 + \iota_2 X_2 + \iota_3 X_3; \quad \dots \dots \dots (180)$$

but I say, 3rd, that “its three coefficients, or coordinates,  $X_1, X_2, X_3$  have constant ratios,” or that “the product  $\omega\omega'$  may be constructed by a right line in space of which the direction though not the length is fixed,” and which may therefore be conceived to “coincide in position with one fixed axis ( $\xi$ ) of the system.” In fact, by (163) (165) (174), we have

$$m_1 X_1 = m_2 X_2 = m_3 X_3 = X, \quad \dots \dots \dots (181)$$

and therefore

$$(A_4) \dots \omega\omega' = X\xi, \quad \dots \dots \dots (182)$$

if we make for abridgement

$$(A_5) \dots \left\{ \begin{array}{l} X = m_2 m_3 (x_2 x'_3 - x_3 x'_2) + m_3 m_1 (x_3 x'_1 - x_1 x'_3) + m_1 m_2 \\ \quad (x_1 x'_2 - x_2 x'_1) \\ \quad + m_1 r_1 (x_1 x'_4 - x_4 x'_1) + m_2 r_2 (x_2 x'_4 - x_4 x'_2) + m_3 r_3 \\ \quad (x_3 x'_4 - x_4 x'_3), \quad \dots \dots \dots (183) \\ \text{and} \quad \xi = m_1^{-1} \iota_1 + m_2^{-1} \iota_2 + m_3^{-1} \iota_3 \quad \dots \dots \dots (184) \end{array} \right.$$

In the 4th place, “if any quadrinomial vector  $\omega$  be multiplied by or into the axis  $\xi$ , the product vanishes;” or in symbols,

$$(A_6) \dots \xi\omega = 0, \quad \omega\xi = 0; \quad \dots \dots \dots (185)$$

because by (172) the scalar coefficient  $X$  becomes  $= 0$ , if we change either  $x_1, x_2, x_3$ , and  $x_4$ , or  $x'_1, x'_2, x'_3$ , and  $x'_4$ , to  $m_1^{-1}$ ,

$m_2^{-1}$ ,  $m_3^{-1}$ , and 0, respectively. This coefficient X vanishes also, when we equate  $x'_1 x'_2 x'_3 x'_4$  to  $x_1 x_2 x_3 x_4$  respectively; and hence, or from (179), we may infer, 5th, that "in this system of quines (A), the square of every quadrinomial vector vanishes." And finally, by an easy combination of the formulæ (182) (185), or of the 3rd and 4th of the foregoing properties of this system, we see, 6th, that, in it, "every product of three quadrinomial vectors vanishes;" or that

$$(A_7) \dots \omega \omega' \cdot \omega'' = 0, \quad \omega \cdot \omega' \omega'' = 0. \quad \dots \quad (186)$$

[29.] The *associative property* (164) is therefore verified for the system (A), by showing that, *in it*, each of the two ternary products of vectors, which ought to be equal, vanishes. In the system (B), it is easy to see that any such ternary product must be itself a vector; because, in (B), no binary product of vectors involves  $\iota_4$ , nor does any such product involve a scalar part, except what arises from  $\iota_4^2$ . We have, therefore, *here*, this new result,

$$(B_3) \dots S(\omega \omega' \cdot \omega'') = S(\omega \cdot \omega' \omega'') = 0. \quad \dots \quad (187)$$

And when we proceed to develop these two ternary products, the associative property of multiplication is again found to be verified, under the form,

$$(B_4) \dots \omega \omega' \cdot \omega'' = \omega \cdot \omega' \omega'' = a_4 (\omega x'_4 x''_4 - \omega' x''_4 x_4 + \omega'' x_4 x'_4); \quad (188)$$

where it is worth observing that, by the laws of the system in question, the result may be put under this other and somewhat simpler form:

$$(B_5) \dots \omega \omega' \cdot \omega'' = \omega \cdot \omega' \omega'' = \omega S \omega' \omega'' - \omega' S \omega'' \omega + \omega'' S \omega \omega'. \quad (189)$$

Indeed, this last expression might have been *foreseen*, as a consequence from the *general principles* of this whole theory of *associative polynomes\**, combined with the particular property (187) of the quines (B). For, by that property, each of the two ternary products is equal to its own vector part; but by (101) we have, *generally*, in the present theory, as in the calculus of quaternions, the following expression for the *vector part of the product of any three vectors*, of any such associative polynomes as we are considering:

$$V \cdot \rho \sigma \tau = \rho S \sigma \tau - \sigma S \tau \rho + \tau S \rho \sigma; \quad \dots \quad (190)$$

\* It will hereafter be proved *generally* that for *all* associative polynomes which satisfy the law of conjugation (though *not exclusively* for such *associative* polynomes), the *tensor*, as defined in [6.], is also a *modulus*; which theorem can be verified without difficulty for the quines (A) and (B), and for the quadrinomes and tetrads so lettered in [13.], as well as for the trinomes [11.].



which is a formula of continual application in Quaternions\*, and in these extensions also is important.

Observatory of Trinity College, Dublin,  
August 16, 1854.

[This little paper, which I propose to call (as above) *Section the Fourth* of these Extensions, has been in type for a couple of months, but other occupations have prevented me from revising it till now. Although very unwilling to encroach on the valuable space of the Philosophical Magazine, I have drawn up sketches of some continuations, which I shall perhaps shortly copy and send, to be printed at such times as may suit the convenience of the Editors, whose constant courtesy is hereby thankfully acknowledged.—W. R. H.

Observatory, Dec. 22, 1854.]

[To be continued.]

IX. *On a case of disturbed Elliptic Motion.*

By the Rev. A. THACKER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

YOU may perhaps find a place in your Journal for the following investigation, which was suggested by a recent discussion on the subject of the lunar theory. The problem I propose to solve may be stated thus:—A body is acted on by a central force equal to  $\frac{\mu}{r^2} + \mu'r$ ,  $\mu'$  being small; to determine the motion when the orbit is not nearly circular.

The equations of motion are

$$\frac{d^2x}{dt^2} + \left(\frac{\mu}{r^2} + \mu'r\right)\frac{x}{r} = 0, \quad \frac{d^2y}{dt^2} + \left(\frac{\mu}{r^2} + \mu'r\right)\frac{y}{r} = 0;$$

and from these we deduce immediately,

$$\frac{d\theta}{dt} = \frac{h}{r^2}, \quad \dots \dots \dots (1)$$

$$v^2 + C = \frac{2\mu}{r} - \mu'r^2, \quad \dots \dots \dots (2)$$

$$\left(\frac{dr}{dt}\right)^2 + C + \frac{h^2}{r^2} - \frac{2\mu}{r} + \mu'r^2 = 0\dagger. \quad \dots \dots (3)$$

\* Towards the close of Section III., namely of the communication which appeared in October, 1854, an accent was inadvertently omitted in the last term of the important formula (67),  $V_i V_i' t'' = i'' S_i' t - i' S_i'' t$ , which had been correctly printed in Section II., for August, 1854. [Section I. was published in the Supplement for July, 1854.] Also in the first formula (116), of par. [17.], for  $a_1$  read  $a_4$ . And in (148), of [22.], for  $s_2 r_3$  read  $s_2 u_3$ .

† This is the equation given by Professor Challis.

Since the force is central, there cannot be more than two apsidal distances. Let them be  $\alpha$  and  $\beta$ , the former being the greater. For each of these values of  $r$ ,  $\frac{dr}{dt}$  must be zero. We have, therefore,

$$\begin{aligned} 0 &= -C\alpha^2 - h^2 + 2\mu\alpha - \mu'\alpha^4, \\ 0 &= -C\beta^2 - h^2 + 2\mu\beta - \mu'\beta^4; \end{aligned}$$

whence

$$\begin{aligned} C &= \frac{2\mu}{\alpha + \beta} - \mu'(\alpha^2 + \beta^2), \\ h^2 &= \frac{2\mu\alpha\beta}{\alpha + \beta} + \mu'\alpha^2\beta^2. \end{aligned}$$

If these values be substituted for  $C$  and  $h^2$ , it will be found that

$$-Cr^2 - h^2 + 2\mu r - \mu'r^4 = (\alpha - r)(r - \beta) \left\{ \frac{2\mu}{\alpha + \beta} + \mu'(r + \alpha)(r + \beta) \right\},$$

whence

$$\frac{dt}{dr} = \pm \frac{r}{\sqrt{(\alpha - r)(r - \beta) \left\{ \frac{2\mu}{\alpha + \beta} + \mu'(r + \alpha)(r + \beta) \right\}}},$$

the upper or lower sign being taken according as the body is moving from or towards the nearer apse. If the expression  $\left\{ \frac{2\mu}{\alpha + \beta} + \mu'(r + \alpha)(r + \beta) \right\}^{-\frac{1}{2}}$  be expanded in a series ascending by powers of  $\mu'$ , the value of  $t$  in terms of  $r$  may be found by integration to any required degree of accuracy. Neglecting powers of  $\mu'$  above the first, putting  $k$  for  $\frac{\mu'}{\mu} \cdot \frac{\alpha + \beta}{2}$ , and  $U$  for  $(\alpha - r)(r - \beta)$ , we have

$$\sqrt{\frac{2\mu}{\alpha + \beta}} \cdot \frac{dt}{dr} = \frac{r}{\sqrt{U}} - \frac{1}{2}k \cdot \frac{r^3 + (\alpha + \beta)r^2 + \alpha\beta r}{\sqrt{U}}.$$

Integrating, and supposing  $t$  to begin when the body is at the nearer apse, we obtain

$$\begin{aligned} \sqrt{\frac{2\mu}{\alpha + \beta}} \cdot t &= \frac{1}{2}(\alpha + \beta) \cos^{-1} \frac{\alpha + \beta - 2r}{\alpha - \beta} - \sqrt{U} \\ &\quad - \frac{1}{32}k(\alpha + \beta)(11\alpha^2 + 10\alpha\beta + 11\beta^2) \cos^{-1} \frac{\alpha + \beta - 2r}{\alpha - \beta} \\ &\quad + \frac{1}{48}k \{ 8r^2 + 22(\alpha + \beta)r + 33\alpha^2 + 74\alpha\beta + 33\beta^2 \} \sqrt{U}. \end{aligned}$$

If we make  $\cos^{-1} \frac{\alpha + \beta - 2r}{\alpha - \beta} = u$ , it follows that

$$r = \frac{1}{2}(\alpha + \beta) - \frac{1}{2}(\alpha - \beta) \cos u; \quad . . . \quad (A)$$

and, eliminating  $r$  and restoring the value of  $k$ , we have

$$\left. \begin{aligned} \frac{2\sqrt{2}\mu}{(\alpha+\beta)^{\frac{3}{2}}} \cdot t = u - \frac{\alpha-\beta}{\alpha+\beta} \sin u - \frac{\mu'}{\mu} \cdot \frac{\alpha+\beta}{32} \cdot (11\alpha^2 + 10\alpha\beta + 11\beta^2)u \\ + \frac{\mu'}{\mu} \cdot \frac{\alpha-\beta}{192} \{ (93\alpha^2 + 198\alpha\beta + 93\beta^2) \sin u - 15(\alpha^2 - \beta^2) \sin 2u \\ + (\alpha - \beta)^2 \sin 3u \}. \end{aligned} \right\} \text{(B)}$$

Again,

$$\frac{d\theta}{dr} = \frac{h}{r} \cdot \frac{1}{\sqrt{(\alpha-r)(r-\beta)} \left\{ \frac{2\mu}{\alpha+\beta} + \mu'(r+\alpha)(r+\beta) \right\}}$$

Integrating this in the same way, and to the same degree of approximation, we shall have

$$\theta = \cos^{-1} \frac{2\alpha\beta - (\alpha+\beta)r}{(\alpha-\beta)r} - \frac{1}{2} k\alpha\beta \left\{ -\sqrt{U} + \frac{3}{2}(\alpha+\beta) \cos^{-1} \frac{\alpha+\beta-2r}{\alpha-\beta} \right\}.$$

Let  $\cos^{-1} \frac{2\alpha\beta - (\alpha+\beta)r}{(\alpha-\beta)r} = v$ , and  $\cos^{-1} \frac{\alpha+\beta-2r}{\alpha-\beta} = u$ ; then it is easily proved that

$$\tan \frac{u}{2} = \sqrt{\frac{\beta}{\alpha}} \tan \frac{v}{2}, \dots \dots \dots \text{(C)}$$

and, consequently,

$$\theta = v - \frac{1}{8} \cdot \frac{\mu'}{\mu} \sqrt{\alpha\beta} \{ 3(\alpha+\beta)^2 u - (\alpha^2 - \beta^2) \sin u \}. \dots \text{(D)}$$

The equations (A), (B), (C), (D) determine the motion; and as there is no limitation with regard to the relative magnitudes of  $\alpha$  and  $\beta$ , it follows that those equations are true, however much the orbit differs from a circle.

The apsidal angle is the value of  $\theta$  corresponding to  $r = \alpha$ , and is therefore equal to  $\pi \left\{ 1 - \frac{3}{8} \cdot \frac{\mu'}{\mu} \sqrt{\alpha\beta} (\alpha + \beta)^2 \right\}$ , or

$$\pi \left( 1 - \frac{3\mu'}{n^2} \cdot \frac{\sqrt{\alpha\beta}}{\alpha + \beta} \right),$$

if  $n$  be the mean angular motion of the body.

In the lunar theory, the expression for the radial disturbing force contains a term  $-\frac{1}{2} n'^2 r$ , where  $n'$  is the mean motion of the earth, and the effect of this on the moon's motion may be found by substituting  $-\frac{1}{2} n'^2$  for  $\mu'$  in the preceding equations.

For example, if we denote by  $m$  the ratio of the earth's mean motion to that of the moon, the apsidal angle becomes

$$\pi \left( 1 + \frac{3}{2} m^2 \cdot \frac{\sqrt{\alpha\beta}}{\alpha + \beta} \right),$$

or

$$\pi \left( 1 + \frac{3}{4} m^2 \sqrt{1 - e^2} \right),$$

if  $\alpha = a(1 + e)$  and  $\beta = a(1 - e)$ .

On referring to Pontécoulant (vol. iv. p. 566), it will be seen that the value of  $1 - c$ , which measures the rate of progression of the moon's perigee, involves the terms

$$\frac{3}{4} m^2 - \frac{3}{8} m^2 e^2 - \frac{3}{32} m^2 e^4,$$

which are identical with the first three terms of the expansion of  $\frac{3}{4} m^2 \sqrt{1 - e^2}$ .

I will only add, that if the approximation be carried a step further, the ratio of the apsidal angle to  $\pi$  will be found to be

$$1 - \frac{3}{8} \cdot \frac{\mu'}{\mu} (\alpha + \beta)^2 \sqrt{\alpha\beta} \left\{ 1 + \frac{1}{64} \cdot \frac{\mu'}{\mu} (\alpha + \beta) (25\alpha^2 + 54\alpha\beta + 25\beta^2) \right\}.$$

I am, Gentlemen,

Your obedient Servant,

Trinity College, Cambridge.

A. THACKER.

### X. Notices respecting New Books.

*On the Atmospheric Changes which produce Rain and Wind, and the Fluctuations of the Barometer. Second Edition, with additional Essays and Diagrams.* By THOMAS HOPKINS, M.B.M.S. London: John Weale. 1854.

THE chief feature of Mr. Hopkins's Essay is the importance he attaches to one fact, which he affirms has not hitherto been sufficiently attended to; and this is, the conveyance of heat to different regions of the earth's surface by *vapour*, and the effects of its liberation when the vapour is condensed. He denies the competency of the commonly received theories of the trade-winds, &c., and maintains that the principal cause of these and other similar phenomena is to be found in the fact above mentioned. After referring to the common theory of the trade-winds, originally advanced by Hadley, and now generally adopted, he says, "That the effects of the unequal heating power of the sun on the surface of the globe, and of the varying rotatory velocities of the different parts of the earth between the equator and the poles, must be of the nature described in this theory, is sufficiently evident. But it is not equally evident that the causes pointed out are adequate to the production of those general winds which are known to exist. The difference

in the rotatory velocities of different latitudes must certainly have the kind of effect described upon any winds which pass from the equator to the poles, and from the poles to the equator; and the degree of effect will be proportioned to the rapidity of the passage of the wind from one latitude to another. But the unequal heating of the surface of the globe, and consequently of the air near it, by the sun, does not, as will be hereafter shown more fully, produce those palpable or strong winds which blow in some parts towards, and in other parts from, the tropics. The heating of the surface of the globe and of the air near it, by the sun, is not a sufficiently powerful cause to produce these winds, though, like the law of diffusion, such surface-heating may, and no doubt does, to a certain extent, bring into active operation another and a more powerful cause, and as we shall see hereafter, one that is fully adequate to their production." This other "and more powerful cause" is the heat taken up in the process of evaporation, and after being carried away to other regions, liberated on the condensation of this vapour. "In the following pages," says the author, "it is proposed to show that the heat which is thus taken up by vapour, in the process of evaporation, is carried away to various parts of the atmospheric regions, and in those parts is liberated on the vapour being condensed; and that it is this liberated heat which produces that inequality of temperature which causes the greater part of those aerial movements called winds on different parts of the surface of the globe." (P. 6.)

On a subject so intricate, and of which so little is known as the present one, it is dangerous to make any positive assertions involving the general laws of the phenomena. And still more dangerous is it to assert that certain causes are, or are not, sufficient to account for certain effects, whilst neither the causes nor the effects have yet been submitted to accurate measurement and calculation. We agree with Mr. Hopkins in thinking the common theory of the trade-winds unsatisfactory in some respects; and for this very reason, viz. that we possess no accurate estimates of the elements involved in the problem. Mr. Hopkins asserts, that, "if no other cause of rain or wind existed than those yet named" (viz. in the ordinary theories), "there would be no copious rain nor strong wind in any part of the world, as those causes would evidently all operate mildly and gently. Inequality of temperature, on different parts of the earth's surface, in no place directly produces a strong wind, though there are parts where that inequality is great within small distances." (P. 11.) Now here, we think, Mr. Hopkins has made an assertion difficult of proof; and, further, that his own theory is open to the very same objection, viz. that the action of the heat of vapours, pointed out by him as the most powerful cause of winds, &c., would seem at first sight to be just as likely to "operate mildly and gently" as the inequalities of temperature, &c.

His reasonings from page 14 to page 18 are rather obscure, and, as relating to operations of which we have little or no experimental knowledge, cannot be considered satisfactory; and as these pages appear to contain the main part of our author's theories, we fear that

the whole work is open to the same remark. Still, it is highly probable that the heat of vapour plays a more important part in meteorological phenomena than has hitherto been generally supposed, and Mr. Hopkins has done a real service to the science by calling attention to this point. If he has attributed rather too much to this particular cause, it is only what is generally done, and might naturally be expected, whenever a person discovers the importance of some cause hitherto neglected.

In treating of other points, such as the fluctuations of the barometer, dew, &c., our author does not appear to have availed himself sufficiently of the recent researches of Dove, Melloni, and others. The subject of 'dew' in particular has received the greatest accession of illustration and explanation by the labours of Melloni, to which Mr. Hopkins makes no reference whatever. We also meet with some expressions in the course of the work which indicate rather loose notions of elementary scientific principles; as, for instance, at page 72, where the author speaks of a current being "*forced up*" the sides of elevated land "by its *inertia*."

Upon the whole, however, the work is written in a scientific spirit, and indicates considerable care and labour in the collection of facts. Moreover, it is eminently *suggestive*, and therefore likely to be of still greater service by leading to further inquiries.

## XI. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

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

June 15, 1854.—The Earl of Rosse, President, in the Chair.

THE following papers were read:—

“On the Structure of certain Microscopic Test-objects, and their Action on the Transmitted Rays of Light.” By Charles Brooke, M.A., F.R.S.

In order to arrive at any satisfactory conclusions regarding the action of any transparent medium on light, it is necessary to form some definite conceptions regarding the external form and internal structure of the medium. This observation appears to apply in full force to microscopic test-objects; and for the purposes of the present inquiry it will suffice to limit our observations to the structure of two well-known test-objects, the scales of *Podura plumbea*, and the siliceous loricae or valves of the genus *Pleurosigma*, freed from organic matter: the former of these is commonly adopted as the test of the *defining* power of an achromatic object-glass, and the several species of the latter as the tests of the *penetrating* or *separating* power as it has been termed. The defining power depends only on the due correction of chromatic and spherical aberrations, so that the image of any point of an object formed on the retina may not overlap and confuse the images of adjacent points; this correction is never theoretically perfect, since there will always be residual terms in the general expression for the aberration, whatever prac-

ticable number of surfaces we may introduce as arbitrary constants; but it is practically perfect, when the residual error is a quantity less than that which the eye can appreciate. The separation of the markings of the Pleurosigmata and other analogous objects, is found to depend on good defining power associated with large angle of aperture.

The Podura scale appears to be a compound structure, consisting of a very delicate transparent lamina or membranë, covered with an imbricated arrangement of epithelial plates, the length of which is six or eight times their breadth, somewhat resembling the tiles on a roof, or the long pile of some kinds of plush. This structure may be readily shown by putting a live Podura into a small test-tube, and inverting it on a glass slide; the insect should then be allowed for some time to leap and run about in the confined space. By this means the scales will be freely deposited on the glass, and being subsequently trodden on by the insect, several will be found, from which the epithelial plates have been partially rubbed off, and at the margin of the undisturbed portion, the form and position of the plates may be readily recognized. This structure appears to be rendered most evident by mounting the scales thus obtained in Canada balsam, and illuminating them by means of Wenham's parabolic reflector. The structure may also be very clearly recognized when the scale is seen as an opaque object under a Ross's  $\frac{1}{12}$ th (specially adjusted for uncovered objects), illuminated by a combination of the parabola and a flat Lieberkuhn, as the writer has elsewhere described\*. The underside of the scale thus appears as a smooth glistening surface with very slight markings, corresponding probably to the points of insertion of the plates on the contrary side. The minuteness and close proximity of the epithelial plates will readily account for their being a good test of *definition*, while their prominence renders them independent of the *separating* power due to large angle of aperture.

The structure of the second class of test-objects above mentioned differs entirely from that above described; it will suffice for the present purpose to notice the valves of three species only of the genus *Pleurosigma*, which, as arranged in the order of easy visibility, are, *P. formosum*, *P. hippocampus*, *P. angulatum*.

These appear to consist of a lamina of homogeneous transparent silex, studded with rounded knobs or protuberances, which, in *P. formosum* and *P. angulatum*, are arranged like a tier of round shot in a triangular pile, and in *hippocampus*, like a similar tier in a quadrangular pile, as has frequently been described; and the visibility of these projections is probably proportional to their convexity. The "dots" have by some been supposed to be depressions; this however is clearly not the case, as fracture is invariably observed to take place *between* the rows of dots, and not *through* them, as would naturally occur if the dots were depressions, and consequently the substance thinner there than elsewhere.

This in fact is always observed to take place in the siliceous loriceæ

\* See British Association Reports for 1850.

of some of the border tribes that occupy a sort of neutral, and not yet undisputed, ground between the confines of the animal and vegetable kingdoms; as for example the *Isthmia*, which possesses a reticulated structure, with depressions between the meshes, somewhat analogous to that which would result from pasting together bobbinet and tissue paper.

The valves of *P. angulatum* and other similar objects have been by some writers\* supposed to be made up of two substances possessing different degrees of refractive power; but this hypothesis is purely gratuitous, since the observed phenomena will naturally result from a series of rounded or lenticular protuberances of one homogeneous substance. Moreover, if the centres of the markings were centres of greatest density, if in fact the structure were at all analogous to that of the crystalline lens, it is difficult to conceive why the oblique rays only should be visibly affected. When *P. hippocampus* or *P. formosum* is illuminated by a Gillett's condenser, with a central stop placed under the lenses, and viewed by a quarter-inch object-glass of 70° aperture, both being accurately adjusted, we may observe in succession, as the object-glass approaches the object, first a series of well-defined bright dots; secondly, a series of dark dots replacing these; and thirdly, the latter are again replaced by bright dots, not however as well defined as the first series. A similar succession of bright, dark, and bright points may be observed in the centre of the markings of some species of *Coscinodiscus* from Bermuda.

These appearances would result if a thin plate of glass were studded with minute, equal and equidistant plano-convex lenses, the foci of which would necessarily lie in the same plane. If the focal surface or plane of vision of the object-glass be made to coincide with this plane, a series of bright points would result from the accumulation of the light falling on each lens. If the plane of vision be next made to coincide with the surfaces of the lenses, these points would appear dark, in consequence of the rays being refracted towards points now out of focus. Lastly, if the plane of vision be made to coincide with the plane beneath the lenses that contains their several foci, so that each lens may be, as it were, combined with the object-glass, then a second series of bright points will result from the accumulation of the rays transmitted at those points. Moreover, as all rays capable of entering the object-glass are concerned in the formation of the second series of bright focal points, whereas the first series are formed by the rays of a conical shell of light only, it is evident that the circle of least confusion must be much less, and therefore the bright points better defined, in the first than in the last series.

If the supposed lenses were of small convexity, it is evident that the course of the more oblique rays only would be sensibly influenced; hence probably the structure of *P. angulatum* is recognized only by object-glasses of large angular apertures, which are capable of admitting very oblique rays.

The writer has recently, in an address to the members of the

\* Vide Quarterly Journal of Microscopical Science, No. V. pp. 9, 10.



Royal Institution, proposed to explain the extreme darkness of the dots, under certain conditions of focus and illumination, by the hypothesis that some of the oblique rays are thrown out of the field by internal reflexion, being incident at the upper surface at an angle too large for emergence; but this does not appear to invalidate the present hypothesis respecting the course of the transmitted rays.

It does not appear to be desirable that objects should be illuminated by an entire, or, as it may be termed, a *solid* cone of light of much larger angle than that of the object-glass. The extinction of an object by excess of illumination may be well illustrated by viewing with a one-inch object-glass the *Isthmia* illuminated by Gillett's condenser. When this is in focus, and its full aperture open, the markings above described are wholly invisible; but as the aperture is successively diminished by the revolving diaphragm, the object becomes more and more distinct, and is perfectly defined when the aperture of the illuminating pencil is reduced to about  $20^\circ$ . The same point may be attained, although with much sacrifice of definition, by gradually depressing the condenser, so that the rays may diverge before they reach the object; and it may be remarked generally that the definition of objects is always most perfect, when an illuminating pencil of suitable form is accurately adjusted to focus, that is, so that the source of light and the plane of vision may be conjugate foci of the illuminator. If an object-glass of  $120^\circ$  aperture or upwards be used as an illuminator, the markings of Diatomaceæ will be scarcely distinguishable, with any object-glass; the glare of the central rays overpowering the effects of structure on those that are more oblique.

“On the Formation of Powers from Arithmetical Progressions.”  
By C. Wheatstone, Esq., F.R.S.

The same sum  $n^2$  may be formed by the addition of an arithmetical progression of  $n$  terms in various ways. Hence we are enabled to construct a great variety of triangular arrangements of arithmetical progressions, the sums of which are the natural series of square, cube and other powers of numbers. Among these there are several which render evident some remarkable relations.

Each of the following triangles is formed of a series of arithmetical progressions, the number of terms increasing successively by unity.

The first term of an arithmetical progression of  $n$  terms having a common difference  $\delta$ , and whose sum is  $n^2$ , is equal to

$$n^{(a-1)} + \frac{\delta}{2}(1-n).$$

### § 1. SQUARE NUMBERS.

If  $S = n^2$ , the first term  $= n + \frac{\delta}{2}(1-n)$ .

A.

Every square  $n^2$  is the sum of an arithmetical progression of  $n$  terms, the first term of which is unity and the difference 2.

$$\begin{aligned}
 1 & \dots\dots\dots = 1^2 \\
 1+3 & \dots\dots\dots = 2^2 \\
 1+3+5 & \dots\dots\dots = 3^2 \\
 1+3+5+7 & \dots\dots\dots = 4^2 \\
 1+3+5+7+9 & \dots\dots\dots = 5^2 \\
 1+3+5+7+9+11 & \dots\dots\dots = 6^2 \\
 1+3+5+7+9+11+13 & \dots\dots\dots = 7^2
 \end{aligned}$$

Thus, every square number is formed by the addition of a series of odd numbers commencing with unity; a result universally known.

The difference of any two squares is either an odd number, or the sum of consecutive odd numbers.

Each series may be resolved into two others consisting of alternate odd numbers, the respective sums of which are two adjacent triangular numbers, the addition of which it is well known forms a square. *Ex. :*

$$\begin{aligned}
 1+5+9+13 &= 28 \\
 3+7+11 &= 21 \\
 \hline
 49 &= 7^2
 \end{aligned}$$

## B.

Every square  $n^2$  is the sum of an arithmetical progression of  $n$  terms, the first term of which is  $\frac{n+1}{2}$ , and the common difference 1.

$$\begin{aligned}
 1 & \dots\dots\dots = 1^2 \\
 1\frac{1}{2}+2\frac{1}{2} & \dots\dots\dots = 2^2 \\
 2+3+4 & \dots\dots\dots = 3^2 \\
 2\frac{1}{2}+3\frac{1}{2}+4\frac{1}{2}+5\frac{1}{2} & \dots\dots\dots = 4^2 \\
 3+4+5+6+7 & \dots\dots\dots = 5^2 \\
 3\frac{1}{2}+4\frac{1}{2}+5\frac{1}{2}+6\frac{1}{2}+7\frac{1}{2}+8\frac{1}{2} & \dots\dots\dots = 6^2 \\
 4+5+6+7+8+9+10 & \dots\dots\dots = 7^2
 \end{aligned}$$

This arrangement renders evident that every square of an odd number is the sum of as many consecutive natural numbers as the root has units.

Every square of an odd number is the difference between two triangular numbers the bases of which are respectively  $(3n+1)$  and  $n$ . For, the sum of any series of natural numbers is the difference of two series of natural numbers commencing with unity; and since, as it is shown above, every square of an odd number is the sum of a series of natural numbers, it is also the difference between two triangular numbers.

It is also evident that series, the sums of which are squares of odd numbers, may be so taken that, when placed in succession, they will form an uninterrupted progression of natural numbers commencing with unity, the sum of which is a triangular number;

(1) + (2 + 3 + 4) + (5 + 6 + 7 + 8 + 9 + 10 + 11 + 12 + 13) ... &c. =  
 $(1^2 + 3^2 + 9^2 + 27^2 \dots + (3^n)^2) =$   
 a triangular number the base of which is the series  
 $(1 + 3 + 9 + 27 \dots + 3^n).$

§ 2. CUBE NUMBERS.

If  $S = n^3$ , the first term  $= n^2 + \frac{\delta}{2}(1 - n).$

C.

Every cube  $n^3$  is the sum of an arithmetical progression of  $n$  terms, the first term of which is unity, and the difference  $2(n + 1).$

1 ..... =  $1^3$   
 1 + 7 ..... =  $2^3$   
 1 + 9 + 17 ..... =  $3^3$   
 1 + 11 + 21 + 31 ..... =  $4^3$   
 1 + 13 + 25 + 37 + 49 ..... =  $5^3$   
 1 + 15 + 29 + 43 + 57 + 71 ..... =  $6^3$   
 1 + 17 + 33 + 49 + 65 + 81 + 97 ..... =  $7^3$

D.

Every cube  $n^3$  is the sum of an arithmetical progression of  $n$  terms, the first term of which is the root  $n$ , and the difference  $2n.$

1 ..... =  $1^3$   
 2 + 6 ..... =  $2^3$   
 3 + 9 + 15 ..... =  $3^3$   
 4 + 12 + 20 + 28 ..... =  $4^3$   
 5 + 15 + 25 + 35 + 45 ..... =  $5^3$   
 6 + 18 + 30 + 42 + 54 + 66 ..... =  $6^3$   
 7 + 21 + 35 + 49 + 63 + 77 + 91 ..... =  $7^3$

The last terms of these series are the alternate triangular numbers. If they be respectively divided by the first terms, the quotients will be the series of odd numbers.

E.

Every cube  $n^3$  is the sum of an arithmetical progression of  $n$  terms, the first term of which is  $(n^2 - n + 1)$ , and the difference 2.

1 ..... =  $1^3$   
 3 + 5 ..... =  $2^3$   
 7 + 9 + 11 ..... =  $3^3$   
 13 + 15 + 17 + 19 ..... =  $4^3$   
 21 + 23 + 25 + 27 + 29 ..... =  $5^3$   
 31 + 33 + 35 + 37 + 39 + 41 ..... =  $6^3$   
 43 + 45 + 47 + 49 + 51 + 53 + 55 ..... =  $7^3$

This, it will be observed, is a triangular arrangement of the uneven numbers in their regular order.

Every cube is the sum of as many consecutive odd numbers as there are units in the root\*.

The known theorem, that the sum of the cubes of any succession of the natural numbers commencing with unity is equal to the square of the sum of the roots, or, in other words, to the square of the corresponding triangular number, is an immediate consequence of the above.

$$(1^3 + 2^3 + 3^3 + 4^3 \dots + n^3) = (1 + 2 + 3 + 4 \dots + n)^2 = \left(\frac{n^2 + n}{2}\right)^2.$$

The sum of any series of odd numbers commencing with unity being equal to the square of the number of terms (A.), the sum of the numbers in any triangle formed as above is necessarily equal to the square of a triangular number. It is also easy to see that each cube is the difference between the squares of two consecutive triangular numbers; and, that the difference between the squares of any two triangular numbers whatever is the sum of consecutive cubes. The following equations have been found by ascertaining what differences of the squares of two triangular numbers are equal to single cubes:—

$$3^3 + 4^3 + 5^3 = 6^3$$

$$11^3 + 12^3 + 13^3 + 14^3 = 20^3.$$

#### F.

Every cube  $n^3$  is the sum of an arithmetical progression of  $n$  terms, the first term of which is a triangular number  $\frac{n^2 + n}{2}$ , and the difference  $= n$ .

$$\begin{array}{rcl} 1 & \dots\dots\dots & = 1^3 \\ 3 + 5 & \dots\dots\dots & = 2^3 \\ 6 + 9 + 12 & \dots\dots\dots & = 3^3 \\ 10 + 14 + 18 + 22 & \dots\dots\dots & = 4^3 \\ 15 + 20 + 25 + 30 + 35 & \dots\dots\dots & = 5^3 \\ 21 + 27 + 33 + 39 + 45 + 51 & \dots\dots\dots & = 6^3 \\ 28 + 35 + 42 + 49 + 56 + 63 + 70 & \dots\dots\dots & = 7^3 \end{array}$$

Each number contained in this triangle is itself the sum of an arithmetical progression of  $n$  terms. Thus, taking the fifth row for example:—

\* Since the present note was communicated to the Royal Society, I have found that this relation has been already noticed by Count d'Adhémar (*Comptes Rendus*, tom. xxiii. p. 501). Cauchy observes, "quoiqu'elle puisse, comme on le voit, se déduire des principes déjà connus, toutefois, elle est assez curieuse et très simple."

$$\begin{aligned}
 1+2+3+4+5 &= 15 \\
 2+3+4+5+6 &= 20 \\
 3+4+5+6+7 &= 25 \\
 4+5+6+7+8 &= 30 \\
 5+6+7+8+9 &= 35 \\
 &\underline{\hspace{1.5cm}} \\
 &125=5^3
 \end{aligned}$$

The sum of all the numbers contained in a square thus formed is equal to the cube of the number which occupies the upper right-hand and lower left-hand corners. The sum of the numbers in either of the diagonals is the corresponding square, and in the case of the odd numbers the sum of the middle horizontal or vertical line is also the square.

This last-mentioned relation was pointed out by Lichtenberg\*, who stated the theorem thus:—If  $a$  be a whole number, and  $A$  be the sum of all the natural numbers from 1 to  $a$ , then :

$$a^3 = A + (A + a) + (A + 2a) + (A + 3a) \dots + (A + [a - 1]a).$$

G.

Every cube  $n^3$  above 1 is the sum of an arithmetical progression of  $n$  terms, the first term of which is  $(n - 2)^2$ , and the difference = 8.

$$\begin{aligned}
 0+8 &\dots\dots\dots = 2^3 \\
 1+9+17 &\dots\dots\dots = 3^3 \\
 4+12+20+28 &\dots\dots\dots = 4^3 \\
 9+17+25+33+41 &\dots\dots\dots = 5^3 \\
 16+24+32+40+48+56 &\dots\dots\dots = 6^3 \\
 25+33+41+49+57+65+73 &\dots\dots\dots = 7^3
 \end{aligned}$$

Each progression of this triangle, consisting of an uneven number of terms, contains two consecutive odd square numbers.

An *uninterrupted* arithmetical progression commencing with unity and proceeding by the constant addition of 8, arranged in a triangular form, presents some curious results. 1st. The first terms of each line are the squares of the odd numbers in their regular sequence. 2nd. The sum of all the numbers in any two adjacent lines is the cube of an odd number.

$$\begin{aligned}
 1 &\dots\dots\dots \} \dots = 3^3 \\
 9+17 &\dots\dots\dots \} \dots = 5^3 \\
 25+33+41 &\dots\dots\dots \} \dots = 7^3 \\
 49+57+65+73 &\dots\dots\dots \} \dots = 9^3 \\
 81+89+97+105+113 &\dots\dots\dots \} \dots = 11^3 \\
 121+129+137+145+153+161 &\dots\dots\dots \}
 \end{aligned}$$

It is evident from the preceding arrangement that

$$(2n + 1)^2 = 1 + 8\left(\frac{n^2 + n}{2}\right).$$

\* G. C. Lichtenberg's *Vermischte Schriften*, vol. ix. p. 359. Göttingen, 1806.

Thus any triangular number multiplied by 8 with 1 added is equal to the square of an odd number; or, any square of an uneven number minus 1 is divisible by 8, and the quotient is a triangular number.

## § 3.

Of the higher powers I will confine myself to one example.

## H.

Every fourth power  $n^4$  is the sum of an arithmetical progression of  $n$  terms, the first term of which is  $n^2$ , and the difference  $2n^2$ .

$$\begin{array}{rcl} 1 & \dots\dots\dots & = 1^4 \\ 4 + 12 & \dots\dots\dots & = 2^4 \\ 9 + 27 + 45 & \dots\dots\dots & = 3^4 \\ 16 + 48 + 80 + 112 & \dots\dots\dots & = 4^4 \\ 25 + 75 + 125 + 150 + 225 & \dots\dots\dots & = 5^4 \\ 36 + 108 + 180 + 252 + 324 + 396 & \dots\dots\dots & = 6^4 \end{array}$$

This triangle consists of the progressions in (D.) multiplied respectively by  $n$ , or of those in (A.) multiplied by  $n^2$ .

“On the Structure and Functions of the Rostellum in *Listera ovata*.” By J. D. Hooker, M.D., F.R.S.

The author first gives an account of the form and structure of the rostellum of *Listera ovata*, and its relation and position to the anther and stigma. He finds that the rostellum is divided by parallel septa (at right angles to the plane of that organ) into a series of longitudinally elongated loculi, which gradually taper from the base upwards, and terminate at two opaque cellular spots, one on each side of the apex of the rostellum, towards which latter the loculi also converge. When the flower is fully expanded, these loculi are distended with a viscid grumous fluid, full of chlorophyll granules. Their external walls, and the septa dividing them, are formed of a delicate, transparent tissue, which is cellular at the base and apex of the rostellum only.

Their grumous contents, when examined at the earliest period of development, present the appearance of opaque club-shaped compressed bodies, with areolated surfaces; a form and appearance that may be restored at a later period by coagulating with alcohol.

At the period of impregnation the slightest irritation of the rostellum causes the sudden and forcible discharge of the contents of these loculi (through the rupture of the cellular tissue at the apex of the rostellum) and its protrusion in the form of two viscid glands, which coalesce into one, after which the rostellum rapidly collapses and contracts.

The pollen-masses, when freed from the anther-case, fall naturally upon the rostellum; they are retained there by their viscid gland-like contents, and, breaking up, the pollen-grains become (by the contraction of the rostellum) applied to the subjacent stigmatic surface.

The author adds remarks on the structure of the rostellum in

allied genera of Orchidææ, and indicates some of the more important morphological changes to which that organ is subjected, in connexion with the development of various appendages to the column and pollen in the same natural family.

“On the Immediate Principles of the Excrements of Man and Animals in the Healthy Condition,” by William Marcet, M.D.

The author describes a new method of extracting the immediate chemical constituents of the excrements of Man and animals, and gives an account of the substances obtained by its employment.

Healthy human fæces are boiled to exhaustion in alcohol. The residue is insoluble in æther, and yields to boiling water nothing but ammoniaco-magnesian phosphate. The strained alcoholic solution deposits, on standing, a sediment, from which it is decanted and then mixed with milk of lime. The subsiding lime is of a yellow-brown colour; it is dried on filtering-paper and treated with æther, cold or hot, and the solution thus obtained yields, on spontaneous evaporation, beautiful silky crystals, which are purified by solution in a mixture of alcohol and æther, repeated filtration through animal charcoal and recrystallization; they then appear in circular groups, have the form of acicular four-sided prisms, and polarize light very readily. This crystalline body the author proposes to call *Excretine*. It is very soluble in æther, cold or hot, but sparingly soluble in cold alcohol; its solution has a decided though weak alkaline reaction. It is insoluble in hot or cold water, and is not decomposed by dilute mineral acids. It fuses between 95° and 96° C., and at a higher temperature burns away without inorganic residue. When boiled with solution of potash it does not dissolve. As to its qualitative constitution, it is found to contain nitrogen and sulphur, though in small proportions; the products of its decomposition have not yet been investigated.

The author has in several cases observed the excretine to crystallize directly in the alcoholic solution of fæces before the addition of lime, and has scarcely any doubt that it exists for the most part in a free state in the excrements, and constitutes one of their immediate principles. As to its source, he observes that it appeared in excess when a considerable quantity of beef had been taken, and in less than the usual quantity in a case of diarrhœa attended with loss of appetite; but none could be directly obtained from beef on subjecting it to the same process of extraction as fæces. Neither could it be found in ox-bile, the urine, or the substance of the spleen. From the difficulty of obtaining the contents of the human small intestine in a healthy state, its presence or absence in that part of the alimentary canal has not yet been satisfactorily determined.

The lime precipitate, after having been thus thoroughly deprived of the excretine by æther, is next treated with hydrochloric acid, and water or alcohol, by which means margaric acid is extracted from it. The author is uncertain whether the margaric acid of the fæces is free or combined with excretine, but he is disposed to conclude that the neutral fats are decomposed in the intestinal canal and their acid set free. Not having been able to detect stearic acid

in human evacuations, he supposes that what is contained in the fat of mutton or beef taken as food must be converted into margaric acid in its passage through the alimentary canal.

The lime precipitate, freed from excretine and dissolved in alcohol by means of hydrochloric acid, forms a dark port-wine-coloured solution, from which the margaric acid is deposited. On then adding water to the solution and concentrating it on the water-bath, a flaky colouring matter separates, which, being purified by solution in æther and washing with water, is obtained as a dark-brown or black amorphous substance, similar to the colouring matter of blood, and to that which Dr. Harley has lately extracted from urine.

The matters brought down with the lime having been thus extracted, the sediment which spontaneously subsides from the alcoholic solution of fæces before its treatment with the milk of lime, is next examined. This deposit appears to be complex in its nature; it has a strongly acid reaction, and presents under the microscope small oily globules, mixed sometimes with crystals of excretine and accompanied by a yellow amorphous matter. By boiling with alcohol and filtration, a residue remains which the author has not yet examined, and two substances are obtained from the filtrate. The first is deposited on cooling; when collected and dried on filtering-paper it has a granular character and is quite colourless; it is very sparingly soluble in æther, fuses by heat, and burns with a bright fuliginous flame, leaving a white residue consisting of phosphate of potash. The author has not yet been able satisfactorily to decide whether this is a pure immediate principle or not; he is inclined to consider it as a combination of phosphate of potash and a pure organic substance. The filtered fluid, after separation of this matter, still contains a substance which he has called *Excretolic acid*. It is obtained by evaporating to dryness, extracting the residue with æther, adding to the æthereal solution alcohol and lime-water, and heating. The acid is precipitated in combination with lime, from which it is separated by means of sulphuric or hydrochloric acid and solution in æther. The æthereal solution, after being well washed with water to remove mineral acid, yields the pure excretolic acid on evaporation. This body is of an olive colour; it fuses between  $25^{\circ}$  and  $26^{\circ}$  C., and at a higher temperature burns without residue. It is insoluble in water and in a boiling solution of potash; very soluble in æther, sparingly soluble in cold alcohol, readily so in hot; its solutions having a marked acid reaction. The author is disposed to believe that in excrement it is combined in form of a salt, with excretine or a basic substance closely allied to it, which is obtained in the filtrate from which the excretolic acid is precipitated in combination with lime in the process of its purification.

The author failed to obtain evidence of the presence either of butyric or of lactic acid in the clear alcoholic solution of fæces filtered from the precipitate formed by the milk of lime. From the above investigation, therefore, it appears that healthy human excrements contain:—

1. A new organic substance, possessing an alkaline reaction, which the author names *Excretine*.



2. A fatty acid, having the properties of margaric acid, but not constantly present.

3. A colouring matter, similar to that of blood and urine.

4. A light granular substance, whose properties have not yet been sufficiently examined to admit of its being considered a pure substance.

5. An acid olive-coloured substance, of a fatty nature, named *Excretolic acid*.

6. No butyric acid and no lactic acid.

The fæces of various animals were submitted to the same process of analysis, with the following results:—

1. The excrements of carnivorous mammalia, viz. the Tiger, Leopard and Dog (fed on meat), contain a substance allied in its nature to excretine, but not identical with it. They contain no excretine; they yield butyric acid, which is not present in human excrements.

2. The excrements of the Crocodile contain cholesterine and no uric acid, whilst those of the Boa yield uric acid and no cholesterine.

3. The fæces of herbivorous animals, viz. the Horse, Sheep, Dog (fed on bread), Wild Boar, Elephant, Deer and Monkey, contain no excretine, no butyric acid and no cholesterine.

“On the Vine-Disease in the Port-wine Districts of the Alto-Douro, in April 1854. With a Supplementary Note on the Proposed Remedies for its Eradication.” By Jos. James Forrester, Esq., F.R.G.S.

In Portugal, where the vine-disease committed great ravages last year, no measures have as yet been adopted for ascertaining whether the disease is *radical*, or only superficial; or whether any *practical remedy* may be adopted in order to arrest the progress of the evil.

At Oporto, and in the north of Portugal, an opinion prevails—

“That the *Oidium* is the *effect*, and not the *cause* of the epidemic.

“That the roots and the wood of the vines are diseased.

“That *sporules* of the *Oidium* exist in the interior of the vine, and about its roots.

“That the obstruction to the ascent of the sap through the various ducts, originates in the roots.

“That black spots appear in the joints of the branches, indicating that disease exists throughout the body of the vine.

“That a new fungus has appeared on the vines, in the shape of small globules, containing carbonic acid.

And “that, although vegetation may continue for a while, the fruit will not ripen, and the vines will die in a couple of years from this date.”

Considering that it would be of some importance to determine whether the disease has its origin in the roots or from external causes, and with a hope that some practical cure for the diseased vines grown in the open air may be discovered, I record the results of my own observations of the progress of the vine-malady in the Alto-Douro.

The Port-wine District extends eight leagues west and east from the Serra do Marão (an elevation of 4400 feet\* from the level of the sea) to the Quinta do Baleira, near Sam João da Pesqueira, and four leagues north and south, between Villa Real and the city of Lamego †.

The winter streams, tributaries to the Douro, on the right bank, are the Sermenha, Corgo, Ceira, Pinhão, and Tua; and, on the left, the Varoza, Temilobos, Tedo, Tavora, and Torto.

At Baleira, the Douro runs at an elevation of not more than 250 feet; whence some opinion may be formed of the nature and inequality of the country, and of the numerous abrupt mountain ridges, on the inclines of which the vines are grown. The Wine-Districts of the Alto-Douro form a long irregular basin, girt by the granite chains of the Tras-os-Montes and Beira; and this being for the most part of schist formation, and protected from the bleak winds, is particularly adapted for the cultivation of the vine. The strata of the margins of the Douro differ from the higher and middle grounds in character, "being composed of strong clays, more or less micaceous."

The extreme northern and southern boundaries—from the Serra do Marão to Favaio, and from the Serra do Monte Muro (near Lamego) to Sam João da Pesqueira—are undulating mountain plains of still heavier soil, and more suitable for the growth of firs than vines. In former years, this fact was clearly defined by the Royal Wine Company, who divided the districts into two, one being termed Feitoria (where the most superior wines were produced and classified for exportation), the other Ramo, where only very inferior wines, for the consumption of the country and for distillation, were produced to a small extent. Now, the two districts have become one; the plantations of pines on the heights and the corn-producing valleys having alike been converted into vineyards; the *quantity*, and not the *quality*, of the produce being the results sought by the wine-grower within this privileged demarcation.

One thousand vines generally produce a pipe of wine, and the total number of vines in the Port-wine Districts above described may be estimated at 90,000,000.

In the summer time, there is great scarcity of water throughout the district. The vineyards are for the most part situated on abrupt mountain slopes, the vines being planted on terraces, which are not appropriate for the cultivation of anything else. The vines are grown not higher than three feet from the ground, and are planted about six feet apart, supported with canes or stakes. The labour in the vineyards is performed by the natives of Galicia, who visit the district three or four times a year in search of employment.

In July 1850, I first observed a blight on three or four vines, at a considerable distance from each other, in the Wine Districts. The

\* "Considerações geraes sobre a Constituição Geologica do Alto-Douro." By Dr. J. P. Rebello. Porto, 1848.

† See map of the Wine-Districts of the Alto-Douro. By J. J. Forrester.

general appearance of this blight to the naked eye greatly resembled that which appears on the peach-tree and the rose. The Douro farmers had often previously noticed a similar *po' branco* (white powder) on the vines.

In 1851 the season was favourable, and the vines (on which we had observed the blight in the previous year) were vigorous, and produced perfect fruit. The vintage of 1851, throughout the Alto-Douro, was excellent. In 1852 there was much wet and cold; the blight again appeared, and the vines were attacked to the extent of about one in fifteen hundred. The vintage of 1852 was of inferior quality; but no one ascribed the failure to any disease in the vine. From the autumn of 1852 until midsummer 1853, continued rain, sleet, hail, and bleak winds prevailed, and in 1853 there was no spring. In March of the same year the navigation of the Douro was impeded, and the bar rendered impassable on account of the floods; and in April and May of the same year, prayers were offered up in the churches throughout the Wine Districts for fine weather.

In March 1854, only half-cargoes could be brought down the river Douro, on account of the want of water, and rain was prayed for.

Early in June 1853, the heat became suddenly intense, and the vines had already burst forth with great vigour; whilst, in the middle of the same month, the nights became as cold as in winter.

In the most exposed situations the vines received the greatest shock; the circulation of the sap was evidently deranged, and their fruit withered as soon as it appeared. In some neighbouring vineyards, less exposed, the grapes grew no larger than peas; they were then suddenly covered with the blight (now designated the *Oidium*), and in about three days became rotten.

On the inclines of the mountains on either bank of the river Douro, the waters had run off, and but little blight appeared. In the low and heavy grounds, the most sheltered from the winds, the waters remained stagnant; yet the fruit grew to its full size, and had come to maturity, when the new wood, leaves, and fruit were all, to a greater or less degree, covered with the *Oidium*. The blight sometimes attacked entire vineyards, and at other times only partially affected one property, and then showed itself in others at a distance—intermediate estates being for the time wholly untouched.

It was in July 1853 that the existence of the disease in the vineyards of the Douro first attracted particular attention; but many vines betrayed no unhealthy symptoms until the fruit was nearly ripe. The upper part of the branches was first attacked. In some instances the woody part of the young branches was speckled with the *Oidium*, while the bunches of fruit were apparently altogether free from it. In other instances, the grapes became touched with the disease immediately before the vintage, but the woody part of the branches betrayed no such symptoms. In some vines, which I supposed had altogether escaped the disease (and long after the fruit was gathered and the leaves had fallen off), blotches or stains, evidently the *mycelium* of the *Oidium*, appeared on the wood.

The usual number of seeds in a black grape is two or three; but

in the year 1853, in all instances the grapes, which at first promised abundance of wine, were found each to contain from three to five seeds.

Twenty-one baskets of grapes usually produce one pipe of wine; but in the year 1853, a pipe of wine was rarely obtained even from thirty baskets of grapes. From seven to nine pipes of ordinary wine generally give a pipe of brandy, 20 per cent. above British proof; but in the year 1853, from ten to twelve pipes of ordinary wine were required to give one pipe of brandy of that strength.

Wines, when properly made, should be trodden continuously for 36 hours in the *lagar* (an open stone vat), and remain there for 36 to 48 hours more, until the tumultuous fermentation be completed, when they should be run off into larger *tonels* (wooden vats, not tightly bunged), where the second fermentation will be completed about Christmas. In 1853, in situations where the disease most prevailed, the grapes fermented before they had been trodden more than twelve hours, when the wines were drawn off and passed into *tonels*, where brandy, as a precautionary measure, was given to them. The fermentation of these wines ceased altogether before the 15th October. In other situations, where the disease had not made progress, the grapes were sound; and, where they were properly crushed and fermented, they produced excellent wine, without the addition of brandy.

Wines, during their second fermentation, deposit a thick coating of argol on the sides of the *tonels*. In 1853 there was very little argol deposited; but the gross lees of the wine were in great demand, and sold for about 15s. per basket,—a sum which in former years might almost have purchased double the quantity of grapes.

In the same manner as the form and colour of the wood, leaves, and fruit of vines differ, so does their pith vary in appearance, according to the age of the wood or the quality of the vine. The pith in an old vine, when the sap is rising, graduates from a deep vandyke-brown colour to a pale yellow, the shade being always darker near the joints.

In April 1854, I rooted up many vines of different qualities, and in various situations, and I was unable then to detect any remarkable appearance in the interior of the vine different from what I had seen in other years after continued wet and cold weather; but the exterior of all the last year's branches bore palpable evidence of having been violently attacked with the *Oidium*. Some vines had suffered more than others, and many of their vessels were evidently choked; but, in most instances, in cutting the vine longitudinally, this obstruction was found to have arisen either from wounds, bad pruning, or natural decay. I found no black spots at the joints of the branches; and, with the exception of the stains left by the disease of last year, the vines looked healthy and vigorous, throwing out strong shoots and promising an abundance of fruit.

Towards the end of April 1854, much rain fell in the district; the easterly winds destroyed the young branches; and in exposed situations and heavy soils the *Oidium* again made its appearance.

In 1853, the disease attacked the vines bodily, and almost simultaneously: whereas, in 1854, the *Oidium* appears to be creeping out of the skin of the last year's wood, and insidiously to extend itself over the branches.

The globules (to which allusion has been made above) cover the young shoots. I have been familiar with these for twenty-three years past, and the Douro farmers call them the "perspiration" of the vine. They do not indicate disease, whereas the smallest possible quantity of the *po' branco*, or white powder, being transferred to a perfectly healthy vine, immediately infected it.

In the Alto-Douro the oranges, lemons, citrons and limes have all been blighted, and every kind of vegetable appears to be suffering from sickness.

The vines which suffered most in the Alto-Douro, in 1853, were the Muscatel, Malvazia, Alvarilhão, Ferral, Agadanho and Senzão.

Since my arrival in this country I have noticed that the vines grown on walls in the open air, vines grown in greenhouses, vines grown in hot-houses, vines forced, all show identically the same effects of the *Oidium* of last year, as exist on the vines in the Alto-Douro.

Taking into consideration all the circumstances above narrated, I have come to the conclusion,—

'That the *Oidium* is the *cause*, and not the *effect* of the disease; that the inclemency of the season in 1853, by checking the circulation of the sap in the vines, produced a predisposition for disease; that if the *Oidium* continues to appear on the branches of the vines, it is only too probable that it may in a very few years be destroyed; that the *globules* are a sign of health and not of disease, and have no connexion whatever with the fungus called *Oidium*; and that if the germ of the *Oidium*, probably still lurking on the old branches, can be destroyed in the open air as effectually as it appears to have been destroyed under glass, then I feel persuaded that all the vines in the Port-wine districts of the Alto-Douro may be saved.

"Supplementary Note on the proposed Remedies for the Eradication of the Vine-Malady."

1st. I will take the annual production of wines in the Port-wine districts of the Alto-Douro at 80,000 pipes instead of 90,000, and the number of vines to be treated as diseased at 80,000,000.

2nd. The value of freehold land in that district, for the growth of 1000 vines, or one pipe of wine, may be estimated at 50*l.*, yielding an interest or rental of 3*l.* per annum.

3rd. The total freehold value of the vineyards in those districts may be estimated at 4,000,000*l.* sterling, giving an annual revenue of 240,000*l.*

4th. In the event of the disease not being checked in its progress, and the grapes being destroyed this year in the Alto-Douro, a *minimum* loss of 240,000*l.* will be sustained, and should the vines perish, the loss may be 4,000,000*l.*

5th. Portugal is said to produce annually 1,000,000 pipes of wine of all sorts and qualities, but I will estimate the total produc-

tion at 800,000 pipes, and the total number of vines in the country at 800,000,000.

If *Flour of Sulphur* be used, the leaves, branches and shoots are first moistened as equally as possible with a syringe; then the whole is dusted with sulphur, which adheres to the moistened surface.

This operation would have to be repeated thrice, and would consume two ounces of sulphur for every vine, in each of the operations, making a total of 480,000,000 ounces, or about 13,392 tons for the treatment of the 80,000,000 vines in the Alto-Douro, and 133,920 'ons for the vines of the whole country.

Sulphur would not cost less than 10*l.* per ton, delivered in the centre of the Alto-Douro districts, or in any other part of the interior of Portugal. The expense of sulphur required for the Douro would be 133,920*l.*, and for the whole country 1,339,200*l.*

One man could moisten one vine in one minute, and another man could dust it with sulphur in the same time, so that two men could perform the complete operation on about 700 vines daily, at a cost of 1*s.* 3*d.* each man for labour, making a total of 14,285*l.* in the Alto-Douro, and 142,850*l.* for all Portugal.

I will suppose that there are 4000 vineyards in the Alto-Douro, planted each with 20,000 vines. The first cost of syringes and fumigators would amount to not less than 10*l.* for each vineyard, or a total of 40,000*l.* for the Alto-Douro.

One quart of water would be required for every vine in each operation, making a total of about 90,000 pipes, the cartage of which, and the labour of distributing it over the mountain vineyards, in tubs, on men's heads, would cost a minimum of 10*s.* per pipe, or a total of 45,000*l.* for the Alto-Douro, and 450,000*l.* for the whole country.

#### Recapitulation.

	In the Douro.	In the whole country.
For sulphur, say .....	£135,000	£1,350,000
For labour, at £15,000 for each of the } three operations .....	45,000	450,000
For water, at £45,000 for each of the } three operations, or as much as the } sulphur.....	135,000	1,350,000
For instruments .....	40,000	400,000
	<u>£355,000</u>	<u>£3,550,000</u>

This is independent of any charge for factors or superintendents, or for the extra expense in treating vines and vineyards which are so much further apart than are those in the Alto-Douro.

This expense to be incurred *in the endeavour* to save *one year's crop*, would be equal to a charge of 4*l.* 10*s.* per pipe, or to a *year and a half's rental* of the vineyards, or to more than the whole revenue of Portugal for an entire year.

If a *solution of lime and sulphur* be employed instead of *flour of sulphur*, the operation would not be less expensive.

If, in *conjunction with the sulphuring* of the branches, the roots were to be exposed, and sulphur and lime thrown upon them, I could not estimate the total expense at less than  $1\frac{1}{4}d.$  to  $1\frac{1}{2}d.$  per vine, which would entail a charge equal to another year and a half's rental of the vineyards, or 18 per cent. on their freehold value for the chance of saving one year's crop.

Again, if the trunks of the vines be bored and the sulphur inserted, this most delicate operation could only be performed by the factors themselves, and if the vines were to be cut down to the ground and grafted with cuttings from sound vines, the entire operation (which could only be performed by the factors) would cost  $1\frac{1}{4}d.$  to  $1\frac{1}{2}d.$  for each vine, or as much as the sulphuring process; and besides this, there would be a loss of four years' produce at  $3l.$  per pipe per annum, making a total loss of  $16l. 10s.$  in every vineyard growing vines capable of yielding one pipe of wine, or about  $33\frac{1}{3}$  per cent., or one-third of the freehold value of the estate.

Lastly, the dressing of the trunk and branches of 800,000,000 vines with mineral tar could not be carried into operation within any reasonable period, on account of the tediousness of the process and the scarcity of labourers. The expense of the tar would also be a bar to its being used.

## XII. Intelligence and Miscellaneous Articles.

ON A PECULIAR PHÆNOMENON IN THE ELECTRO-DEPOSITION OF ANTIMONY. BY MR. G. GORE.

**I**F a piece of metallic antimony is connected by a wire with the positive pole of a small Smee's battery of one or two pairs of plates, and immersed in a solution of hydrochlorate of terchloride of antimony, *i. e.* the ordinary chloride of antimony as prepared for pharmaceutical purposes, and a clean piece of sheet copper of similar size, or a little larger, connected by a wire with the negative pole of the battery, and immersed in the same liquid, at a distance of two or three inches from the antimony, a strong current of electricity will pass through the liquid, and metallic antimony will immediately be deposited all over the piece of copper, and will form a distinct coating in two or three minutes; if the power of the battery is too strong, the deposited metal will have a dull appearance, and less battery power should be employed, or the piece of antimony immersed to a smaller extent in the liquid; the deposit will then in a short time assume a fine bright appearance, somewhat similar to highly polished silver. If the process be allowed to continue for twenty-four hours, the coating of antimony will be at least half the thickness of a sixpence; and by continuing it for eight or nine days, I have obtained a continually bright and reguline deposit of upwards of half an inch in thickness.

If, during any part of the time the deposit is progressing, the deposited antimony be taken out and struck gently, or rubbed, with any hard substance, such as metal or glass, an explosion occurs, with a small cloud of white vapour, sometimes with a flash of light, and nearly always with considerable heat, sufficient to burn one's fingers, melt gutta percha, burn paper, and even scorch deal wood quite brown, and invariably accompanied by fracture of the deposited metal; sometimes, if the process of deposition has been interrupted, and the deposited metal is not homogeneous, only a thin scale falls off, in such case the explosion and heat are less; in other instances, where the process was regular and the metal homogeneous, the fracture extended quite through the metal to upwards of one-eighth of an inch in depth.

I have observed this phenomenon in about nine instances, in several of which the explosion took place *even in the liquid*, by striking the deposit against the glass containing vessel; and in one instance it occurred after the metal had been well washed with dilute hydrochloric acid, dried, and had remained out of the liquid several hours.

The same phenomenon occurred with deposits obtained in a solution composed of one fluid ounce of the antimony liquid, and half a fluid ounce of a saturated aqueous solution of hydrochlorate of ammonia.

Birmingham.

#### ON THE COMPOSITION AND PROPERTIES OF FATS. BY D. HEINTZ.

According to the investigations of Heintz upon fats, these bodies always furnish, on saponification with potash, fatty acids and glycerine, as indeed has been known ever since Chevreul's experiments. According to the author's experiments, the acids of the acetic acid series, expressible by the formula  $C^n H^n O^4$ , occur together with oleic acid in fats; but those acids of this series in which  $n$  is a number not divisible by 4, are absent from the products of the saponification of fats. Thus the non-existence of margaric acid ( $C^{34} H^{34} O^4$ ) as a chemically pure substance is particularly proved.

In his most recent investigation, Heintz shows that even the saponification of spermaceti furnishes no other fatty acids than those which can be expressed by the general formula  $C^{4n} H^{4n} O^4$  ( $n$  = a whole number). The cetic acid ( $C^{30} H^{30} O^4$ ) and cocic acid ( $C^{26} H^{26} O^4$ ), formerly supposed by him to exist in that substance, are mixtures of at least two of the fatty acids of spermaceti. Besides stearic acid ( $C^{36} H^{36} O^4$ ) and palmitic acid ( $C^{32} H^{32} O^4$ ), the occurrence of which in spermaceti has already been proved by the author, two other acids have also been obtained in a chemically pure state from that substance. These are myristic acid ( $C^{28} H^{28} O^4$ ) and laurostearic acid ( $C^{24} H^{24} O^4$ ). Their separation was effected by the method of partial precipitation combined with that of recrystallization.

Myristic acid in the pure state has hitherto been unknown, for



Playfair, who first mentioned it, gives its melting-point at  $120^{\circ}$  F., whilst it really fuses at  $129^{\circ}$  F. When cold, it is exactly like palmitic acid, appearing in scaly crystals. It is however more readily soluble in alcohol, and crystallizes from this solution in laminae of a pearly lustre. Analyses of the acid itself, as well as of its silver, lead, copper, baryta and magnesia salts, furnish concordant results leading to the formula  $C^{28}H^{27}O^3 + RO$ . Its combination with oxide of æthyle (myristic æther) melts at  $83^{\circ}\cdot3$  F., crystallizes very beautifully in the cold, and dissolves readily in hot alcohol. Its composition is  $C^{28}H^{27}O^3 + C^4H^5O$ .

Laurostearic acid, which was prepared by Marsson from the oil of laurel berries, by Sthamer from the fat of pichurine beans, and by Görgey from cocoa-nut oil, melts with rather more difficulty than stated by these chemists. Its melting-point is  $110^{\circ}\cdot5$  F. It dissolves very readily in alcohol, and only crystallizes partially from this solution at a low temperature. It is transparent, but still forms scaly crystals; it consists of  $C^{24}H^{23}O^3 + HO$ , as appears from analyses not only of the acid itself, but also of its silver, lead and baryta salts.

Gottlieb has already noticed, that stearic acid mixed with margaric acid in certain proportions, may acquire a lower melting-point than that of the last-mentioned acid. As this, according to the author's previous investigations, is a mixture of stearic and palmitic acids, this peculiarity must be a property of this mixture, which in fact is the case, as margaric acid itself is nothing but a mixture of palmitic acid with stearic acid, which melts more readily than the former acid. Heintz has found, however, that any two fatty acids may form a mixture possessing a lower melting-point than even the most fusible of them in a pure state. He has drawn up the following tables, showing the melting-point and mode of solidification of mixtures in simple proportions of every two of the four acids,—stearic acid, palmitic acid, myristic acid, and laurostearic acid:—

A mixture of		Melts at	Solidifies at	Form of solidification.
stearic acid. parts.	palmitic acid. parts.			
100	0	$156\cdot56^{\circ}$ F.	$\dots^{\circ}$ F.	scaly crystalline.
90	10	152·96	144·5	the same.
80	20	149·48	140·54	finely acicular.
70	30	145·08	106·68	the same.
60	40	140·48	133·70	uncrystalline tubercular.
50	50	133·88	131·00	laminar crystalline,
40	60	133·40	130·1	the same.
35	65	132·08	129·74	uncrystalline, shining.
32·5	67·5	131·36	129·2	the same.
30	70	131·18	129·2	the same, lustreless.
20	80	135·5	128·84	very indistinctly acicular
10	90	140·18	130·1	beautifully acicular.
0	100	143·6	$\dots$	scaly crystalline.

A mixture of		Melts at	Solidifies at	Form of solidification.
palmitic acid. parts.	myristic acid. parts.			
100	0	143·6° F.	.. °F.	scaly crystalline.
95	5	141·98	136·4	the same.
90	10	140·18	132·26	the same.
80	20	136·4	128·3	fine scaly crystals.
70	30	130·82	124·34	extremely fine needles.
60	40	124·7	121·1	uncrystalline tubercular.
50	50	119·04	113·54	large laminar crystals.
40	60	116·6	110·66	indistinctly laminar.
35	65	115·7	..	uncrystalline opaque.
32·5	67·5	115·16	111·2	the same.
30	70	115·16	110·66	the same.
20	80	121·1	106·34	uncrystalline.
10	90	125·54	113·54	in long needles.
0	100	128·84	..	scaly crystals.

A mixture of		Melts at	Solidifies at	Mode of solidification.
myristic acid. parts.	laurostearic acid. parts.			
100	0	128·84° F.	.. °F.	scaly crystals.
90	10	125·54	117·14	the same.
80	20	121·28	112·1	fine crystals, neither distinctly scaly nor acicular.
70	30	116·06	102·2	the same.
60	40	109·4	102·2	uncrystalline, with a few shining spots.
50	50	99·32	96·26	large laminar crystals.
40	60	98·06	92·3	uncrystalline, with a few shining spots.
30	70	95·18	90·14	uncrystalline.
20	80	101·3	91·4	the same.
10	90	106·34	96·8	acicular crystals.
0	100	110·48	..	scaly crystals.

A mixture of		Melts at	Form of solidification.
stearic acid. parts.	myristic acid. parts.		
0	100	128·84° F.	
10	90	125·06	uncrystalline opaque.
20	80	119·04	indistinctly crystalline.
30	70	118·76	laminar crystals.
40	60	122·72	beautiful large laminar crystals.

A mixture of  
palmitic laurostearic  
acid. acid.

parts.	parts.	Melts at	Form of solidification.
0	100	110·48° F.	
10	90	106·7	uncrystalline.
20	80	98·73	indistinctly crystalline.
30	70	100·94	small laminar crystals.
40	60	104·18	beautiful large laminar crystals.

A mixture of  
stearic laurostearic  
acid. acid.

parts.	parts.	Melts at	Form of solidification.
0	100	110·48° F.	
10	90	106·7	uncrystalline.
20	80	101·3	uncrystalline, warty.
30	70	110·12	the shining faces of small crystals appearing on the surface.
40	60	123·44	uncrystalline, warty.

From these tables it appears that,—

1. By the addition of any fatty acid to from 4 to 10 times its quantity of another fatty acid, the melting-point of the latter is lowered, even though the acid added be more difficult of fusion.

2. The mixture of two acids differing by  $C^4 H^4$ , which possesses the lowest melting-point, consists of about 3 parts of that which contains the most carbon and 7 parts of the other.

3. The mixture of two acids differing by  $C^8 H^8$ , which possesses the lowest melting-point, consists of about 25 parts of the richest in carbon and 75 of the other.

4. The mixture of two acids differing by  $C^{12} H^{12}$ , which possesses the lowest possible melting-point, consists of about 20 parts of that which contains most carbon and 80 of the other.

5. Thus the greater the difference in the amount of carbon in two acids, the smaller is the quantity of that which contains most carbon required to produce the lowest melting-point.

6. The greater the amount of carbon in two acids differing by  $C^4 H^4$ , the less is the difference between the melting-point of the pure acids and the lowest point of the mixed acids.

7. If to 9 parts of an acid  $C^{4n} H^{4n} O^4$ , we add 1 part of an acid  $C^{4(n+1)} H^{4(n+1)} O^4$ , and to a similar quantity of the former also 1 part of an acid  $C^{4(n-1)} H^{4(n-1)} O^4$ , two mixtures are obtained possessing the same melting-point. The same applies, or nearly so, to mixtures of 8 and 7 parts  $C^{4n} H^{4n} O^4$ , and 2 and 3 parts  $C^{4(n+1)} H^{4(n+1)} O^4$ , or  $C^{4(n-1)} H^{4(n-1)} O^4$ .

8. A mixture of a little more than 3 parts of the acid  $C^{4n} H^{4n} O^4$  with a little less than 7 parts of the acid  $C^{4(n+1)} H^{4(n+1)} O^4$ , possesses the same melting-point as the acid  $C^{4n} H^{4n} O^4$  in the pure state.

The mixture of 9 parts  $C^{4n} H^{4n} O^4$  with 1 part  $C^{4(n+1)} H^{4(n+1)} O^4$ , solidifies in acicular crystals (like margaric acid).

10. The mixture of equal portions of fatty acids differing by  $C^4 H^4$ , solidifies in large laminar crystals (like anthropic acid).

11. Mixtures of 20 to 30 parts  $C^{2n} H^{4n} O^4$  with 80 to 70 parts  $C_{4(n+1)} H^{4(n+1)} O^4$ , crystallize in extremely fine needles.

12. Mixtures of 60 parts  $C^{4n} H^{4n} O^4$  with 40 parts  $C^{4(n+2)} H^{4(n+2)} O^4$ , solidify in large laminar crystals (like anthropic acid).

Heintz has also found that when, to a mixture of two acids differing by  $C^4 H^4$ , a small quantity of one containing a larger amount of carbon, and consequently more difficult of fusion, is added, the melting-point becomes still lower by several degrees. For instance, if about 3 to 4 parts of stearic acid, which melts at  $124^{\circ}26$  F., be added to the mixture, fusing at  $115^{\circ}16$  F., of palmitic acid (which fuses at  $143^{\circ}6$  F.) and myristic acid (which melts at  $128^{\circ}84$  F.), the mixture obtained fuses at  $110^{\circ}84$  F. Such mixtures of these fatty acids consequently behave very like the readily fusible metallic mixtures, which also consist of three metals (lead, tin and bismuth). — *Bericht der Akad. der Wiss. zu Berlin*, 1854, p. 207.

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ON PHILLYRINE. BY C. BERTAGNINI.

In the *Annalen der Chemie* for October, M. Bertagnini gives a preliminary account of some experiments with *Phillyrine*, by which he finds it to be a body analogous with salicine.

The phillyrine is obtained by treating a decoction of the bark of the *Phillyria* with oxide of lead or lime. After filtration and appropriate evaporation, the phillyrine separates out in crystalline form.

It is almost tasteless, and little soluble in cold water, soluble in hot water and alcohol, insoluble in æther; not precipitated from its solutions by salts of the metals.

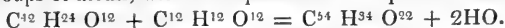
On boiling it with dilute hydrochloric acid, a resinous matter separated. The liquid was then neutralized with carbonate of lead, filtered and evaporated: the mass was treated with alcohol, and the alcoholic solution evaporated in the water-bath, when a sweet syrup was obtained, which after some weeks changed into a papillary crystalline mass, with all the properties of grape-sugar.

Phillyrine was not acted upon by synaptase; but when submitted to the lactic fermentation, it gave, on the one hand, the usual products of the lactic fermentation of sugar, and on the other, the resinous substance obtained by the action of hydrochloric acid, but in this case pure. This is *Phillygenine*. It crystallizes easily, and gives pure white pearly masses. It is not soluble in cold, and only slightly so in warm water, but easily soluble in alcohol and æther. By concentrated sulphuric acid it is coloured red.

The analyses of phillyrine lead to the formula  $C^{54} H^{34} O^{22} + 3HO$ . At  $100^{\circ}$  C. it loses all its water of crystallization.

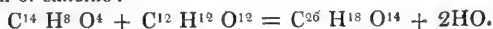
Phillygenine, on analysis, gave numbers corresponding with the formula  $C^{42} H^{24} O^{12}$ . Since phillyrine gives on decomposition only grape-sugar and phillygenine, its constitution must entirely corre-

spond with that of salicine, and it must be formed by the union of two groups of atoms, with the separation of 2 equivs. of water:—



Phillygenine. Grape-sugar. Anhydrous phillyrine.

This equation is quite analogous to that which expresses the constitution of salicine:—



Saligenine. Grape-sugar. Salicine.

It is remarkable that phillygenine is polymeric with saligenine, for the formula of saligenine tripled gives exactly that of phillygenine:—



Saligeniné. Phillygenine.

The action of chlorine and bromine on phillyrine is entirely analogous with their action on salicine.

The author is engaged in the study of these bodies.—*Ann. der Chem. und Pharm.*, vol. xcii. p. 109.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1854.

*Chiswick*.—November 1. Dense fog: very fine. 2. Foggy: hazy: very fine. 3. Fine: clear. 4. Overcast: fine. 5. Cloudy. 6. Clear, and very fine. 7. Foggy: cloudy. 8. Overcast: fine. 9. Clear and cold: sharp frost: rain. 10. Fine, but cold: rain. 11. Slight rain: cloudy: fine. 12. Fine: frosty at night. 13. Foggy: fine. 14. Densely clouded: heavy rain. 15. Heavy rain: fine: rain. 16. Rain: foggy at night. 17. Foggy: rain: overcast. 18. Densely overcast and windy. 19. Overcast: clear and cold. 20. Cloudy: clear and cold: fine. 21. Overcast. 22. Clear: densely clouded: clear. 23. Fine: cloudy: sharp frost. 24, 25. Cloudy and cold. 26. Clear: overcast. 27. Foggy. 28. Overcast: rain. 29. Cloudy and fine. 30. Clear: rain at night.

Mean temperature of the month .....	39°·35
Mean temperature of Nov. 1853 .....	40·14
Mean temperature of Nov. for the last twenty-eight years ...	43·07
Average amount of rain in Nov. ....	2·38 inches.

*Boston*.—Nov. 1. Fine. 2. Cloudy. 3. Fine. 4. Fine: rain P.M. 5. Cloudy. 6, 7. Fine. 8. Cloudy. 9. Fine: snow and rain A.M. and P.M. 10. Fine: rain P.M. 11. Fine. 12. Fine: rain P.M. 13. Cloudy. 14. Cloudy: rain A.M. 15, 16. Rain A.M. and P.M. 17. Rain A.M. 18. Cloudy: rain P.M. 19. Fine: rain A.M. and P.M. 20. Cloudy. 21. Cloudy: rain A.M. and P.M. 22, 23. Cloudy. 24, 25. Cloudy: rain A.M. and P.M. 26, 27. Fine. 28. Cloudy: rain P.M. 29. Fine rain A.M. 30. Cloudy.

*Sandwick Manse, Orkney*.—Nov. 1. Showers A.M.: cloudy P.M. 2. Rain A.M.: clear P.M. 3. Bright A.M.: showers P.M. 4. Cloudy A.M.: rain P.M. 5. Cloudy A.M. and P.M. 6. Cloudy A.M.: showers P.M. 7. Showers A.M. and P.M. 8. Showers A.M.: snow-showers P.M. 9. Snow-showers A.M.: sleet-showers P.M. 10, 11. Cloudy A.M.: clear P.M. 12, 13. Cloudy A.M. and P.M. 14. Cloudy A.M.: cloudy, drops P.M. 15. Showers A.M. and P.M. 16. Showers A.M.: rain P.M. 17. Drizzle A.M.: damp P.M. 18. Fine, cloudy A.M.: showers P.M. 19. Fine, cloudy A.M.: clear P.M. 20. Damp A.M.: showers P.M. 21. Showers A.M. and P.M. 22. Rain A.M.: showers, aurora P.M. 23. Clear, frost A.M.: snow-showers frost P.M. 24. Snow A.M. and P.M. 25. Snow, thaw A.M.: thaw, rain P.M. 26. Fog A.M.: cloudy P.M. 27. Cloudy A.M.: showers, sleet P.M. 28. Showers A.M. and P.M. 29. Showers A.M.: sleet-showers P.M. 30. Clear A.M.: sleet-showers P.M.

Mean temperature of Nov. for twenty-seven previous years .	42°·67
Mean temperature of this month .....	42·05
Mean temperature of Nov. 1853 .....	44·87
Average quantity of rain in Nov. for thirteen previous years	4·25 inches.



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FEBRUARY 1855.

XIII. *On some Points of Magnetic Philosophy.*  
By Professor FARADAY, D.C.L., F.R.S. &c.\*

3300. **W**ITHIN the last three years I have been bold enough, though only as an experimentalist, to put forth new views of magnetic action in papers having for titles, "On Lines of Magnetic Force†," and "On Physical Lines of Magnetic Force‡." The first paper was simply an attempt to give, for the use of experimentalists and others, a correct expression of the dual nature, amount, and direction of the magnetic power both within and outside of magnets, apart from any assumption regarding the character of the source of the power; that the mind, in reasoning forward towards new developments and discoveries, might be free from the bondage and deleterious influence of assumptions of such a nature (3075. 3243.). The second paper was a speculation respecting the possible physical nature of the force, as existing outside of the magnet as well as within it, and within what are called magnetic bodies, and was expressly described as being entirely hypothetical in its character (3243.).

3301. There are at present two, or rather three general hypotheses of the physical nature of magnetic action. First, that of æthers, carrying with it the idea of fluxes or currents, and this Euler has set forth in a simple manner to the unmathematical philosopher in his Letters§;—in that hypothesis the magnetic

\* Communicated by the Author.

† Phil. Trans. 1852, p. 25.

‡ Phil. Mag. 1852, June, p. 401.

§ Euler's Letters, translated, 1802, vol. i. p. 214; vol. ii. pp. 240. 242. 244.

fluid or æther is supposed to move in streams through magnets, and also the space and substances around them. Then there is the hypothesis of two magnetic fluids, which being present in all magnetic bodies, and accumulated at the poles of a magnet, exert attractions and repulsions upon portions of both fluids at a distance, and so cause the attractions and repulsions of the distant bodies containing them. Lastly, there is the hypothesis of Ampère, which assumes the existence of electrical currents round the particles of magnets, which currents, acting at a distance upon other particles having like currents, arranges them in the masses to which they belong, and so renders such masses subject to the magnetic action. Each of these ideas is varied more or less by different philosophers, but the three distinct expressions of them which I have just given will suffice for my present purpose. My physico-hypothetical notion does not go so far in assumption as the second and third of these ideas, for it does not profess to say how the magnetic force is originated or sustained in a magnet; it falls in rather with the first view, yet does not assume so much. Accepting the magnet as a centre of power surrounded by lines of force, which, as representants of the power, are now justified by mathematical analysis (3302.), it views these lines as *physical* lines of power, essential both to the existence of the force within the magnet, and to its conveyance to, and exertion upon, magnetic bodies at a distance. Those who entertain in any degree the æther notion might consider these lines as currents, or progressive vibrations, or as stationary undulations, or as a state of tension. For many reasons they should be contemplated round a wire carrying an electric current, as well as when issuing from a magnetic pole.

3302. The attention of two very able men and eminent mathematicians has fallen upon my proposition to represent the magnetic power by lines of magnetic force; and it is to me a source of great gratification and much encouragement to find that they affirm the truthfulness and generality of the method of representation. Professor W. Thomson, in referring to a like view of lines of force applied to static electricity (1295. 1304.), and to Fourier's law of motion for heat, says that the lines of force give the same mathematical results as Coulomb's theory, and by more simple processes of analysis (if possible) than the latter\*; and afterwards refers to the "strict foundation for an analogy on which the *conducting power of a magnetic medium for lines of force* may be spoken of †." Van Rees has published a mathematical paper on my lines of force in Dutch ‡, which has been transferred

\* Phil. Mag. 1854, vol. viii. p. 53.

† Ibid. p. 56.

‡ Trans. Royal Acad. Sciences of Amsterdam, 1854, p. 17.



into Poggendorff's *Annalen*\*, and of which I have only a very imperfect knowledge by translated abstracts. He objects, as I understand, to what I may call the physical part of my view as assigning no origin for the lines, and as not presenting the higher principle conveyed by the idea of magnetic fluids or of electric currents: he says it does not displace the old theories, or render them superfluous; but I think I am right in believing, that, as far as the lines are taken to be representations of the power, he accepts them as correct representations, even to the full extent of the hypotheses, either of magnetic fluids or electric currents. It was always my intention to *avoid* substituting anything in place of these fluids or currents, that the mind might be delivered from the bondage of preconceived notions; but for those who desire an idea to rest upon, there is the old principle of the æthers.

3303. The encouragement I derive from this appreciation by mathematicians of the mode of figuring to one's self the magnetic forces by lines, emboldens me to dwell a little more upon the further point of the true but unknown natural magnetic action. Indeed, what we really want, is not a variety of different methods of representing the forces, but the one true physical signification of that which is rendered apparent to us by the phænomena, and the laws governing them. Of the two assumptions most usually entertained at present, magnetic fluids and electric currents, *one* must be wrong, perhaps *both* are; and I do not perceive that the mathematician, even though he may think that each contains a higher principle than any I have advanced, can tell the true from the false, or say that either is true. Neither of these views could have led the mind to the phænomena of diamagnetism, and I think not to the magnetic rotation of light; and I suppose that if the question of the possibility of diamagnetic phænomena could have been asked beforehand, a mathematician, guided by either hypothesis, must have denied that possibility. The notion that I have introduced complicates the matter still further, for it is inconsistent with either of the former views, so long as they depend exclusively upon action at a distance without intermediation; and yet in the form of lines of force it represents magnetic actions truly in all that is not hypothetical. So that there are now three fundamental notions, and *two* of them at least must be impossible, *i. e.* untrue.

3304. It is evident, therefore, that our physical views are very doubtful; and I think good would result from an endeavour to shake ourselves loose from such preconceptions as are contained in them, that we may contemplate for a time the force as much as possible in its purity. At present we cannot think of polarity

\* Poggendorff's *Annalen*, 1853, vol. xc. p. 415.

without feeling ourselves drawn into one or the other of the two hypotheses of the origin of polar powers; and as mathematical considerations cannot give a decision, we feel as if the subject were in that same doubtful condition which hung over the conflicting theories of light prior to the researches of modern time; but as there the use of Wheatstone's reflector, combined with Arago's suggestion of a decisive experiment, and its realization by Leon Foucault, appear to have settled that question, so we may hope by a due exertion of judgement, united with experiment, to obtain a resolution of the magnetic difficulty also.

3305. If we could tell the *disposition* of the force of a magnet, first at the place of its origin, and next in the space around, we should then have attained to a very important position in the pursuit of our subject; and if we could do that, assuming little or nothing, then we should be in the very best condition for carrying the pursuit further. Supposing that we imagine the magnet a sort of sun (as there is every reason to believe that the sun is a magnet) polarized, with antithetical powers, ever filling all space around it with its curved beams, as either the sun or a candle fills space with luminous rays; and supposing that such a view takes equal position with either of the two former views in representing truly the disposition of the forces, and that mathematical considerations cannot at present decide which of the three views is either above or inferior to its co-rivals; it surely becomes necessary that physical reasoning should be brought to bear upon the subject as largely as possible. For if there be such physical lines of magnetic force as correspond (in having a real existence) to the rays of light, it does not seem so very impossible for experiment to touch *them*; and it must be very important to obtain an answer to the inquiry respecting their existence, especially as the answer is likely enough to be in the affirmative. I therefore purpose, without asserting anything regarding the physical hypothesis of the magnet more strongly than before (3299.), to call the attention of experimenters, in a somewhat desultory manner, to the subject again, both as respects the deficiency of the present physical views and the possible existence of lines of physical force, concentrating the observations I may have to make about a few points—as *polarity*, *duality*, &c., as occasion may best serve; and I am encouraged to make this endeavour by the following considerations. 1. The confirmation by mathematicians of the truthfulness of the abstract lines of force in representing the direction and amount of the magnetic power;—2. My own personal advantageous use of the lines on numerous occasions (3174.);—3. The close analogy of the magnetic force and the other dual powers, either in the static or dynamic state, and especially of the magnet with

the voltaic battery or any other sustaining source of an electric current;—4. Euler's idea of magnetic æthers or circulating fluids;—5. The strong conviction expressed by Sir Isaac Newton, that even gravity cannot be carried on to produce a distant effect except by some interposed agent\* fulfilling the conditions of a physical line of force;—6. The example of the conflict and final experimental settlement of the two theories of light.

3306. I believe that the use by me of the phrase "places of force" has been considered by some as objectionable, inasmuch as it would seem to anticipate the decision that there are physical lines of force. I will endeavour so to use it, if necessary, as not to imply the assertion. Nevertheless I may observe, that we use such a phrase in relation to a ray of light, even in those parts of the ray where it is not extinguished, and where therefore we have no better knowledge of it or its existence than in similar magnetic cases; and we also use the phrase when speaking of gravity in respect of places where no second body to gravitate upon is present, and where, when existing, it cannot, according to our present views, cause the gravitating force of the primary body, or even the determination of it, upon that particular place.

#### *Magnetic polarity.*

3307. The meaning of this phrase is rapidly becoming more and more uncertain. In the ordinary view, polarity does not necessarily touch much upon the idea of lines of physical force; yet in the one natural truth it must either be essential to, and identified with it, or else absolutely incompatible with, and opposed to it. Coulomb's view makes polarity to depend upon the resultant in direction of the action of two separated and distant portions of two magnetic fluids upon other like separated portions, which are either originally separate, as in a magnet, or are induced to separate, as in soft iron, by the action of the dominant magnet;—it is essential to this hypothesis that the polarity force of one name should repel polarity force of the same name and attract that of the other name. Ampère's view of polarity is, that there are no magnetic fluids, but that closed currents of electricity can exist round particles of matter (or

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

round masses), and that the known experimental difference on the opposite sides of these currents, shown by attraction and repulsion of other currents, constitutes polarity. Ampère's view is modified (chiefly by addition) in various ways by Weber, De la Rive, Matteucci, and others. My view of polarity is founded upon the character in direction of the force itself, whatever the cause of that force may be, and asserts that when an electro-conducting body moving in a constant direction near or between bodies acting magnetically on themselves or each other, has a current in a constant direction produced in it, the magnetic polarity is the same; if the motion or the current be reversed, the contrary polarity is indicated. The indication is true either for the exterior or the interior of magnetic bodies whenever the electric current is produced, and depends upon the unknown but essential dual or antithetical nature of the force which we call magnetism (3154.).

3308. The numerous meanings of the term polarity, and various interpretations of polarity indications at present current, show the increasing uncertainty of the idea and the word itself. Some consider that the mere set or attraction, or even repulsion, shown by a body when subject to a dominant magnet is sufficient to mark polarity, and I think it is as good a test as any more refined arrangement (2693.) when the old notion of polarity only is under consideration. Others require that two bodies under the power of a dominant magnet should by their actions show a mutual relation to each other before they can be considered as polar. Tyndall, without meaning to include any idea of the nature of the magnetic force, takes his type from soft iron, and considers that any body presenting the like or the antithetical phenomena which such iron would present under magnetic action, is in a like or antithetical state of polarity\*. Thomson does not view two bodies which present these antithetical positions or phenomena as being necessarily the reverse of each other in what may be called their polar states†, but, I think, looks more to differential action, and in that approaches towards the views held generally by E. Becquerel and myself. Matteucci considers that the whole mass of the polar body ought to be in dependence by its particles as a mass of iron is, and that a solution of iron and certain salts of iron have not poles, properly speaking, but that at the nearest points to the dominant pole there is the contrary magnetism to that of the pole, surrounded by the same magnetism as of the pole in the further part, the two ends of a bar of such matter between two dominant poles having no rela-

\* Athenæum, No. 1406, p. 1203.

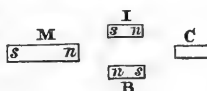
† Ibid. column 3 at bottom.

tion to each other\*. Becquerel considers that polarity may in certain cases occur transverse to the length, and so produce results which others explain by reverse polarity. The views of very many parties always include the idea of the source of the polar action, whether that be supposed to depend on the accumulation of magnetic fluids at the chief poles of the dominant magnet, or the action of electric currents in a determinate position around its molecules; and such views are adhered to even when the polarity induced is of the reverse kind, as in bismuth, &c., to that of the inducing magnet. Others, like Weber, add to Ampère's hypothesis an idea of electricity, loose as regards the particles, though inseparably associated with the mass of the body under induction. Some, I think, make the polarity not altogether dependent upon the dominant magnet, but upon the neighbouring or surrounding substances; and I propose, if the physical lines of force should hereafter be justified, to make that which is commonly called polarity, in distinction from the true polarity (3307.), dependent upon the better or worse magneto-conduction power of the substances presenting the usual polar phenomena (2818.).

3309. The views of polar action and of magnetism itself, as formerly entertained, have been powerfully agitated by the discovery of diamagnetism. I was soon driven from my first supposition, that the N pole of a magnet induced like or N polarity in the near part of a piece of bismuth or phosphorus; but as that view has been sustained by very eminent men, who tie up with it the existence of magnetic fluids or closed electric currents as the source of magnetic power, it claims continued examination, for it will most likely be a touchstone and developer of real scientific truth, whichever way the arguments may prevail. To me the idea appears to involve, if not magnetic impossibilities, at least great contradiction and much confusion, some of which I proceed to state, but only with the desire of elucidating the general subject.

3310. If an ordinary magnet M, fig. 1, acting upon a piece of iron or other paramagnetic matter I, renders it polar by throwing its near end into the contrary or S state in the manner usually understood, and, acting upon a like piece of diamagnetic matter as bismuth B, renders it also polar, but with the near end in the same state; then B and I are for the time two magnets, and must act back upon the magnet M; or if they could be made able to retain their states after M is removed (and that is the case with I), would act as magnets upon a third piece of

Fig. 1.



\* *Cours spécial sur l'induction, &c.*, p. 201.

magnetic matter as C. When M acts upon I, it exerts its influence, according to the received theories, upon all the particles of the latter, bringing them into like polar position with itself, and these, consistently with the simple assumption, act also upon each other as particle magnets, and exalt the polarity of the whole mass in its two extremities. In like manner M should act upon B, polarizing the mass and all its particles; for the particles of the diamagnetic body B, even to the smallest, must be operated upon; and we know experimentally, that a tube filled with powdered bismuth acts as a bar of the metal does. But then what is the mutual action of these bismuth particles on each other? for though all may be supposed to have a reverse polarity to that of M, they cannot in that case be reverse in respect of each other. All must have like polarity, and the N of one particle must be opposed to the S of the next particle in the polarity direction. That these particles act on each other, must be true, and Tyndall's results on the effect of compression have proved that by the right means, namely, experiment. If they were supposed to have no such action on each other, it would be in contradiction to the essential nature of magnetic action, and there would remain no reason to think that the magnet itself could act on the particles, or the particles react on it. If they acted on each other as the magnet is supposed to act on them, *i. e.* to induce contrary poles, then the power of the magnet would be nullified, and the more effectually the nearer the particles were together; whereas Tyndall has shown that the bismuth magnetic condition is exalted by such vicinity of the particles, and hence we have a further right to conclude that they do act on, or influence each other, to the exaltation of the state of the mass. But if the N-ness of one particle corresponds to, and aids in sustaining and exalting, the S-ness of the next particle, the whole mass must have the same kind of force; so that, as a magnet, its polarity must have the same kind of polarity as that of the particles themselves. For whether a particle of bismuth be considered as acting upon a neighbouring particle or upon a distant particle of bismuth, or whether a mass of particles be considered as acting on the distant particle, the action in both cases must be precisely of the same kind.

3311. But why should a polarized particle of bismuth acting upon another particle of bismuth produce in it like polarity, and with a particle of iron produce a contrary polarity? or why should masses of bismuth and iron, when they act as magnets (3310.), produce such different effects? If such were the case, then the N pole of a paramagnetic body would induce an S pole on the near end of an iron rod, whilst the N pole of a diamagnetic body would produce a pole contrary to the former, *i. e.* an

N pole at the same end of the iron rod in the same position and place. This would be to assume two kinds of magnetism, *i. e.* two north fluids (or electric currents) and two south; and the northness of bismuth would differ from the northness of iron as much as pole from pole. Still more, the northness of bismuth and the southness of iron would be found to have exactly like qualities in all points, and to differ in nothing but name; and the southness of bismuth and northness of iron would also prove to be absolutely alike. What is this, in fact, but to say they *are* the same? and why should we not accept the confirmation and unfailling proof that it is so, which is given to us experimentally by the moving wire? (3307. 3356.)

3312. If we employ a magnet as the originally inducing body (3310.), and entertain the idea of magnetic fluids accumulated at the poles, which act by their power of attracting each other, but repelling their like, then the inconsistency of supposing that the north fluid of a given pole can attract the north fluid of one body and the south fluid of another, or that the north and south fluids of the dominant magnet can attract one and the same fluid in bismuth and in iron, &c., is very manifest. Or if we act by a solenoid or a helix of copper wire carrying an electric current instead of a magnet, and find that analogous effects are produced, are we to admit at once that the electric currents in it, acting upon the assumed electric circuits round the particles of matter, sometimes attract them on the one side and sometimes on the other? or if such bodies as bismuth and platinum are put into such a helix, are we to allow that currents in opposite directions are induced in them by one and the same inducing condition? and that, too, when all the other phænomena, and there are many, point to a uniformity of action as to direction with a variation only in power.

#### *Media.*

3313. Let us now consider for a time the action of different *media*, and the evidence they give in respect of polarity. If a weak solution of protosulphate of iron\*, *m*, be put into a selected thin glass tube about an inch long, and one-third or one-fourth of an inch in diameter, and sealed up hermetically (2279.), and be then suspended horizontally between the magnetic poles in the air, it will point axially, and behave in other respects as iron; if, instead of air between the poles, a solution of the same kind as *m*, but a little stronger, *n*, be substituted, the solution in the tube will point equatorially, or as bismuth. A like solution somewhat weaker than *m*, to be called *l*, enclosed in a similar

\* Let *l* contain 4 grains, *m* 8 grains, *n* 16 grains, and *o* 32 grains, of crystallized protosulphate of iron in each cubic inch of water.

tube, will behave like bismuth in air but like iron in water. Now these are precisely the actions which have been attributed to polarity, and by which the assumed reverse polarities of paramagnetic and diamagnetic bodies have been considered as established; but when examined, how will ideas of polarity apply to these cases, or they to it? The solution *l* points and acts like bismuth in air and like iron in water; are we then to conclude that it has reverse polarity in these cases? and if so, what are the reasons and causes for such a singular contrast in that which must be considered as dependent upon its internal or molecular state?

3314. In the first place, no want of magnetic continuity of parts can have anything to do with the inversion of the phenomena; for it has been shown sufficiently by former experiments\*, that such solutions are as magnetically continuous in character as iron itself.

3315. In the next place, I think it is impossible to say that the medium interposed between the magnet and the suspended cylinder of fluid can cut off, or in any way affect the direct force of the former on the latter, so as to change the direction of its internal polarity. Let the tube be filled with the solution *m*, then if it be surrounded by the solution *l*, it will point as iron; if the stronger solution *n* surround it, it will point as bismuth; and with sufficient care a succession of these fluids may be arranged as indicated in figs. 2, 3, where the outlines between the poles represent the forms of thin glass troughs, and the letters the solutions in them.

In fig. 2 we see that the action on *m* is the same as that on *m'*, and the pointing of the two portions is the same, *i. e.* equatorial; neither has the action on *m* been altered by the power of the poles having to traverse *n*, *m'* and *n'*; and in fig. 3 we see, that, under like circumstances of the power, *m'* points as bismuth and *m* as iron, though they are the same solution with each other and with the former *m m'* solutions. No cutting off of power by the media could cause these changes;—repetitions of position in the first case, and inversions in the second. All that could be expected from any such interceptions would be perhaps diminutions of action, but not inversions of polarity; and every consideration indicates that all the portions of these solutions in the field at once have *like polarity*, *i. e.* like direction of force through them, and like internal condition;

Fig. 2.

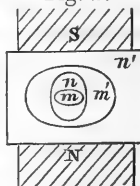
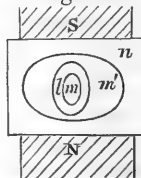


Fig. 3.



\* Phil. Mag. 1846, vol. xxix. p. 254.



each solution in its complex arrangement being affected exactly in the same way and degree as if it filled the whole of the magnetic field, although in these particular arrangements it sometimes points like iron, and at other times like bismuth (2362. 2414.).

3316. These motions and pointings of the same or of different solutions, contain every action and indication which is supposed to distinguish the contrary polarities of paramagnetic and diamagnetic bodies from each other, and the solutions *l* and *m* in air repeat exactly the phenomena presented in air by phosphorus and platinum, which are respectively diamagnetic and paramagnetic substances. But we know that these actions are due to the differential result of the masses of the moving or setting solution and of that (or the air) surrounding it. No structural or internal polarity, having opposite directions, is necessary to account for them (2361. 2757.). If, therefore, it is still said that the solution *m* has one polarity in *l* and the reverse polarity in *n*, that would be to make the polarity depend upon the mass of *m* independently of its particles; for it can hardly be supposed that the particles of *m* are more affected by the influence upon them of the surrounding medium (itself under like inductive action only, and almost insensible as a magnet), than they are by the dominant magnet\*. It would be also to make the polarity of *m* as much, or more, dependent upon the surrounding medium than upon the magnet itself;—and it would be, to make the masses of *m* and *l* and even their *form* the determining cause of the polarity; which would remove polarity altogether from dependence upon internal molecular condition, and, I think, destroy the last remains of the usual idea. For my own part, I cannot conceive that when a little sphere of *m* in the solution *l* is attracted upon the approach of a given magnetic pole, and repelled under the action of the same pole when it is in the solution *n*, its particles are in the two cases polar in two opposite directions; or that if for a north magnetic pole it is the *near* side of the particles of *m* when in *l* that assume the south state, it is the *further* side which acquires the same state when the solution *l* is changed for *n*. Nor can I think that when the particles of *m* have the same polar state in both solutions, the whole, as a mass, can have the opposite states.

3317. These differential results run on in one uninterrupted

\* If the polarity of the inner mass of solution is dependent upon that of the outer, and cannot be affected but through it, then why is not air and space admitted as being in effective magnetic relation to the bodies surrounded by them? How else could a distant body be acted upon by a magnet, if the inner solution of sulphate of iron is so acted on? Are we to assume one mode of action by contiguous masses or particles in one case, and another through distance in another case?

course from the extreme of paramagnetic bodies to the extreme of diamagnetic bodies; and there is no substance within the series which, in association with those on each side of it, may not be made to present in itself the appearances and action which are considered as indicating the opposite polarities of iron and bismuth. How then is their case, in the one or the other condition, to be distinguished from the assumed polarity conditions of bismuth or of iron?—only, I think, by assuming other points which beg the whole question. In the first place, it must be, or is assumed, that no magnetic force exists in the space around a magnet when it is in a vacuum, it being denied that the power either crosses or reaches a locality in that space until some material substance, as the bismuth or iron, is there. It is assumed that the space is in a state of magnetic darkness (3305.), an assumption so large, considering the knowledge we have of natural powers, and especially of dual forces, that there is none larger in any part of magnetic or electric science, and is the very point which of all others should be held in doubt and pursued by experimental investigation. It is as if one should say, there is no light or form of light in the space between the sun and the earth because that space is invisible to the eye. Newton himself durst not make a like assumption even in the case of gravitation (3305.); but most carefully guards himself and warns others against it, and Euler\* seems to follow him in this matter. Such an assumption, however, enables the parties who make it to dismiss the consideration of differential effects when bodies are placed in a vacuum, and to divide the bodies into the well known double series of paramagnetic and diamagnetic substances. But in the second place, even then, those who assume the reverse polarity of diamagnetic bodies, must assume also that the state set up in them by conduction is less favourable to either the exercise or the transmission of the magnetic force than the original unpolarized state of the bismuth; an assumption which is, I think, contrary to the natural action and final stable condition into which the physical forces tend to bring all bodies subject to them. That a magnet acting on a piece of iron should so determine and dispose of the forces as to make the magnet and iron mutually accordant in their action, I can conceive; but that it should throw the bismuth into a state which would make it repel the magnet, whereas if unaffected it should be so far favourable as to be at least indifferent, is what I cannot imagine to myself. In the third place, those who rest their ideas on *magnetic fluids*, must assume that in all diamagnetic cases, and in them only, the fundamental idea of their mutual action must not only be set aside but inverted, so that the hypothesis would be at war with

\* Letters, &c. translated. Letter LXVIII., or pp. 260–262.

itself; and those who assume that *electric currents* are the cause of magnetic effects, would have to give up the law of their inducing action (as far as we know it) in all cases of diamagnetism, at the very same moment when, if they approached the diamagnetic bismuth in the form of a spiral to the pole, they would have a current produced in it *according* to that law.

#### *Time.*

3318. I will venture another thought or two regarding the condition into which diamagnetic bodies are brought by the act of magnetic induction, in connexion with the point of *time*. It appears, as far as I remember, that all natural forces tend to produce a state of rest, except in cases where vital or organic powers are concerned; and that as in *life* the actions are for ever progressive, and have respect to a future rather than a present state (Paget), so all *inorganic* exertions of force tend to bring about a stable and permanent condition, having as the result a state of rest, *i. e.* a static condition of the powers.

3319. Applying this consideration to the case of bismuth in the magnetic field, it seems to me more like the truth of nature that the state assumed by the bismuth should be one more favourable to the final and static exercise of the power of the dominant magnet upon it, than that state belonging to the bismuth before it had suffered or undergone the induction; exactly as in soft iron we know that before it has acquired the state which a dominant magnet can induce upon it, it is not so favourable to the final static condition of the powers as it is afterwards. Now it is very manifest, by numerous forms of experiment, that *time* enters as an element into ordinary magnetic and magneto-electric actions, and there is every reason to expect into diamagnetic actions also; and it is also well known that we can take advantage of this time, and test the state of a piece of iron in the magnetic field before it has attained its finally induced state, and afterwards;—as, for instance, by placing it with a helix round it in the magnetic field and quickly connecting the helix *afterwards* with a galvanometer, when a current of electricity in such direction as to prove the truth of the statement will be obtained. In other forms of experiment, and with large pieces of iron, the time which can be so separated or snatched up during the act of progressive induction will amount to a minute or more. Supposing this could be done in any sensible degree with diamagnetic bodies, then the following considerations present themselves. A globe or bar of bismuth in the magnetic field may have its states, before and after induction, considered as separated by a moment of time; if the induction raises up a state of polarity the reverse of that of the magnet, then the bismuth

ought to be more favourable to the determination of magnetic force upon it *before* the induction than *after*; whereas if, according to my view, the polarity is not reversed, but is the same as that of the magnet, the metal ought to be more favourable to the determination of magnetic force upon or through it *after* induction than *before*. Believing this to be an experiment which would settle the question of reverse polarity, and perhaps the existence or non-existence of physical lines of magnetic force, I have made many attempts in various ways, and especially by alternating motions of cylinders and balls of bismuth between soft iron magnetic poles furnished with helices, to obtain some results due to the *time* of induction, but have been as yet unable to succeed. I cannot doubt that time is concerned; but it seems to be so brief in period as to be inappreciable by the means I have employed.

3320. Professor Thomson has put this matter of time and polarity in another form. If a globe of bismuth be placed without friction in the middle of the magnetic field, it will not point or move because of its shape; but if it have reverse polarity, it will be in a state of unstable equilibrium; and if *time* be an element, then the ball, being once moved on its axis ever so little, would then have its polarity inclined to the magnetic axis, and would go on revolving for ever, producing a perpetual motion. I do not see how this consequence can be avoided, and therefore cannot admit the principles on which it rests. The idea of a perpetual motion produced by static forces is philosophically illogical and impossible, and so I think is the polarly opposed or adverse static condition to which I have already referred.

3321. It is not necessary here that I should refer to the manner in which my view of the lines of magnetic force meet these cases, for it has been done in former papers (2797, &c.); but I will call the attention of those who like to pursue the subject, to a true case of reverse polarity in the magnetic field (Experimental Researches, 3238, fig. 15), and there they will easily see and comprehend the beginning of the rotation of Professor Thomson's bismuth globe, and its continuance, if, as supposed, the polar state represented in the figure could be continually renewed.

3322. When the north pole of a magnet repels a piece of bismuth in a vacuum, or makes a bar of it set equatorially, and is found to produce like actions with many paramagnetic bodies when surrounded by media a little more paramagnetic than themselves, and with as many diamagnetic bodies when surrounded by media a little less diamagnetic, it would seem more cautious in the first instance to inquire how these latter motions take place, and how it is that parts, which with the paramag-

netics have certainly been brought into a south condition by the north end of the pole, recede from it; and to apply these results in the first instance to those obtained with bismuth in a vacuum, before we assume a total change in principle, and yet an exceptional change as to substances, in the general law of magnetic polarity, without any cause assigned than, or any supporting facts beyond, the effect in question.

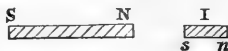
*Curved lines of magnetic force—dependence of the dualities.*

3323. The representative idea of lines of magnetic force which I entertain, includes in it the thought of the curvature of these lines, not as a merely convenient notion making the idea of the lines more manageable, but as one flowing from and suggested, if not proved, by the phenomena themselves. It is in this point of view that I proceed to consider it; and as the proof of the curvature is, in respect of *principle*, in the essential and necessary dependence of the two qualities or parts of a dual force upon each other (3324, &c.); and in respect of *experiment*, by the numerous results supplied during the mutual actions of magnets and magnetic bodies and the phenomena of moving conductors (3337, &c.), I will consider each in turn.

3324. There is no known case of one form or part of a dual power existing otherwise than with, and in dependence on, the other, which then exists simultaneously to an equivalent, *i. e.* an equal, degree. In static electricity, where supposed electric fluids are considered as being separated from each other, they are in equal amount (1177.), are ever related to each other (1681.), often by curved lines of force (1215.), and the existence of the one electricity without the other, or in the smallest degree of excess or deficiency, is absolutely impossible (1174.). In the voltaic battery, or in the electric current produced in any other way, as by thermo arrangements or inductions, the current in one part of the circuit is absolutely the same in amount and in dual character as in another; and in the insulated, unconnected voltaic battery, where the sustaining power is internal, not the slightest development of the forces, or of either of them, can occur until circuit is completed, or induction allowed at the extremities; for if, when there is no circuit, the induction be prevented, not merely no current, but no stock of electricity at the battery poles ready to produce a current can be evolved in the slightest degree. In like manner I am fully persuaded that the northness and southness of magnetism (in whatever they may be supposed to consist) cannot exist alone;—nor without exact proportion to each other;—nor without mutual dependence upon each other;—but that they are subject to the mutual relation and dependence of all dual force.

3325. Let us consider a hard invariable magnet in space, fig. 4. If a piece of soft iron, I, be brought towards it, the N end of the magnet will cause southness in the near end of the iron and northness in the further end, and this will continue until the iron is removed, the southness and northness at the two ends or halves of the magnet

Fig. 4.



having remained all the time unchanged in their equality and amount (3223. 3221.). Now to say that the force emanating from N could act on the iron, producing like and the contrary force, and then, by removal of the iron, cease to act there *or elsewhere*; and then again act on the iron if approached, or anything else, and then cease to act, and so on; would in my mind be to deny the *conservation of force*:—and we know that there is no equivalent action within the magnet, to explain by any alternate excitement and suppression of the dual parts, any supposed appearance and disappearance of the powers at the different times; for a helix closely applied round the middle part of the magnet during the experiment gives no current, and by that shows that there is no equivalent *internal* derangement of the power, when the outer exercise of it may be supposed to change between active and inert.

3326. Suppose the power of such a magnet to be due to magnetic N and S fluids; can it be thought that the N particles can be sometimes exerting their attraction for S particles, and sometimes not? Would not that be equivalent to the assumption of a suppression, *i. e.* a destruction of force?—which surely cannot be. Such an assumption could be surpassed only by that which supposes that the N fluid might sometimes attract S and repel N, and at other times repel S and attract N (3311. 3312. 3317.).

3327. As to the soft iron under induction (3325.), its dual magnetic forces do re-enter into their former mutually dependent and mutually satisfying state: but suppose it to be replaced by steel, and that the magnetisms produced in it do not recombine or disappear on the removal of the dominant magnet, then on what is their power ultimately turned, *if not on each other*? (3257. 3324.) Where is the S power of the steel disposed of when it is separated from its relation with the N power of the magnet that evolved it? The case cannot be met except by affirming the independent existence of the two powers (3329.); or, admitting the suppression of force, and of either of these forces the one without the other (3330.); or allowing the mutual dependence of the two polarities of the magnet (3331.).

3328. When the N pole of a magnet (fig. 5) is acting in

free space, its force is sensible around to a certain amount (114.); when a piece of soft iron, I, is brought near it, much of its force gathers up upon that iron, but the whole amount of force from and about the N pole is the same; when

Fig. 5.



an S pole is brought up, either of another magnet or of itself (for the effect is precisely the same), much of the force exerted upon the iron is removed from it, and falls upon the S pole, but the amount of force about the pole N remains the same; all of which can be proved experimentally by a helix on the soft iron and loops carried over the N pole (3218. 3223.). Indeed the way in which the power of one pole over either iron or bismuth is affected and diminished by the approximation on the same side of a contrary pole, is perfectly well known, and there are hundreds of cases in which the disposition in direction of the magnetic power can be varied in a great variety of ways, without the slightest change in the *sum* of its amount at the source, each of which gives evidence of the antithetical and inseparable condition of the two forms of force.

3329. As to independent existence of the two powers (3327.), how is it then that they cannot be shown separately? —not even up to the degree which is exhibited, so to say, by static electricity. There is nothing like a charge of northness or a charge of southness in any one of the innumerable phenomena presented by magnetism (3341.). The two are just as closely connected as the two electricities of a voltaic battery; whether we consider it as giving the current when properly connected, or exhibiting induction at its extremities when unconnected. The difficulty, indeed, is to find a fact which gives one the least hold for consideration of the thought that the two magnetic forces can be separated, or considered apart from each other.

3330. As to the suppression of force (3327.), I conceive that the creation, annihilation, or suppression of force, and still more emphatically of one form only of a dual force, is as impossible as the like of matter. All that is permitted under the general laws of nature is to displace, remove, and otherwise employ it; and these conditions are as true of the smallest suppression of a force, or part of a force, as of the suppression of the whole. I may further ask, whether, as it is *physically* impossible to annihilate or suppress force, it is not also *mathematically* impossible to do so, consistently with the law of the conservation of force?

3331. If we say that the forces in the cases of removal (3327.) are disposed of, sometimes in one direction and some-

times in another, but with the preservation of their full and equivalent amount, then how are we to consider them disposed of in the case of a cylinder or globular magnet, placed in air or *vacuo*, so as to be entirely self-dependent?—or in the case of a magnetic sphere placed in an inverted position in the magnetic field, so as to be entirely surrounded and enclosed by magnetic forces having a contrary direction to its own (3321. 3238.)?

3332. If we say that the dualities of such a magnet are dependent on each other (which is the third case (3327.)), then we have to consider how this can be, consistently with the distant mutual action, either of magnetic fluids or electric currents, acting in right lines only. Such action must then be through the body of the magnet (3260.). If we confine our attention to magnetic fluids, then the direction of their forces towards each other through the magnet when it is alone, must be of the like nature as their direction to approached iron, in which they are supposed to induce collections of the contrary fluids, or towards the fluids at the contrary poles of approached equal or superior magnets; *i. e.* the two poles of the magnet must be conceived of as centres of force, sometimes exerting their power towards each other in a given direction through the body of the magnet, and at other times exerting them outwardly to external poles in a direction exactly the contrary. But the currents which are evolved by the rotation of the magnet, or of discs of metal combined with it (3119. 3163.), show that the direction of the force (which is its polarity) is not thus reverse in the two halves of the case, but is the same within the magnet as in the prolongation of direction through and beyond the pole; and also, that whether the magnet be alone, and therefore supposed to have the polar forces exerted on each other through it, or be in relation to outer magnets, so as to have this exertion of force entirely removed from its interior, still it is always the same; having in both cases the same condition, direction, and amount of power within it (3116.).

3333. If the charged and polar state of the magnet be supposed to depend upon molecular electric currents, held by some internal condition in a position of parallelism, it is impossible that these can act backwards upon each other through the magnet in straight lines, so as to put the northness and southness of the pole in mutual dependence, as they are supposed to be in relation to external poles, without the currents themselves being *displaced and turned*, until the whole magnet is neutralized; falling back into the undeveloped state, just as a piece of soft iron falls back. When this return of state happens in soft iron or steel in any degree, a helix round these show the induced currents consequent on such a change; and a loop (3133. 3217.)



shows the difference when the iron or magnet is polar outwards and when its state has fallen. No such effects happen with a hard magnet, when it is alternately left to itself or put in relation to external poles of other magnets. The body of the magnet, and the forces passing through it, remain unchanged, whether examined by the loop (3223.) or by its own motion, and that of discs or wires associated with it (3116. &c.). Its force ever remains the same in quantity and general direction.

3334. The case of the steel ring magnet (3283.) is well known, and the manner in which such a magnet, showing no external relation, develops strong poles when it is broken. The phenomena assure us, I think, that when broken the northness and southness then appearing, cannot, when the pieces are by themselves, be determined upon each other backward through the magnet; there is no sufficient reason to suppose such a thing. And, again, the mutual destruction of highly-charged linear magnets, such as steel needles, when many of them are made into a thick, short bundle, shows the same thing; for if when alone the polar powers are not external, but are determined upon each other through each individual magnet, they are as free for a like disposal when the elementary magnets are associated as when they are separated:—and then there remains no sufficient reason to expect a dominant action over each other superior to that which each has over itself.

3335. It is not to be supposed that the change of force which occurs when the magnet first acting externally is then made to act internally or through itself, would be small and unnoticeable. It should be as great as the whole amount of power which the magnet can show under the most favourable circumstance; and the means are abundantly sufficient, by moving wires and discs, to make that evident in any case which might imply its passing through, or being removed out of, the magnet:—so that no difficulty can occur in that respect, and there remains, therefore, in my mind, but two suppositions; either the N polar force of a magnet when taken off from external compensating S polar force, is not exerted elsewhere as magnetic force at all; or else it is externally thrown upon and associated with the S polar force of the same magnet, and so sustained and disposed of, for the time, in its natural equivalent and essential state. If converted into any new form of power, what is that form? where is it disposed of? by what effects is it recognized? what are the proofs of its existence? To these inquiries there are no answers. But if it be directed externally upon the opposite S pole of the magnet, then all the consequences and foundations of my hypotheses of magnetic force and its polarity come forth; and, as I incline to believe, a consistent and satisfactory account

of all magnetic phænomena, short of the idea of the nature of the magnetic force itself, is supplied.

3336. For if the dual forces of the poles of a magnet in free space are related to, and dependent upon, each other, and yet not through the magnet (3331.), then it must be through the space around. Then space must have a real magnetic relation to the force passing across it, just as it has to the ray of light passing from an illuminating to an illuminated body. Then the directions in which the two forces are exerted upon each other cannot be in right lines, which must, if they existed, pass of necessity through the magnet; but in curved lines, seeing that it is impossible that any but curved lines can hold the poles in relation to each other through the surrounding space (3297.) :—and if they be curved lines, then I cannot imagine them to be anything else than physical lines of force; lines fitted to transfer the power onwards in consistency with its inevitable dual relation, and in conformity with that direction which ought, as I think, to be properly called *polarity*. And it further appears to me, that if we once admit the magnetic relation of a vacuum, then all the phænomena of paramagnetic and diamagnetic bodies; of differential polarity and individual polarity; of solutions, needles, crystals and moving conductors, are presented in a simple mutual relation, without any contradiction of fact or hypothesis, and in perfect harmony with each other.

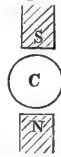
3337. I wish to avoid prolonging this paper by a repetition of the considerations and reasons already advanced on former occasions, and therefore will very briefly call to mind the idea I have put forth, that there are such lines of force in the space around a magnet; that the mutual dependence of the dualities, which is essential in the isolated magnet, is thus sustained; and that bodies in this space produce paramagnetic or diamagnetic phænomena, according as they favour or oppose the degree of sustaining power which mere space possesses. That these bodies, or *media* as they may be called, have a magnetic relation like that of space, is easily shown by numerous experimental results; but as they have a further relation amongst themselves, dependent upon their relative electro-conducting power, I think a little time may be usefully employed in considering how far the consequent results illustrate the probable condition of space where they are not present. Consider a magnet pole N, fig. 6, placed in relation to an equal magnetic pole S, so that their powers are mutually related and sustained, and the space between them, *a, a, a*, occupied by a vacuum, nitrogen, or some other gas at magnetic zero (2770. &c.) :—the force exerted by N on S, or

Fig. 6.



reciprocally, is easily taken cognizance of by spirals, &c. as regards any change in direction or degree. Then consider the medium *a, a, a* to be all copper or all mercury, still the forces are undisturbed: or consider it part mercury or copper, and part vacuum or glass, divided either by a line running from S to N, or along *a a a*, or any other way, still the forces are undisturbed; any of these media act exactly like space, or so like it, we can scarcely trace a difference. Then consider the metal moving, either as a finely divided stream at *a, a, a*, or as a solid globe (of copper) C, fig. 7, revolving rapidly round the line from N to S; still it is exactly like the vacuum or indifferent gas or glass, and there is no effect as yet by which we might distinguish the material medium from the mere space. But let the stream of metallic particles be converted into a continuous plate, and then we know it becomes filled with abundant currents of electricity; or if we apply the wires of a galvanometer to the revolving copper globe C, at the axial and equatorial parts, we can then cause it to develop (by permission of currents) a new effect, and the currents are sent out most abundantly by the conductors applied. If the copper globe C be rapidly revolved upon an axis perpendicular to the line S N, so strong and influential a medium is it, magnetically considered, that the two poles, N and S, if free to move, do move in the same direction as the near parts of the globe; and are absolutely carried away from each other, in opposition to their mutually attractive force, which tends strongly all the while to draw them together. Now, how is it possible to conceive that the copper or mercury could have this power in the moving state, if it had no relation at all to the magnetic force in the fixed state? or, that it should have like power in the compact state, and yet have no relation to the magnetic force in the divided and moving state? The mere addition of motion could do nothing, unless there were a prior static dependence of the magnet and the metal upon each other. We know very well that the actions in the moving cases involve the evolution, or a tendency to the evolution, of electric currents; but that knowledge is further proof that the metals are in prior relation to the magnetic forces; and as bodies, even down to aqueous solution, have these electric currents set up in them under like circumstances, we have full reason to believe that all bodies when in the magnetic field are in like static relation as the copper when not moving:—and that when motion is superadded, they would all evolve electric currents, were it not for their bad electro-conducting powers.

Fig. 7.

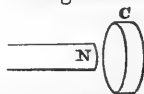


3338. These effects of motion are known to be identical with

those of the moving wire (36. 55.), or those of voltaic induction (6. &c.); and their intensity and power is very well shown in the force of Elkington's magneto-electric apparatus and Ruhmkorff's induction coil. *Time* is concerned in their production, and Professor Henry has shown us, in some degree, that when the currents are moving in helices, the magnetic action across them is for a time cut off or deflected (1730.). These actions are, in every case, simple; *i. e.* a line of force in a given polar direction produces, or tends to produce, in a body moving across it, whether paramagnetic, neutral, or diamagnetic (3146. 3162.), a current in the *like* direction; which current must, as I conceive, be dependent upon a previous like *static* state. Nothing in the slightest degree analogous to the supposed oppositely polar states of paramagnetic and diamagnetic bodies has ever been discovered amongst them;—and it has never been said, or supposed, as far as I know, that the two actions, *i. e.* the magnetic and the magneto-electric, are separate in their essential nature, or that they are not the consistent and accordant, and I must add reciprocal, actions of one force.

3339. That the copper, &c. are effectual as magnetic media when in the field, may be stated also thus:— Let N, fig. 8, be a magnetic pole, and C a thick disc or short cylinder of copper. If the copper revolve ever so rapidly on its axis, there will be no production of currents in it; and the magnetic action of N on other magnets will be the same, as if the metal were quiescent or even away. If N recede from C there are then currents in C, though it be not moving; and though the effect of N upon other magnets, as far as we know them, are unchanged; yet there is then a slight attraction between C and the N pole. If N be made to approach C, the reverse currents and actions occur. As N approaches or recedes more quickly or slowly, the currents produced, and consequent temporary magnetic state, are higher. A cylinder electro-magnet will show these effects very well. The copper has all the time been still, no motion has been purposely given to it; it has been affected by the approximation and recession of the pole, has passed from one state to another, which states remain stationary as long as the poles are quiescent, and it shows every character of a medium affected by the magnetic force. By expedients the currents in the copper may be allowed or prevented; but whether they be allowed or not, the state the copper medium arrives at is the same. If disallowed as the magnet approaches, but allowed as it recedes, then the current due to the last change occurs, an effect easily shown with a magnet and helix; and this seems to prove very distinctly that

Fig. 8.



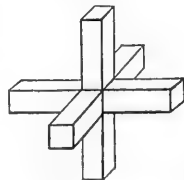
the copper within the constant influence of the magnet has a permanent, static, magnetic condition; and is therefore a magnetic medium, having lines of force passing through it. If C be of bismuth instead of copper, the same currents in the same direction occur, though in a far smaller degree; and, as it is believed, only because of deficiency in its conducting power.

3340. There can be no doubt that very much is involved in these phænomena, of the nature of which we have little or no knowledge; and the results obtained by Matteucci will probably lead to developments and discoveries of great importance. He states\* that copper, when finely divided, presents very persisting phænomena, proving its right to be considered as a diamagnetic body; but that when aggregated, all, or nearly all, its diamagnetic character disappears. Nothing is known as yet of the manner in which the mere difference of cohesion or division can so affect the diamagnetic character. He finds, too, that in other respects, as in Arago's rotation, particles of matter act in a manner not to be anticipated from what is at present known of them as masses; and it is to be hoped and expected that when these results are enlarged and developed, we shall be able to form a better judgement of the true physical action of magnetism than at present.

*Places of no magnetic action.*

3341. The essential relation and dependence of the two magnetic dualities is manifested, I think, in a very striking manner, by the results which occur when we attempt to isolate northness or southness, by concentrating either of them on one space or piece of matter, and looking for their presence by effects, either of tension or any other kind, whether connected with polarity or not. A soft iron bar, an inch square, 3 or 4 inches long and rounded at the edges, had thirty-two convolutions of covered copper wire 0.05 of an inch in diameter put round it, so that covering the middle part of the bar, chiefly, it could be shifted if needful a little nearer to one end than the other; such a bar could be rendered magnetic by an electric current passed through the wire, and a degree of adjustment, in the strength of the N and S extremities, could be effected by this motion of the iron in its helix. Having six of these, it was easy to arrange them with their like poles together, so as to include a cubical space or chamber, fig. 9; and in this space I worked by every means at my disposal. Access to it was easily obtained by a previous removal of a portion of the solid angles

Fig. 9.



\* *Cours spécial sur l'induction, &c.*, 1854. pp. 165, 269.

of the ends which were to be brought together, or by withdrawing the electro-magnets a little the one from the others, and then a ray of light could be passed into or across it; magnetic needles or crystals of bismuth could be suspended in it;—a ring helix could be introduced and rotated there; and the motions of anything within could be observed by the eye outside.

3342. A small magnetic needle hung in the middle of this space, gave no indication of any magnetic power; near the open edges and angles vibrations occurred, but they were as nothing compared to the powerful indications given outside the chamber; even when the needle was many inches away. A crystal of bismuth was entirely indifferent. A piece of soft iron hung on a jointed copper wire within the chamber showed no trace of magnetic power, whether examined by the little needle or in any other manner. Iron filings on a card across the chamber were not affected in the middle part, but only near the partly open angles. A ring helix of many convolutions, having its terminations passing out at opposite corners, was connected with a very sensitive galvanometer and rotated; it showed no trace of inductive action. Numerous other experiments were made, but with results altogether negative. Attempts (though desperate) were made to ascertain if any electro-chemical conditions were induced there, but in vain. Every kind of trial that I could think of, not merely by tests of a polar character, but of all sorts, were instituted, but with the same negative result.

3343. It was of course not to be expected that any polar, *i. e.* any dually related polar, action could be exerted in this place; but if the polarities can exist without mutual relation, we might surely expect some condition, some tonic or static state, in a chamber thus prepared and surrounded with a high intensity of magnetic power, acting in great concentration on one particular spot or substance. But it is not so; and the chamber offers a space destitute of magnetic action, and free, under the circumstances, from magnetic influence. It is the complete analogue of the space presented within a deep metallic vessel or globe\*, when charged with electricity (1174.). There is then no electricity within, because that necessary connexion and dependence of the electric duals, which is essential to their nature, cannot be. In like manner, there is no appearance of magnetic force in the cubical chamber, because the duals are not both there at once, and one cannot be present without the other.

3344. There are many ways of examining in a more or less perfect manner these neutral and highly instructive magnetic places. A cavity in the end of an electro-magnetic core or a permanent magnet will present similar phenomena, and in some respects even more perfectly; for though a trace of power will

\* Phil. Mag., Oct. 1846, vol. xxix. p. 257, note.

perhaps appear at the bottom of the cavity, the sum or amount, as compared to the sum of power at the end of the magnet, will show how complete the analogy between this space and the interior of a metallic vessel charged with positive or negative electricity is. A cylinder of soft iron, 9 inches in length and 1.6 in diameter, had a chamber 0.9 in diameter and 1 inch in depth formed in one extremity concentric with the cylinder; and being placed in a powerful helix of thick copper wire, and associated with a Grove's battery of ten pair of plates, was ready for experiment:—a like chambered magnet can be prepared by putting a proper iron ring against the end of any electro- or ordinary magnet, and will show the phænomena I am about to describe. A piece of soft iron, not more than 0.3 of an inch in length or thickness, held at the end of a copper wire and brought near the outer edge of the excited magnet pole, will be very strongly attracted; but if it be applied to the bottom of the chamber it will present no such effect, but be quite indifferent. If applied about the sides of the chamber, it will indicate no effect until it approaches the mouth. If the magnet be placed horizontally, and a piece of card-board be cut, so that it can enter the chamber and represent a horizontal section of its cavity; and, being sprinkled over with clean iron filings, is then put into its position and the magnet excited for a moment that it may develop its power over the chamber and filings and give them their indicative position; it will be found that only those near the mouth have been driven into a new position (about the outside angles of the pole), and that four-fifths of those upon the surface of the card within the chamber have been left unaffected, unmoved. If the chamber be filled with iron filings, closed with a card, placed in a vertical position with the aperture downward, and the magnet be then excited and the card removed, the filings will fall out; as they come out they will be caught away, and form a fine fringe round the external angles of the pole, but not one will remain at the bottom of the chamber, or even anywhere within the chamber, except near to its external edge. Yet, if a piece of iron long enough to reach out of the chamber, as a nail 2, 3, or 4 inches long, touch the *bottom* of the chamber, it is strongly attracted and held there, and will support a weight of several ounces, though prevented from touching the chamber anywhere else by a card with a hole in it placed over the mouth.

3345. If a small magnetic needle, about 0.1 of an inch long, be brought towards this excited magnet, it is almost unmanageable by reason of the force exerted upon it; but, as soon as it has entered the chamber, the power rapidly diminishes, and at the bottom the needle is scarcely, if at all, affected.

3346. If, instead of the core and chamber described, an iron tube of sufficient thickness of metal (as part of a gun-barrel) be employed, then like effects occur. If the magnetic needle be introduced, it ceases to be acted upon when about 1.5 inches within the tube. If the tube be more or less filled with iron filings, and then be excited and held vertical, they will all pour out and fall away, except those which are retained at the external edges. Yet, if a long nail be introduced, so as to be partly out of the cylinder, then it will be strongly attracted at the internal point, where it touches the iron of the tube core.

3347. The realization of like effects by grouping together the poles of ordinary magnets gives most interesting results. I have four very hard steel magnets, each 6 inches in length, 1 inch in breadth, and 0.4 nearly in thickness.

When the four like poles are put together, fig. 10, they form a flat square chamber in the same plane as that of the magnets. If a piece of stiff paper, the size of this chamber, be raised on a block 0.2 of an inch high, then sprinkled over with iron filings, and the magnets afterwards approached regularly until the square chamber is formed, a little tapping on the card will then arrange the filings in lines from the sides of the square chamber to the centre. The filings show at once the direction of the lines of force in this medium plane, and their greater abundance at the middle of each pole than at the re-entering angles; and if the filings be then removed and the indication of the course of the lines be followed out by a small magnetic needle, it will be found that the lines rise upwards from this plane above, and descend from it below, and then turn back upon their course in the free space over and beneath the arrangement towards the S poles of the different magnets. The condition will be understood in a moment, by considering the spondyloids of power belonging to each magnet (3271.), and the manner in which they are associated when the four like poles come together.

3348. When the magnets are turned edges upwards, they form a vertical chamber 1 inch high and only 0.4 of an inch in width, and now phenomena like those just described occur, but only near the entrances to the chamber; as the little needle proceeds into the enclosed space, the power of the magnets becomes less and less, and at the middle of the chamber scarcely a trace remains; that place being, like the closed chamber, formed with six poles (3341.),

Fig. 10.

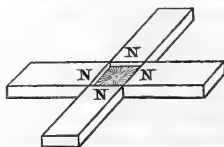
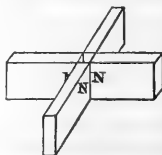


Fig. 11.



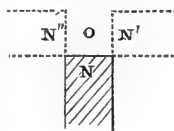


or like the bottom of a chamber formed in the end of a magnetic pole, a neutral place, or place of no magnetic action.

3349. The transition by degrees, from a pointed conical pole to an inclosed chamber, is, from the results described, very evident; and so also is their connexion with those belonging to the numerous neutral places produced under ordinary circumstances (3234. figs. 6, 10, 11, 15). Not the slightest difficulty or hesitation occurs, when these results are read or considered by the principle of representative lines of force; all the variations in the strength of the magnetic force and in the direction appear at once. But the great point is to observe how they all concur in showing the necessity of the complete and equivalent dual relation of the magnetic forces. When that is diminished or interfered with in any degree, in the same proportion does the power as a whole become diminished; until, at last, it absolutely disappears from a given place, though energies of the strongest kind are directing the force on to that spot, supposing that one of the dual elements could exist in any degree without, or independent of, the other.

3350. When formerly working with bismuth and magnets, I described several results (2298. 2487. 2491.) due to the principle of neutral magnetic places, more or less developed. If a sphere or cube of bismuth be delicately suspended by a vertical suspension or on a torsion balance, and an N pole be brought towards it, fig. 12, the bismuth will be repelled and the suspension deflected:—if a second N' pole be brought up, as in the figure, the bismuth will be less repelled by N than before, will return towards it, and N' will also seem to attract it, for on approaching the bismuth will tend to go into the angle formed by N and N'. If a third pole, N'', be brought up on the opposite side, the bismuth will then seem to be attracted by it, and by the first pole, and will, in fact, return very nearly into the position it would have if all the magnets were away. I thought at one time that magnetic structure, given by the second north pole N' to the bismuth, might produce the approximation of it to N, and if so that this would be neutralized by the action of a like pole N'' on the opposite side, and so the approximation of the bismuth (if due to such a cause) be prevented. On the contrary, however, such a pole *increased* it; and a moment's consideration, by showing that the three poles form a chamber of diminished or no action (3341. 3347.), shows also that such ought to be the case. All the movements of the bismuth are the result of the tendency which it has to pass from stronger to weaker places of magnetic action (2418.); and

Fig. 12.



in the present case they show *that* weakened place, which in a higher degree would be a place of no magnetic force.

*The moving conductor.*

3351. I wish to make a few further remarks (3336. 3337.) upon the value of the moving conductor, as a means of investigation in magnetical science. It will be sufficient to refer to former papers for a statement of the principles, the power, and the certainty of its indications (3156. 3172. 3176. 3270.). At present, I desire to apply it in a direct form of experiment, to the supposed reverse polarity of iron and bismuth (3309.).

3352. Four metallic spheres of copper, bismuth, soft iron, and hard steel, 0·8 of an inch in diameter, have been prepared; each has a copper axis carrying a small wooden pulley, so that when in its supporting frame, rotation, more or less rapid, can be given to it by the band of a multiplying wheel; each also has a thin copper ring driven tightly on to it at the equator, which, being grooved, serves to retain a galvanometer wire pressed against that part during the revolution of the globe; the other wire meanwhile being held against the copper axis. These globes, in their frame, could be placed one by one in the magnetic field of a powerful permanent Logeman magnet, so as to be subject to the magnetic force, fig. 13; and then rotated, and the currents of electricity induced in them carried to galvanometers. Two such instruments were employed, one, a Ruhmkorff's, with fine wire (2651.), the other with a thick wire of only four revolutions (3178.). The latter was the best, but both gave good indications. The position of all things concerned was preserved undisturbed during the experiments, so that it will not be necessary to do more than to describe a standard effect, and afterwards refer other effects to it. This standard may be taken from the current indicated when the *copper* globe was in the magnetic field; and it was such, when the upper part of the globe moved westward, as to send the south ends of the galvanometer needles to the west also: eight or ten revolutions of the globe would cause the needles to pass through  $80^\circ$  or  $90^\circ$ .

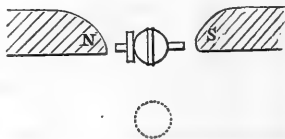


Fig. 13.

3353. The *soft iron* sphere was placed in the magnetic field; it was so good in character as to retain very slight traces of magnetism when taken out again. Being revolved, it gave a current of electricity, the same in direction as that of the

standard or copper ball. It is easy to understand, that if the globe be moved parallel to itself, but away from the magnet, in a line perpendicular to the magnetic axis (as into the dotted position, 3352. fig. 13), it will pass through places of weaker magnetic action. Under such changes of place, the induced current was weaker or stronger, according to the distance, but always in the same direction. Assuming that the rotating metal does give a true indication of the polarity or direction of the magnetic force (3077.), the results show that the polarity of the force which induces these currents, and which is the magnetic force of the dominant magnet, is the same both in the copper and in the iron. Other cases of the current from revolving iron may be referred to in the Exp. Res. (3162.).

3354. The *bismuth* globe was placed in the magnetic field. If made to revolve much, with the galvanometer wire pressing against the copper equator (3352.), the latter became warm by friction, and a permanent thermo-current was produced: this has been considered on a former occasion (3168.). Its effect is easily eliminated by revolving the globe a given number of times in opposite directions, observing the two deflections, adding them together, and taking the half of the sum for the amount of induced current in either one direction or the other; for as the thermo-current is added on the one side and subtracted on the other, such a process gives the real amount of the induced current. When, however, the bismuth sphere is revolved only five or ten times, the thermo-effect is so small as to make the galvanometer deflection very little more in one direction than in the other. When due attention was given, the rotation of the bismuth sphere produced an induced current in precisely the same direction as those obtained with the copper and iron; and so far, therefore, it indicated precisely the same direction of polarity for the magnetic force then acting upon and in it.

3355. The *hard steel* sphere, having been previously examined by a small needle and found to be unmagnetized, was placed in the magnetic field. It was then revolved, and gave an induced magneto-electric current in the *same* direction as the former currents. Being removed and again examined by the magnetic needle, it was found not to have received any sensible charge of magnetism.

3356. So these four metal globes indicate like polarity of the magnetic force, acting upon and within them, when examined thus by the magneto-electric current due to movement across the lines of force. By researches described elsewhere, it is known that all metals, and all bodies which are sufficiently electro-conductors, down even to aqueous fluids, give the same direction of

the magneto-electric current :—it is never reversed without reversion of the polarity, and reversion of the polarity *always* reverses the induced current.

3357. The hard steel sphere was now made a *magnet*, and though not of good shape to retain magnetism, being very short, yet it was able to sustain being placed in the magnetic field, in a position the reverse of the polarity of that field, and yet retain its polarity; for when taken out and examined by a magnetic needle, the polarity was found to be the same as before. Such being the case, it seemed to me that this magnet might be employed to represent, according to the view of those who conceive that iron and bismuth are polarized in opposite directions in the magnetic field, both *iron* and *bismuth*; inasmuch as it could be placed in the field in that condition of polarity, which these are then supposed respectively to acquire. The *globe magnet* was therefore placed in the magnetic field in a position conformable to that of the dominant magnet, *i. e.* with its N pole towards the S pole of the magnet, &c.; and being rotated, it gave an induced magneto-electric current like that of the standard and of iron (3352. 3353.). The dominant magnet was then withdrawn to a distance (3353.) and the globe rotated by itself; it gave, as it ought to do, the same current as before; for it, by its coercitive force, retains permanently that state of polarity, which the iron could receive only temporarily whilst in the magnetic field. Being now turned  $180^\circ$  in a horizontal direction, the globe magnet was reversed as regards the dominant magnet (the latter being, however, still at a distance), and now the globe magnet gave a current the *reverse* of the former, or of the standard current; and yet a very consistent current in relation to its own polarity.

3358. The dominant magnet was now gradually brought up, and its effect on the reversed globe magnet observed. The current from the latter became less and less, and at last was inverted, becoming like that of the standard current; nor can that be wondered at, when it is considered that the dominant magnet was the largest supplied by Logeman to the Great Exhibition, and able to sustain a weight of 430 pounds, and the sphere magnet only 0·8 of an inch in diameter, and very imperfectly hardened in the interior. But when the dominant magnet was withdrawn a little, a place was soon found for the globe magnet, where its rotation in either direction produced no current at all. Outside of this place, the rotated sphere gave a current, the reverse of that of the standard; whilst the iron and bismuth spheres in the same place, gave currents alike in kind and the same as that of the standard. In this region, therefore (and it is like the whole of the magnetic field of many inferior yet very

powerful magnets), if we represent bismuth by a magnet, reversely polar, as bismuth is supposed to be, we obtain induced magneto-electric currents, not like those of bismuth, but the contrary; and if we turn the representative magnet round, so as to give it the position in which it yields currents like those of the bismuth, then its polarity contradicts, or is the reverse of the assumed polarity of the bismuth.

3359. Now until the polarity or direction of the magnetic force which determines the course of the induced magneto-electric currents produced in every moving conductor, is distinguished and separated from the polarity or direction which causes movement amongst bodies subjected to the same force, how can these phænomena be accounted for by the supposition that the bismuth sphere is in the same polar condition as the reversed globe magnet? The reversed magnet is, in fact, the *contrary* to bismuth and to iron;—then bismuth and iron must be the same. The direct magnet is the *same* as the bismuth, in that polarity which induces a current;—then the magnet and the bismuth are the same. How easily all these effects present themselves in a consistent form, if read by the principle of representative lines of force. The reversed globe magnet at a distance from the dominant, shows, in revolving, the effect of the lines of force within it (3116.); as the magnet is approached, its external spondyloid of power is compressed inwards (3238. fig. 15), and at last the magnet is self-contained; then showing the equalization of its own powers, and as yet the absence from within it of any of the powers of the chief magnet; so that it gives no induced currents, though in a place where bismuth and iron would give them freely. Within that distance the effect of the superior and overpowering force of the great magnet appears (3358.), which, though it can take partial possession of the little magnet, still, when removed, suffers the force of the latter to develop itself again, and present the same series of phænomena as before.

3360. Van Rees admits, I believe, that the moving wire shows truly the presence, direction, and nature of the magnetic force or forces; and it is very important to know that the setting of a magnetic needle, or crystal of bismuth, and the production of a current of electricity in a moving conductor, are like correlative and consequent effects of the magnetic force; the power of producing one or the other effect being rigidly the same. Philosophers should either agree or differ distinctly on this point; so that if they differ, they may point out clearly the physical separation of the phænomena; which, if established, must lead to new and important discoveries. The polarity direction which the moving conductor makes manifest, whether that conductor

be one of the paramagnetic or diamagnetic bodies themselves, or whether it be a conductor moving amongst them, either by itself or with them, is *always the same*. The electric current produced never indicates a change in the direction of the polarity, from that belonging to the *first* source or seat of the power; whether it be a magnet, a solenoid, or of any other nature; the only difference being in the strength of the electric current produced, which difference is directly referable to the electro-conducting power (3143. 3152. 3163.). If such be the natural truth, how can the two modes of indication ever give opposite results? If opposite results seem to appear, and only occasionally, is it that mode of induction which gives one consistent result that we should doubt, or that which seems to be inconsistent with itself? especially when similar contrary phenomena in abundance are known to be produced by bodies having *like* polarity (3316.), and when excellent physical reasons, founded on differential action, offer themselves for their explication. There is sufficient reason to admit, that the magnetic needle cannot be always a true direct indicator of the amount or the direction of magnetic action (2868. 2870. 3156. 3293.). Should we not therefore, in respect of the above phenomena, rather conclude, for the time, that the simple and uniform results of the one mode of action, are the true indication; and that where, in the other mode, the phenomena are reversed or doubled, a part of them are *compound* in their nature? I may, in conclusion, remark, that the effects of motion and those produced in the action of magnetism on light, are never reversed in any case, whatever the medium in which they are observed; both point to one direction of polarity only, namely, that of the dominant source of magnetism.

3361. I will bring these imperfect observations to an end, by a very brief statement of what I suppose to be the condition of a magnet; and by a disclaimer, as to anything like conviction on all points of that which I set forth as a supposition tending to lead to inquiry. Contemplating a bar magnet by itself, I see in it a source of dual power. I believe its dualities are essentially related to each other, and cannot exist but by that relation. I think that though related through the magnet by sustaining power, they are not so related by discharging or inducing power, a power equal in amount to the coercitive or sustaining power. The relation externally appears to me to be through the space around the magnet; in which space a sphondyloid of power is present consisting of closed curves of magnetic force. That the space is not magnetically dark (3305.) appears to me by this; that when bodies occupy that space, having like relation by known phenomena to the power as the space has, as

copper, mercury, &c., they produce magneto-electric currents when moved. When bodies (media) occupy the space around the magnet, they modify its capability of transmitting and relating the dual forces of the magnet, and as they increase or diminish that capability, are paramagnetic or diamagnetic in their nature; giving rise to the phenomena which come under the term of magnetic conduction (2797.). The same magnet can hold different charges, as the medium connecting its poles varies; and so one, fully charged with a good medium, as iron between its poles, falls in power when the iron is replaced by air, or space, or bismuth. Corresponding effects occur with longer or shorter magnets (3290.), or with magnets made thick by adding many sideways together (3287.). The medium about a magnet may be mixed in its nature, and then more dual power is disposed of through the better conductor than the worse, but the whole amount of power remains unchanged. The powers and utility of the media, and of space itself, fail, if the dual force or polar action be interrupted. The magnet could not exist without a surrounding medium or space, and would be extinguished if deprived of it, and is extinguished, if the space be occupied adversely by the dual power of a dominant magnet of sufficient force. The polarity of each line of force is in the same direction throughout the whole of its closed course. Pointing in one direction or another, is a differential action due to the convergence or divergence of the lines of force upon the substance acted on, according as it is a better or a worse conductor of the magnetic force.

3362. But though such is my view, I put it forth with all the reservation made on former occasions (3244. 3299.). I do not pretend to explain all points of difficulty. I have no clear idea of the physical condition constituting the charged magnetic state; *i. e.* the state of the source of magnetic power:— or of the coercitivity by which that state is either resisted in its attainment, or sustained in its permanent condition; for the hypotheses as yet put forth give no satisfaction to my mind. I profess rather to point out the difficulties in the way of the views, which are at present somewhat too easily accepted, and to shake mens' minds from their habitual trust in them; for, next to developing and expounding, that appears to me the most useful and effectual way of really advancing the subject:—it is better to be aware, or even to suspect, we are wrong, than to be unconsciously or easily led to accept an error as right.

Royal Institution, 20 Dec. 1854.

XIV. *On the Action of the Violet and Ultra-violet Invisible Light.*  
By W. EISENLOHR\*.

THE phænomenon described by Stokes under the name *fluorescence*, led me to the supposition that this was caused by the interference of the shorter system of waves, blue-violet and ultra-violet (for the sake of shortness, the chemically-acting invisible rays of the spectrum may be so designated). I think, with many others, that the eye has the greatest sensibility for a certain duration of vibration (the yellow light), and that it is the more sensitive for longer or shorter waves, the more these differ from the medium light in their depth or height.

Light itself consists of the visible systems of waves, and besides these, of such as are longer than red and shorter than violet. As the combination of two tones is always deeper than each single one, out of which the compound tone arises, so from the interference of yellow and blue there can result only light of greater length of undulation, and not violet light. Now since red has the longest undulations of the visible light, the combination of red and yellow waves of light can only give a deeper tint than red, and consequently no visible light. A fluorescence in the dark space of the spectrum near the red is not therefore to be expected. It is quite otherwise at the other end of the spectrum. The ultra-violet is the light acting in the dark space of the spectrum near the violet; its existence could only be shown by its chemical action, before the wonderful discovery of Stokes. It consists of countless systems of undulations, the lengths of which, differing among themselves, have all a shorter duration than the violet light. Through their interference, waves of greater length than their own result; and by their great variety, tints of combination no less numerous; hence in many cases all kinds of visible light, or white.

In other cases a certain colour prevails in the mixture of the tints of combination, which will partly arise from the length of the original waves, and partly from the distance of the reflecting layers of atoms of the fluorescent body †.

Starting from this view, I have made experiments to find sources of light in which high tints prevail, in order to test this idea. Violet and blue glasses, through which the sunlight was admitted into the room by means of a heliostat, separating single parts of the entire spectrum from the rest, and causing

\* Poggendorff's *Annalen*, vol. xciii. p. 623.

† Of course the comparison between the tone of combination, and the light produced by various kinds of ultra-violet or other waves, must not be taken literally, for otherwise the number of vibrations of the resulting colour must be equal to the difference in the number of vibrations of the original rays.



the light thus obtained to penetrate into the fluorescent bodies, proved, at least, not the contrary of my supposition. I ascribe the cause of the partially slight success to the circumstance, that I possessed no blue and violet glasses of sufficient purity, which on that account allowed fewer of the more intensely acting rays to pass through. At last the violet light occurred to me, which results in the so-called electric egg when it is exhausted of air. I tried its action on fluorescent bodies, and was delighted to see that it produced some of the appearances described by Stokes, with a splendour which I have never seen with the most beautiful experiment by means of the spectrum. Paper on which a design had been made with a solution of sulphate of quinine, showed at a distance of ten to twelve feet from the oval receiver in the dark chamber, all the details of the design in the most beautiful white on a *deep* violet ground. Ruhmkorff's induction apparatus is extremely convenient for the production of the electric light in the receiver, when the latter is almost exhausted of air. The appearance is so striking, as to lead to the belief that the writing or design on the paper is itself shining and sparkling.

Hence, in my opinion, it follows,—1. That the violet light produced *in vacuo* is mixed with a large quantity of invisible ultra-violet rays. 2. That out of the ultra-violet rays of the so-called northern light invisible to the naked eye, there results by interference in fluorescent bodies a quantity of visible light, and that therefore this light, reflected from the surface of the paper which has been marked with quinine solution, appears brighter than direct light; that therefore, out of the ultra-violet light invisible to the naked eye, there is produced by mechanical means actual light. 3. That the so-called northern light has the strongest chemical action.

A further conclusion is, that the light *in vacuo* of Ruhmkorff's apparatus, or even that of the electric machine, is a much more powerful agent for testing the fluorescence of bodies than any hitherto employed.

I may here mention that I am still engaged on this subject, and reserve to myself further communications. The short time at my disposal compels me to limit myself to the statement, that a thick white glass in the dark chamber appears of a clear and splendid gray.

I scarcely doubt that the white *colour* of the electric light in air has also its explanation in the combination of the higher systems of waves, which are formed in consequence of the manifold reflexion on the atoms of air, and the consequent interference. Even the sun's rays are, according to Sondhaus, violet, and we see the sun, as it appears to us, only through a mixture of tints, whose production can be explained by the combination of the shorter systems of rays of the violet light.

XV. *On the Law of Absorption of Gases.* By R. BUNSEN\*.

[With a Plate.]

**G**ASEOUS bodies are absorbed by liquids on which they exert no chemical action, in quantities depending upon—

- 1st. The essential nature of the gas and of the absorbing liquid;
- 2nd. The temperature;
- 3rd. The pressure to which the gas is subjected.

The volume of gas, reduced to 0° and 0<sup>m</sup>.76 pressure of mercury, which is absorbed by the unit volume of a liquid, under the pressure of 0<sup>m</sup>.76 is called the *absorption-coefficient*, or *coefficient of absorption*. The value of this absorption-coefficient decreases in general with increase of temperature, in a ratio dependent upon the chemical nature of the absorbed gas and absorbing liquid. The values of the absorption-coefficients for varying temperatures can only be determined empirically. An exact relation exists between the volumes of absorbed gas† and the pressures under which the absorption takes place, the amounts of gas absorbed varying directly as the pressure.

The coefficient of absorption of any gas is therefore known when the following quantities are given:—1, the volume V, before the absorption, reduced to 0° and P pressure; 2nd, the volume V<sub>1</sub> remaining after the absorption reduced to 0° and P<sub>1</sub> pressure; and 3rd, the volume of the absorbing liquid. The amount of gas absorbed by the volume h<sub>1</sub> of liquid under the pressure P<sub>1</sub>, is equal to the difference between the volume of gas originally taken, and that remaining unabsorbed,

$$= \frac{VP}{0.76} - \frac{V_1P_1}{0.76}.$$

If the pressure during the absorption had not been P<sub>1</sub> but 0.76, the amount of gas absorbed would have been, according to the above law,

$$\frac{VP}{P_1} - V_1.$$

Hence it follows that the coefficient of absorption, *i. e.* the amount of gas absorbed in the unit volume of liquid under the pressure 0.76, is

$$\alpha = \frac{1}{h_1} \left( \frac{VP}{P_1} - V_1 \right). \quad . . . . . (1)$$

When the coefficient  $\alpha$  is known, the amount of gas *g* absorbed

\* Communicated by the Author. The Editors are indebted to Mr. Roscoe of Heidelberg for the translation of this memoir.

† The expressions “amount of gas” or “reduced volume,” are henceforward to be understood to signify the volume of gas reduced to 0° and 0<sup>m</sup>.76 pressure of mercury.

in  $h$  volumes of liquid under the pressure  $P$  is given by the equation

$$g = \frac{\alpha h P}{0.76} \dots \dots \dots (2)$$

If two or more gases are mixed together, the absorption of the constituent parts is proportional to the pressures to which these parts are severally subjected. Let the volumes  $v_1, v_2, v_n$ , fig. 4, of different gases, each under the pressure  $P$ , remain unmixed one above the other, separated by the diaphragms  $i i, i_1 i_1$ ; each of the gases exerts a pressure  $P$  against the inclosing diaphragm; withdraw the diaphragm and remove the resistance opposing the action of the pressure, and the particles of  $v_1$  will, in virtue of the pressure  $P$ , penetrate into the gases  $v_2, v_n$ , which offer no resistance. The motion of the particles of the gas  $v_1$  ends with a state of equilibrium, which ensues when the pressure exerted by  $v_1$  has become equally great at every point of the space  $v_1 + v_2 + v_n$ ; that is, when the gas  $v_1$  exerts a uniform tension throughout the total volume. The pressure on  $v_1$  is therefore, according to the law of Mariotte,

$$\frac{v_1}{v_1 + v_2 + v_n} P.$$

In like manner it is found that the gases  $v_2, v_n$ , when equilibrium has ensued, are subject to a pressure of

$$\frac{v_2}{v_1 + v_2 + v_n} P \text{ and } \frac{v_n}{v_1 + v_2 + v_n} P.$$

From these pressures of the constituent parts the total pressure of the mixture is found to be

$$P = \frac{v_1}{v_1 + v_2 + v_n} P + \frac{v_2}{v_1 + v_2 + v_n} P + \frac{v_n}{v_1 + v_2 + v_n} P.$$

The amount of each constituent gas absorbed is proportional to the pressure on that constituent part  $\frac{v_1}{v_1 + v_2 + v_n} P$ ; and these pressures may be distinguished as "partial pressures," in contradistinction to the "total pressures" on the whole mixture.

If a mixture of gas, unalterable by absorption, consists of two or more volumes of chemically different gases  $v_1, v_2, v_n$ , the amount of each gas dissolved in  $h$  volumes of liquid under the pressure  $P$ , when  $\alpha_1, \alpha_2, \alpha_n$  are the respective absorption-coefficients of the different gases at the observed temperature, is of the first gas,

$$\frac{\alpha_1 h P v_1}{0.76(v_1 + v_2 + v_n)}; \dots \dots \dots (3)$$

of the second gas,

$$\frac{\alpha_2 h P v_2}{0.76(v_1 + v_2 + v_n)}; \quad \dots \quad (4)$$

of the  $n$ th gas,

$$\frac{\alpha_n h P v_n}{0.76(v_1 + v_2 + v_n)}. \quad \dots \quad (5)$$

The unit volume of the absorbed gaseous mixture contains therefore, of the first gas,

$$u_1 = \frac{\alpha_1 v_1}{\alpha_1 v_1 + \alpha_2 v_2 + \alpha_n v_n}; \quad \dots \quad (6)$$

of the second,

$$u_2 = \frac{\alpha_2 v_2}{\alpha_1 v_1 + \alpha_2 v_2 + \alpha_n v_n}; \quad \dots \quad (7)$$

of the  $n$ th,

$$u_n = \frac{\alpha_n v_n}{\alpha_1 v_1 + \alpha_2 v_2 + \alpha_n v_n}. \quad \dots \quad (8)$$

If, on the contrary, the amounts of the separate gases  $u_1, u_2, u_n$  contained in the unit of absorbed gas are known, the composition of the gas before absorption, or of that remaining unabsorbed, is found from the following equations.

The unit of free gas contains of the first gas,

$$v_1 = \frac{\frac{u_1}{\alpha_1}}{\frac{u_1}{\alpha_1} + \frac{u_2}{\alpha_2} + \dots + \frac{u_n}{\alpha_n}}; \quad \dots \quad (9)$$

of the second,

$$v_2 = \frac{\frac{u_2}{\alpha_2}}{\frac{u_1}{\alpha_1} + \frac{u_2}{\alpha_2} + \dots + \frac{u_n}{\alpha_n}}; \quad \dots \quad (10)$$

of the third,

$$v_n = \frac{\frac{u_n}{\alpha_n}}{\frac{u_1}{\alpha_1} + \frac{u_2}{\alpha_2} + \dots + \frac{u_n}{\alpha_n}}. \quad \dots \quad (11)$$

These formulæ are only strictly true on the supposition that the relation originally existing between the volumes of the constituent gases is not appreciably disturbed in consequence of the absorption; they are therefore, accurately speaking, only applicable in the case in which a gaseous mixture of constant composition, either infinitely large or continually renewed, acts upon a finite volume of liquid. If the volume of liquid employed is

appreciable compared with the volume of gas, the alteration which the absorption causes in the composition of the unabsorbed gas must be brought into the calculation.

Let us next consider the alterations which a mixture of two gases undergoes by absorption, supposing that all the volumes of gas employed are reduced to 0°. Let the total volume of gas under the pressure P be V; in the unit volume of this gas let there be  $v$  volumes of the first gas, and  $v_1$  of the second. Let the absorption-coefficient of the first gas at the observed temperature be  $\alpha$ , and that of the second  $\beta$ , and the volume of absorbing liquid  $h$ . Further, let the total volume of the gas remaining after the absorption be  $V_1$  under the pressure  $P_1$ ; and lastly, let the unit volume of this residual gas contain  $u$  volumes of the first, and  $u_1$  volumes of the second gas.

The volume V contains  $vV$  volumes of the first gas at the pressure P, or  $\frac{vVP}{0.76}$  volumes at 0<sup>m</sup>.76. This volume is separated by absorption into two parts: the first part,  $x$ , remains behind after the absorption as free gas; the second,  $x_1$ , is that absorbed by the water. The amount of this latter is determined by the law of absorption; the unit of liquid absorbs the volume  $\alpha$  under the pressure 0<sup>m</sup>.76; hence under the pressure  $P_1$ ,  $h$  volumes of water will absorb

$$\frac{\alpha h P_1}{0.76}.$$

As, however, the first gas is expanded by mixture with the second from  $x$  to  $\frac{V_1 P_1}{0.76}$ , the amount of gas absorbed by  $h$  is, in consequence of the partial pressure,

$$\frac{\alpha h x}{V_1} = x_1.$$

Hence

$$x + \frac{\alpha h x}{V_1} = \frac{vVP}{0.76},$$

or

$$x = \frac{vVP}{0.76 \left(1 + \frac{\alpha h}{V_1}\right)};$$

and by similar reasoning, the volume of the second gas is

$$y = \frac{v_1 VP}{0.76 \left(1 + \frac{\beta h}{V_1}\right)}.$$

Hence when

$$vVP = A, \quad v_1VP = A_1, \quad \left(1 + \frac{\alpha h}{V_1}\right) = B, \quad \text{and} \quad \left(1 + \frac{\beta h}{V_1}\right) = B_1,$$

we obtain

$$\frac{AB_1}{(AB_1 + A_1B)} = \frac{x}{x+y} = u, \quad . . . . (12)$$

$$\frac{A_1B}{(AB_1 + A_1B)} = \frac{y}{x+y} = u_1. \quad . . . (13)$$

It is clear that, *vice versa*, the composition of an unknown gaseous mixture may be found from the change of volume ensuing from absorption by a liquid. In this way it is possible to analyse mixtures of gases by a purely physical experiment unassisted by chemical decomposition. Such absorptiometric determinations, as I term them, are, under certain conditions, scarcely less correct than a chemical analysis, often much more simple and convenient. Often, indeed, this mode of analysis is of immense importance, as solving questions which by other methods are not determinable.

Let us next consider the case in which two gases are given whose relation to each other is to be determined by an absorptiometric experiment.

Let  $x$  be the original volume of the first gas reduced to the pressure 1;

Let  $x'$  be the volume of the same gas unabsorbed, also reduced to the pressure 1;

Let  $v'$  be the volume of unabsorbed gaseous mixture at the pressure  $P'$ ;

The pressure on the unabsorbed gas 1 is then  $\frac{x'}{v'}$ . If the absorbed amount of the gas 1 be reduced to this pressure, the volume is  $\alpha h$ ; reduced to the pressure 1, it is therefore

$$\frac{x'}{v'} \alpha h,$$

and hence

$$x = x' + \frac{x'}{v'} \alpha h = x' \left(1 + \frac{\alpha h}{v'}\right),$$

or

$$x' = \frac{x}{1 + \frac{\alpha h}{v'}}.$$

Hence the pressure of the unabsorbed gas 1 is

$$\frac{x}{v' + \alpha h}.$$

If  $y$  and  $y'$  represent the same values for the gas 2 which  $x$  and  $x'$  did for the gas 1, the pressure of the unabsorbed gas 2 is

$$\frac{y}{v' + \beta h}$$

As  $P'$  is the pressure of the mixture, we get

$$P' = \frac{x}{v' + \alpha h} + \frac{y}{v' + \beta h}$$

If  $P$  is the pressure under which the mixture originally occupied the volume  $V$ , we have

$$P = \frac{x}{V} + \frac{y}{V}$$

(also obtained when  $h=0$ ). We have then

$$1 = \frac{x}{(v' + \alpha h)P'} + \frac{y}{(v' + \beta h)P'}$$

$$1 = \frac{x}{VP} + \frac{y}{VP}$$

If we place

$$\begin{aligned} VP &= W \\ (V' + \alpha h)P' &= A \\ (V' + \beta h)P' &= B, \end{aligned}$$

we obtain

$$\frac{x}{y} = \frac{W - B}{A - W} \cdot \frac{A}{B'}$$

or the volumes of the first and second gases in the unit volume of the mixture are

$$\frac{x}{x + y} = \frac{W - B}{A - B} \cdot \frac{A}{W} : \dots \dots \dots (14)$$

$$\frac{y}{x + y} = \frac{A - W}{A - B} \cdot \frac{B}{W} \dots \dots \dots (15)$$

For the case in which  $n$  gases are to be determined,  $n$  equations are required, easily obtained by observing for particular temperatures  $t, t_1, t_2 \dots t_{n-1}$ , the corresponding gaseous volumes  $V, V_1, V_2 \dots V_{n-1}$ , at various pressures  $P, P_1, P_2 \dots P_{n-1}$ , or for different volumes of liquid  $h, h_1, h_2 \dots h_{n-1}$ . Thus for a mixture of three gases whose volume is  $x + y + z$ , the following equations are obtained:—

$$1 = \frac{x}{VP} + \frac{y}{VP} + \frac{z}{VP}$$

$$1 = \frac{x}{(V_1 + \alpha_1 h_1)P_1} + \frac{y}{(V_1 + \beta_1 h_1)P_1} + \frac{z}{(V_1 + \gamma_1 h_1)P_1}$$

$$1 = \frac{x}{(V_2 + \alpha_2 h_2)P_2} + \frac{y}{(V_2 + \beta_2 h_2)P_2} + \frac{z}{(V_2 + \gamma_2 h_2)P_2}$$

If we substitute  $a, b, c$  for the coefficients  $\frac{1}{VP}$  in the first equation,  $a_1 b_1 c_1$  for  $\frac{1}{(V_1 + \alpha_1 h_1)P_1}$  in the second, and  $a_2 b_2 c_2$  for  $\frac{1}{(V_2 + \alpha_2 h_2)P_2}$  in the third, we obtain

$$\frac{x}{y} = \frac{A}{B} = \frac{b_1 c_2 - b_2 c_1 + b_2 c - b c_2 + b c_1 - b_1 c}{a c_2 - a c_1 + a_1 c - a_1 c_2 + a_2 c_1 - a_2 c}$$

$$\frac{z}{y} = \frac{C}{B} = \frac{a b_1 - a b_2 + a_1 b_2 - a_1 b + a_2 b - a_2 b_1}{a c_2 - a c_1 + a_1 c - a_1 c_2 + a_2 c_1 - a_2 c}$$

or

$$\frac{y}{x+y+z} = \frac{B}{A+B+C} \dots \dots \dots (16)$$

$$\frac{x}{x+y+z} = \frac{A}{A+B+C} \dots \dots \dots (17)$$

$$\frac{z}{x+y+z} = \frac{C}{A+B+C} \dots \dots \dots (18)$$

The foregoing formulæ are founded upon the well-known hypothesis of Dalton and Henry, which, however, is not confirmed by the extremely inaccurate experiments hitherto made upon the subject. In order to verify them, it is necessary in the first place to obtain an exact determination of the coefficients of absorption; for this purpose I use an absorptiometer of the following construction:—

The absorption-tube,  $e e$ , Plate I. fig. 1, divided into millimetres and calibrated, has a small iron band  $b$ , furnished with a screw luted on to its lower and open end; this fits into another screw attached to the small iron stand  $a a$ , fig. 2. By this arrangement the open end of the tube can be screwed down against a plate of caoutchouc covering the lower surface of the stand, and the tube thus hermetically closed. On each side of the stand are fixed two steel springs  $c c$ , which fit into two upright grooves inside the wooden foot of the apparatus  $f$ , fig. 1, so that the stand can be either raised or depressed, but not turned on its axis. It is thus easy, when the tube and stand are in their places, to open or close the absorption-tube by giving it a very slight motion on its axis to the right or left. The outer cylinder  $g g$ , fig. 1, surrounding the absorption-tube is not luted either into the wooden foot  $f$ , or into the iron rim  $h$ , but the screws  $i i$  press the ground-glass edges of the cylinder against the caoutchouc rings. The tubes  $r r$  serve to pour in mercury, so that any wished-for pressure is obtained in the absorption-tube by raising or depressing the column of mercury in the inner glass cylinder. The temperature of the surrounding water is deter-



mined by the small thermometer *c*. The upper end of the outer cylinder is closed by an iron lid having a hinge at one side, and fastened down by means of a nut and screw attached to the iron rim *h*, fitting into a small slit in the side of the lid. In the middle of the cover inside there is a raised rim of iron, over which a thick sheet of caoutchouc is extended and fastened by a screwed ring *s*. This distended caoutchouc serves as a spring against which the top of the tube can be pressed, keeping it in a fixed position during the violent agitation necessary in the process of absorption.

The experiment itself is conducted in the following manner:— A volume of the gas to be examined is first collected in the tube over mercury, and the usual precautions taken in reading off, &c., as in the reduction and measurement of gases. A measured volume of water freed from air is next admitted under the mercury into the tube, which is then screwed tightly against the caoutchouc plate, and the tube thus closed placed in the cylinder containing some mercury, and over that a quantity of water. As soon as the pressure within and without has been equalised by slightly turning the tube, it is again closed, and the whole apparatus rapidly agitated for about a minute. This agitation, with opening and closing of the tube, is continued many times, until no further change of volume is perceptible. The observations necessary for the measurement and reduction of the residual gas are then made. Besides the temperature *t* and the barometric pressure *p*, four readings from the divided tube are required:—

1. The lower surface of mercury in the outer cylinder at *a*.
2. The upper surface of mercury in the absorption-tube *b*.
3. The upper surface of water in the absorption-tube at *c*.
4. The upper surface of water in the outer cylinder at *d*.

The method of calculation will be best explained by an example. I select for this purpose an experiment on the determination of the absorption-coefficient of nitrogen and water at 19° C.

*Elements of the Calculation.*

1. *Observations before the Absorption.*

Lower surface of mercury in outer cylinder	<i>a</i> = 423·6 millims.
Upper surface of mercury in tube*	<i>b</i> = 124·1 ...
Barometric pressure . . . . .	<i>p</i> = 746·9 ...
Temperature of the absorptiometer . . . . .	<i>t</i> = 19°·2 C.
Temperature of the barometer . . . . .	<i>τ</i> = 19°·0 ...

\* In this experiment the absorptiometer contained only mercury, and no water.

2. Observations after the Absorption.

Lower surface of mercury in outer cylinder	$a_1 = 352.2$ mm.
Upper surface of mercury in tube of absorption	$b_1 = 350.7$ ...
Upper surface of water in tube	$c_1 = 65.5$ ..
Upper surface of water in outer cylinder	$d_1 = 8.0$ ...
Barometric pressure	$p_1 = 746.3$ ...
Temperature of the absorptiometer	$t_1 = 19.0$ C.
Temperature of the barometer	$\tau_1 = 18.9$ C.

Reduction of these Elements.

1. Before the Absorption.

	mm.
Surface of mercury at	$a = 423.6$
Surface of mercury at	$b = 124.1$
Column of mercury in the absorption-tube	$a - b = 299.5$
Ditto reduced to $0^\circ$	$\pi = 298.5$
The barometric pressure ( $p = 0.7469$ ) reduced to $0^\circ$	$\pi_1 = 744.4$
The pressure of the gas reduced to $0^\circ$	$\pi_1 - \pi = 445.9$
Tension of vapour of water at $19.2$ to be subtracted	$= 16.6$
Pressure of the dry nitrogen	$P = 429.3$
The volume of gas corrected from the calibration	} $= 34.90$
read off at $b = 124.1$ at $19.2$ C	
Ditto reduced to $0^\circ$	$V = 32.608$

2. After the Absorption.

	mm.
Barometric pressure $p_1 = 746.3$ reduced to $0^\circ$	$\delta = 743.8$
Surface of mercury at	$a_1 = 352.2$
Surface of mercury at	$b_1 = 350.7$
Column of mercury in tube at $19.0$ C	$a_1 - b_1 = 1.5$
Ditto at $0^\circ$ C.	$\delta_1 = 1.5$
Lower surface of water in tube at	$b_1 = 350.7$
Upper surface of water in tube at	$c_1 = 65.5$
Column of water in the tube	$= (b_1 - c_1)w = 285.2$
Lower surface of water in outer cylinder	$a_1 = 352.2$
Upper surface of water in outer cylinder	$d_1 = 8.0$
Column of water in the outer cylinder ( $a_1 - d_1$ )	$w_1 = 344.2$
The column of water ( $w_1 - w$ ) acting opposite to	} $= 59.0$
the barometer	
Ditto reduced to pressure of mercury	$q = 4.4$
The pressure reduced to $0^\circ$ C	$(\delta - \delta_1 - q) = 746.8$
Tension of watery vapour at $19.0$ to be subtracted	$= 16.3$
Pressure of the unabsorbed nitrogen	$P_1 = 730.5$
The residual volume of gas corrected from cali-	} $= 17.67$
bration read off at division $c_1 = 65.5$	
Ditto reduced to $0^\circ$	$V_1 = 16.522$
The corresponding volume to division $b_1 = 350.7$	$= 200.04$
Volume of absorbing liquid	$h_1 = 182.37$

The value of the absorption-coefficient, as obtained from these elements by equation No. 1, is

$$a = \frac{1}{h_1} \left( \frac{VP}{P_1} - V_1 \right) = 0.01448.$$

The accuracy of these determinations depends in a great measure upon the water employed being perfectly free from air. This is best accomplished by boiling the water briskly for several hours, and then causing it to pass while still boiling into a bottle, the neck of which has been drawn out to a fine point before the blowpipe. The water is then again boiled for half an hour in the bottle, and the end of the tube hermetically sealed, access of air being prevented during the closing by pressing a caoutchouc tube attached to the extremity. When the water thus freed from air, which should strike against the glass like a water-hammer, is required for experiment, the end of the drawn-out tube moistened with solution of corrosive sublimate is broken under mercury, and the water admitted directly into the absorption-tube. Before admitting the water, care must be taken that no bubble of air appears in the bottle, which would show imperfect boiling. The following coefficients of absorption have been thus determined from experiments made in my laboratory, chiefly by Dr. Pauli\*.

### 1. Nitrogen in Water.

The gas was prepared by passing dry ammonia and air, freed from carbonic acid, over glowing copper turnings.

No.	° C.	Coefficient found.	Coefficient from formula (19).	Difference.
1	at 4.0	0.01843	0.01837	-0.00006
2	6.2	0.01751	0.01737	-0.00014
3	12.6	0.01520	0.01533	+0.00013
4	17.7	0.01436	0.01430	-0.00006
5	23.7	0.01392	0.01384	-0.00008

By combination of the experiments 1, 2, 3; 2, 3, 4; and 3, 4, 5, we obtain the interpolation formula,

$$c = 0.20346 - 0.00053887t + 0.000011156t^2. \quad (19)$$

By the help of this formula the following Table is calculated:—

\* This talented young chemist, who most zealously assisted me in the experimental part of this research, has since, to my deep regret, been removed by death from science and from his friends.

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0.02035	0.00054	7	0.01713	0.00039	14	0.01500	0.00023
1	0.01981	0.00049	8	0.01675	0.00038	15	0.01478	0.00022
2	0.01932	0.00048	9	0.01640	0.00035	16	0.01458	0.00020
3	0.01884	0.00046	10	0.01607	0.00033	17	0.01441	0.00017
4	0.01838	0.00044	11	0.01577	0.00030	18	0.01426	0.00015
5	0.01794	0.00042	12	0.01549	0.00028	19	0.01413	0.00013
6	0.01752		13	0.01523	0.00026	20	0.01403	0.00010

### 2. Hydrogen in Water.

The gas was prepared from dilute sulphuric acid and pure zinc.

No.	° C.	Coefficient.	Difference from the mean.
1	at 4.0	0.0185	-0.0008
2	7.0	0.0205	+0.0012
3	9.6	0.0196	+0.0003
4	12.8	0.0186	-0.0007
5	15.5	0.0197	+0.0003
6	18.8	0.0188	-0.0005
7	23.6	0.0194	+0.0001

From these experiments it is seen that the mean coefficient of absorption 0.0193 of hydrogen is constant for temperatures from 0° to 20°.

### 3. Æthyle Gas in Water.

For these experiments a quantity of the same æthyle gas was employed, which Professor Frankland prepared in my laboratory some years ago, and of which he gave the analysis in his research on the æther radicals.

No.	° C.	Coefficient found.	Coefficient calculated from (20).	Difference.
1	at 5.8	0.02637	0.02626	+0.0011
2	8.7	0.02393	0.02428	-0.0035
3	14.0	0.02199	0.02175	+0.0024
4	17.2	0.02103	0.02092	+0.0011
5	21.8	0.02026	0.02061	-0.0035

By combination of the experiments 1, 2, 3; 2, 3, 4; and 3, 4, 5, the following interpolation formula is found,

$$c = 0.031474 - 0.0010449t + 0.000025066t^2, \quad (20)$$

by means of which the following Table is calculated:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0.03147	0.00102	7	0.02541	0.00072	14	0.02179	0.00037
1	0.03045	0.00098	8	0.02474	0.00067	15	0.02147	0.00032
2	0.02947	0.00091	9	0.02412	0.00062	16	0.02121	0.00026
3	0.02856	0.00086	10	0.02355	0.00057	17	0.02100	0.00021
4	0.02770	0.00081	11	0.02303	0.00052	18	0.02084	0.00016
5	0.02689	0.00076	12	0.02257	0.00046	19	0.02073	0.00011
6	0.02613		13	0.02216	0.00041	20	0.02065	0.00008

4. Carbonic Oxide in Water.

The gas was prepared by heating sulphuric acid with pure formate of magnesia; treatment with a ball of potash showed it to be perfectly pure.

No.	° C.	Coefficient found.	Coefficient from formula (21).	Difference.
1	at 5.8	0.028636	0.028691	-0.000055
2	8.6	0.027125	0.027069	+0.000056
3	9.0	0.026855	0.026857	-0.000002
4	17.4	0.023854	0.023642	+0.000212
5	18.4	0.023147	0.023414	-0.000267
6	22.0	0.022907	0.022863	+0.000044

If the mean value from 1, 2, 3, from 2, 3, 4, 5, and from 4, 5, 6, be taken for the calculation of the constants, we get the following formula:—

$$c = 0.032874 - 0.00081632 t + 0.000016421 t^2. \dots (21)$$

This gives for the temperature from 0° to 20°,—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0.032874	0.000800	7	0.027964	0.000603	14	0.024664	0.000373
1	0.032074	0.000767	8	0.027394	0.000570	15	0.024324	0.000340
2	0.031307	0.000734	9	0.026857	0.000537	16	0.024017	0.000307
3	0.030573	0.000701	10	0.026353	0.000504	17	0.023743	0.000274
4	0.029872	0.000669	11	0.025882	0.000471	18	0.023501	0.000242
5	0.029203	0.000636	12	0.025443	0.000439	19	0.023292	0.000209
6	0.028567		13	0.025037	0.000406	20	0.023116	0.000176

5. Protocarburetted Hydrogen in Water.

I have used for this determination a gas, preserved in hermetically-closed tubes, which is found in the mud-volcanoes of Bulganack in the Crimea, where it occurs under similar circumstances as at Baku on the Caspian Sea. This gas was employed because it appeared from my researches that it is the purest which occurs in nature. By treatment with a potash ball, it was freed from a trace of carbonic acid, and it contained, as the following analysis shows, neither nitrogen, oxygen nor olefiant gas:—

	Vol.	° C.	Pressure.	Vol. at 0° and 1 m.
Original volume of gas .....	127.6	4.8	0.1596	20.01
After admission of air .....	499.0	4.8	0.5151	252.60
After admission of oxygen .....	537.4	4.8	0.5500	290.47
After the explosion .....	495.4	4.5	0.5115	249.29
After absorption of carbonic acid...	466.2	4.6	0.4994	228.97
After addition of hydrogen .....	609.3	4.3	0.6284	376.95
After the explosion .....	478.8	4.3	0.5105	240.64

	Found.	Calculated.
Gas employed . . .	20.01	20.45
Carbonic acid formed	20.32	20.45
Contraction . . .	41.18	40.90
Oxygen consumed . .	41.18	40.90

This gas gave the following values:—

No.	° C.	Coefficient found.	Coefficient from formula (22).	Difference.
1	6.2	0.04742	0.04757	-0.00015
2	9.4	0.04451	0.04430	+0.00021
3	12.5	0.04126	0.04134	-0.00008
4	18.7	0.03586	0.03600	-0.00014
5	25.6	0.03121	0.03100	+0.00021

The mean from 1, 2, 3, from 2, 3, 4, and from 3, 4, 5, gives the interpolation formula,

$$c = 0.05449 - 0.0011807 t + 0.000010278 t^2, \dots (22)$$

from which the following Table is calculated:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0.05449	0.00117	7	0.04674	0.00104	14	0.03997	0.00091
1	0.05332	0.00115	8	0.04571	0.00103	15	0.03909	0.00088
2	0.05217	0.00113	9	0.04470	0.00101	16	0.03823	0.00086
3	0.05104	0.00111	10	0.04372	0.00098	17	0.03739	0.00084
4	0.04993	0.00108	11	0.04275	0.00097	18	0.03657	0.00082
5	0.04885	0.00107	12	0.04180	0.00095	19	0.03577	0.00080
6	0.04778		13	0.04088	0.00092	20	0.03499	0.00078

### 6. Methylene Gas in Water.

For this experiment, a specimen of methylene gas, sealed up in a glass tube, was used, which Professor Frankland prepared from iodide of methylene, and analysed some years ago at Marburg:—

No.	° C.	Coefficient found.	Coefficient from formula (23).	Difference.
1	4.6	0.072884	0.073084	-0.000200
2	7.8	0.064732	0.064839	-0.000107
3	12.1	0.055788	0.055703	+0.000085
4	15.2	0.050722	0.050500	+0.000222
5	19.8	0.045715	0.044915	+0.000800
6	24.2	0.040817	0.041960	-0.001143

If the arithmetical mean from 1, 2, 3, 4, from 3, 4, 5, and 4, 5, 6, are used for the conditional equations for the interpolation formula, we obtain,

$$c = 0.0871 - 0.0033242 t + 0.0000603 t^2, \dots (23)$$

and from this the coefficients between the temperatures of 0° and 20° are,—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0.0871	0.0033	7	0.0668	0.0025	14	0.0524	0.0017
1	0.0838	0.0031	8	0.0644	0.0024	15	0.0508	0.0016
2	0.0807	0.0030	9	0.0621	0.0023	16	0.0493	0.0015
3	0.0777	0.0029	10	0.0599	0.0022	17	0.0480	0.0013
4	0.0748	0.0028	11	0.0578	0.0021	18	0.0468	0.0012
5	0.0720	0.0027	12	0.0559	0.0019	19	0.0457	0.0011
6	0.0693		13	0.0541	0.0018	20	0.0447	0.0010

### 7. Olefant Gas in Water.

The gas was prepared with the well-known precautions from alcohol and sulphuric acid. To free it from the vapours of æther and alcohol, and from traces of other hydrocarbons polymeric with clayle, a ball of coke, saturated with concentrated but not fuming sulphuric acid, was placed in the gas until nearly the half of it was absorbed; a ball of potash was afterwards introduced, and left in contact with the gas for some time. Eudiometrical analysis proved the purity of the gas:—

No.	° C.	Coefficient found.	Coefficient from formula (24).	Difference.
1	4.6	0.21870	0.21824	+0.00046
2	9.6	0.18398	0.18592	-0.00194
3	14.0	0.16673	0.16525	+0.00148
4	18.0	0.15324	0.15278	+0.00046
5	20.6	0.14597	0.14791	-0.00194

The interpolation formula is obtained from combination of 1, 2, 3 and 2, 3, 4 and 3, 4, 5,

$$c = 0.25629 - 0.00913631 t + 0.000188108 t^2, \dots (24)$$

and hence the coefficients between 0° and 20°:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0.2563	0.0090	7	0.2018	0.0064	14	0.1652	0.0041
1	0.2473	0.0085	8	0.1952	0.0066	15	0.1615	0.0037
2	0.2388	0.0082	9	0.1893	0.0059	16	0.1583	0.0032
3	0.2306	0.0079	10	0.1837	0.0056	17	0.1553	0.0030
4	0.2227	0.0074	11	0.1786	0.0051	18	0.1528	0.0025
5	0.2153	0.0071	12	0.1737	0.0049	19	0.1506	0.0022
6	0.2082		13	0.1693	0.0044	20	0.1488	0.0018

8. Carbonic Acid in Water.

This gas was prepared by the action of strong sulphuric acid upon chalk, a few drops of water being added to the mixture. The gas, thus steadily evolving, was washed by being passed through boiled water. The experiment gave,—

No.	° C.	Coefficient found.	Coefficient from formula (25).	Difference.
1	4.4	1.4698	1.4584	+0.0114
2	8.4	1.2426	1.2607	-0.0181
3	13.8	1.0654	1.0385	+0.0269
4	16.6	0.9692	0.9610	+0.0082
5	19.1	0.8963	0.9134	-0.0171
6	22.4	0.8642	0.8825	-0.0183

The three conditional equations from the mean 1, 2, 3, 4 and 2, 3, 4 and 3, 4, 5, 6, give the interpolation formula,

$$c = 1.7967 - 0.07761 t + 0.0016424 t^2, \dots \dots (25)$$

and from this the following coefficients are found:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	1.7967	0.0760	7	1.3339	0.0562	14	1.0321	0.0332
1	1.7207	0.0726	8	1.2809	0.0530	15	1.0020	0.0301
2	1.6481	0.0694	9	1.2311	0.0498	16	0.9753	0.0267
3	1.5787	0.0661	10	1.1847	0.0464	17	0.9519	0.0234
4	1.5126	0.0629	11	1.1416	0.0431	18	0.9318	0.0201
5	1.4497	0.0596	12	1.1018	0.0398	19	0.9150	0.0168
6	1.3901		13	1.0653	0.0365	20	0.9014	0.0136

[To be continued.]

XVI. On the Eccentricity of the Moon's Orbit.

By Professor CHALLIS\*.

IT is a satisfactory result of the discussion relating to the moon's orbit which has recently appeared in this Magazine, that it has drawn attention to the general question of apsidal motion, which is one that is by no means exhausted. In the Number for last January, Mr. Thacker has solved a problem of this class by a method which shows the advantage of employing for the purpose the first integrals of the differential equations of the motion. I have used an analogous method to determine the motion of the apses of the moon's orbit. In the problem above mentioned, the force is supposed to be wholly central, and to be expressed by the function  $\frac{\mu}{r^2} + \mu'r$ , and the solution leads

\* Communicated by the Author.



to the remarkable result, that the motion of the apses is uniform *whatever be the eccentricity of the orbit*. I have no grounds for concluding that the author is of opinion that any argument can be deduced from this result against the theorem I have advanced respecting the moon's orbit, viz. that the uniformity of the progression of the lunar apses depends on a certain value of the eccentricity; but as such an argument appears *primâ facie* to be deducible, I have undertaken to show that this is not really the case. For this purpose I propose to solve the problem by a method somewhat different from that employed by Mr. Thacker.

The force is wholly central, and equal to  $\frac{\mu}{r^2} - \mu'r$ , the second term is supposed to be always small compared to the other, and only the first power of  $\mu'$  is retained. From the usual differential equations of the motion, viz.

$$\frac{d^2x}{dt^2} + \frac{\mu x}{r^3} - \mu'x = 0, \quad \frac{d^2y}{dt^2} + \frac{\mu y}{r^3} - \mu'y = 0,$$

may be derived, after substituting  $r \cos \theta$  for  $x$ , and  $r \sin \theta$  for  $y$ , the following :

$$\frac{d^2r}{dt^2} - r \frac{d\theta^2}{dt^2} + \frac{\mu}{r^2} - \mu'r = 0, \quad \dots \dots \dots (1)$$

$$\frac{d\theta}{dt} = \frac{h}{r^2}, \quad \dots \dots \dots (2)$$

$$\frac{d^2r}{dt^2} - \frac{h^2}{r^3} + \frac{\mu}{r^2} - \mu'r = 0. \quad \dots \dots \dots (3)$$

Multiplying (3) by  $2dr$  and integrating,

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \mu'r^2 + C = 0. \quad \dots \dots \dots (C)$$

Hence, eliminating  $dt$  by (2), we have

$$d\theta = \frac{hdr}{r \sqrt{-Cr^2 + 2\mu r - h^2 + \mu'r^4}}.$$

As the integral of this equation can only be obtained approximately, and the approximation is to proceed according to the powers of  $\mu'$ , we get by expanding to the first power of this quantity,

$$d\theta = \frac{hdr}{r(-Cr^2 + 2\mu r - h^2)^{\frac{1}{2}}} - \frac{\mu'h}{2} \cdot \frac{r^3 dr}{(-Cr^2 + 2\mu r - h^2)^{\frac{3}{2}}}.$$

This equation being integrated by the usual rules, gives

$$\theta + \gamma = \cos^{-1} \frac{h^2 - \mu r}{r \sqrt{\mu^2 - h^2 C}} + \frac{3\mu\mu'h}{2C^{\frac{5}{2}}} \cos^{-1} \frac{Cr}{\sqrt{\mu^2 - h^2 C}} - \frac{\mu'h}{2\sqrt{R}} \left\{ -\frac{r^2}{C} + \frac{2(h^2 C - 3\mu^2)(h^2 - \mu r)}{C^2(\mu^2 - h^2 C)} + \frac{3h^2\mu(\mu - Cr)}{C^2(\mu^2 - h^2 C)} \right\},$$

where R is put for  $-Cr^2 + 2\mu r - h^2$ . Let  $a(1+e)$  and  $a(1-e)$  be apsidal distances which satisfy the equation

$$-Cr^2 + 2\mu r - h^2 + \mu'r^4 = 0. \quad \dots \quad (A)$$

Since the square of  $\mu'$  is not retained, it will suffice to employ in those terms of the foregoing equation which involve  $\mu'$ , the values of  $a$  and  $e$  given by the solution of the approximate equation,  $-Cr^2 + 2\mu r - h^2 = 0$ , that is, to suppose the approximation to commence from a fixed ellipse. Consequently,

$$a = \frac{\mu}{C}, \quad ae = \frac{1}{C} \sqrt{\mu^2 - h^2 C},$$

and

$$\cos^{-1} \frac{\mu - Cr}{\sqrt{\mu^2 - h^2 C}} = \cos^{-1} \frac{a - r}{ae}.$$

Let this arc =  $\phi$ . Then the above equation between  $r$  and  $\theta$  gives to the same approximation,

$$\begin{aligned} \cos\left(\theta + \gamma - \frac{3\mu\mu'h}{2C^{\frac{5}{2}}}\phi\right) &= \frac{h^2 - \mu r}{r \sqrt{\mu^2 - h^2 C}} \\ + \frac{h^2\mu'}{2r \sqrt{\mu^2 - h^2 C}} \cdot \left\{ -\frac{r^2}{C} + \frac{2(h^2 C - 3\mu^2)(h^2 - \mu r)}{C^2(\mu^2 - h^2 C)} + \frac{3h^2\mu(\mu - Cr)}{C^2(\mu^2 - h^2 C)} \right\}. \end{aligned}$$

Now since  $r = \frac{1}{C}(\mu - \sqrt{\mu^2 - h^2 C} \cos \phi)$  nearly, it follows that

$$r^2 = 2\mu r - h^2 - \frac{\mu^2 - h^2 C}{C^2} \sin^2 \phi.$$

After substituting this value of  $r^2$ , the equation may be arranged as follows:

$$\begin{aligned} \cos\left(\theta + \gamma - \frac{3\mu\mu'h}{2C^{\frac{5}{2}}}\phi\right) &= \frac{h^2}{\sqrt{\mu^2 - h^2 C}} \cdot \left\{ 1 + \frac{h^2\mu'}{2C^2} \cdot \frac{h^2 C - 2\mu^2}{\mu^2 - h^2 C} \right\} \cdot \frac{1}{r} \\ &\quad - \frac{\mu}{\sqrt{\mu^2 - h^2 C}} \cdot \left\{ 1 + \frac{h^2\mu'}{2C^2} \cdot \frac{3h^2 C - 4\mu^2}{\mu^2 - h^2 C} \right\} \\ &\quad + \frac{h^2\mu'}{2C^3} \cdot \sqrt{\mu^2 - h^2 C} \cdot \frac{\sin^2 \phi}{r}. \end{aligned}$$

The values of the arbitrary constants  $h^2$  and  $C$ , derived in terms of the constants  $a$  and  $e$  from the apsidal equation (A), are

$$C = \frac{\mu}{a} + 2\mu'a^2(1+e^2), \quad h^2 = \mu a(1-e^2) - \mu'a^4(1-e^2)^2.$$

If these be substituted in the foregoing equation, the result to the same approximation is,

$$\cos\left(\theta + \gamma - \frac{3\mu'a^3 \sqrt{1-e^2}}{2\mu}\phi\right) = \frac{a(1-e^2)}{er} - \frac{1}{e} + \frac{\mu'a^4 e(1-e^2)}{2\mu} \cdot \frac{\sin^2 \phi}{r}.$$

Let

$$\cos \psi = \frac{a(1-e^2)}{er} - \frac{1}{e},$$

so that

$$\frac{\sin \phi}{r} = \frac{\sin \psi}{a \sqrt{1-e^2}}.$$

Hence

$$\begin{aligned} \cos \left( \theta + \gamma - \frac{3\mu'a^3 \sqrt{1-e^2}}{2\mu} \phi \right) &= \cos \psi + \frac{\mu'a^3 e \sqrt{1-e^2}}{2\mu} \sin \phi \sin \psi \\ &= \cos \left( \psi - \frac{\mu'a^3 e \sqrt{1-e^2}}{2\mu} \sin \phi \right) \text{ nearly;} \\ \therefore \theta &= \psi + \frac{\mu'a^3 \sqrt{1-e^2}}{2\mu} (3\phi - e \sin \phi), \end{aligned}$$

$\theta$  being supposed to commence with  $\psi$  and  $\phi$ .

It is not necessary for my purpose to pursue the reasoning further. The equation last obtained, which is identical with the principal result of Mr. Thacker's solution, sufficiently proves that the two methods are the same in principle, and lead to the same inferences respecting the motion of the apsides. It is, however, important to remark, that my method shows clearly that the analytical reasoning may legitimately commence from a *fixed ellipse*. Let us now consider the bearing this inference has on the lunar theory.

It is well known that in the lunar theory the analysis fails to give the true development of the moon's radius-vector and true longitude, if the approximation commences from a fixed ellipse. What is the reason that the process fails in this instance, while it succeeds in that just considered? I reply, that the different conditions of the lunar problem entirely account for this difference. In the first place, the force in the lunar theory is not wholly central, and the equation (C) is consequently only approximate. Next, there is introduced into the lunar theory a limitation, according to which the moon's true longitude and radius-vector can never differ much from mean values\*. In consequence of this limitation, which is essential to the subsequent treatment of the problem, the eccentricity of the moon's orbit is always small. Lastly, in the lunar theory it is necessary to conduct the approximation, not only according to the disturbing force, but according to the eccentricity also. The failure just spoken of

\* In Mr. Airy's Lunar Theory (Mathematical Tracts, 3rd edition) this limitation is introduced where it is said, in page 29, that "for  $\theta'$  we shall put the value which it would have if the motions of the sun and moon were both uniform."

proves that, under *these* circumstances, it is not legitimate to commence from a fixed ellipse.

But from independent analytical principles (as illustrated in the case already considered), it is certain that, under *any* circumstances, it would be legitimate to commence from a fixed ellipse, if the aggregate of the terms  $h^2 - 2\mu r + Cr^2$  be of a higher order of magnitude, estimated according to the disturbing force, than the term  $-\mu'r^4$ . It is clear that they cannot be of a lower order.

It follows, therefore, inevitably, that the aggregate of the terms  $h^2 - 2\mu r + Cr^2$  is of the *same* order of magnitude with respect to the disturbing force as the term  $-\mu'r^4$ ; in other words, *that they contain the disturbing force as a factor*. It is not difficult to point out the origin of this analytical circumstance. The integration by which the constant C is introduced (see Phil. Mag. for April 1854, p. 279), may be effected by assuming only that the increment in a given time of the sun's true longitude is small compared to that of the moon's. The remarkable integral I have obtained in the Supplement to the Philosophical Magazine for December 1854 (p. 521), shows that even that assumption is not necessary for effecting an integration. The limitation that the moon's true longitude differs little from a mean value, is made *subsequently* to that integration. Hence the constant C possesses greater generality than comports with the conditions of the problem, and must itself be subject to some limitation. Clearly, therefore, the reasoning will be complete if, by means of the constant C, the condition that  $h^2 - 2\mu r + Cr^2$  contains the disturbing force as a factor can be fulfilled. But this may be readily done as follows.

Let  $h^2C = \mu^2$ , and  $r = \frac{\mu}{C} + fv$ . Then

$$\begin{aligned} h^2 - 2\mu r + Cr^2 - \mu'r^4 &= C\left(r - \frac{\mu}{C}\right)^2 - \mu'r^4 \\ &= Cf^2v^2 - \mu'\left(\frac{\mu}{C} + fv\right)^4. \end{aligned}$$

Hence the required condition is satisfied if  $h^2C = \mu^2$ , the unknown quantity  $f$  being determined by the equation  $f^2 = \mu'$ .

This point being settled, it remains to indicate the process of approximation that must be adopted. Resuming the equation

$$\frac{r^2 dr^2}{dt^2} = -h^2 + 2\mu r - Cr^2 + \mu'r^4, \quad . . . \quad (C)$$

it is evident, since  $-h^2 + 2\mu r - Cr^2$  contains  $\mu'$  as a factor, that the approximation must proceed according to the eccentricity, and not according to the disturbing force. For the same reason,

the equation which gives the apsidal distances must be solved as a biquadratic, the term  $\mu'r^4$  being of equal significance with the others. Since, therefore, from the foregoing reasoning, it appears that  $r$  is nearly equal to  $\frac{\mu}{C}$ , and the apsidal distances by hypothesis differ little from each other, it follows from the theory of equations, that to obtain their approximate values,  $\frac{\mu}{C} + \left(r - \frac{\mu}{C}\right)$  is to be substituted for  $r$  in the last term of the equation

$$-h^2 + 2\mu r - Cr^2 + \mu'r^4 = 0,$$

and then, after expanding to the *second* power of  $r - \frac{\mu}{C}$ , the equation is to be solved as a quadratic. The same process of substitution must for the same reason be gone through to prepare the equation (C) for approximate integration. I need not pursue the investigation further, as the results of the subsequent steps are given in the *Philosophical Magazine* for April 1854, p. 279, and in the *Supplement to the Philosophical Magazine* for December 1854, p. 526. By far the most important result is, that the eccentricity of the moon's orbit has a special value depending on the disturbing force.

The following considerations appear to me proper for proving that, whatever be the law of the disturbing force, and whether it be central or not, the motion of the apse is uniform if the eccentricity be a function of the disturbing force. Let the undisturbed orbit be an ellipse described about the focus, and let the three bodies be always in a given plane, the central body having a fixed position. A straight line being drawn from the centre of the fixed body in any direction in the plane of motion, the radius-vector of the disturbed body at the instants it passes this line has different values in successive revolutions. The change of value in each complete revolution depends on the disturbing force, in such a manner, that the function by which it is expressed would vanish if the disturbing force vanished. The total fluctuations, in the case of a uniform apsidal motion, are the same in all directions; and the difference between the extreme values of the radius-vector in any given direction depends both on the partial fluctuations, and on the rate of the angular motion of the apses. Hence as the partial fluctuations, as well as the motion of the apses, would vanish with the disturbing force, it follows that the difference between the extreme values, and consequently the eccentricity of an orbit nearly circular, is a function of the disturbing force.

I adduced an argument similar to the above in the *Philosophical Magazine* for April 1854 (p. 282), which I subsequently

rejected (*Phil. Mag.* for August 1854, p. 108), because it appeared to lead to the inference that the eccentricity is simply proportional to the disturbing force. But by taking account, as is done above, of the rate of angular motion of the apse, this inference by no means follows, and the eccentricity is still a function of the disturbing force, although the form of it may be discoverable only by analytical investigation.

It is particularly to be remarked, that the above general reasoning only proves that, whatever be the law of the disturbing force, and whether it be central or not, for a certain value of the eccentricity the motion of the apse will be uniform. But it does not prove, that, under particular conditions, for instance, when the disturbing force is wholly central, and the law of it is given, uniform motion of the apse may not be consistent with any value of the eccentricity. In a similar manner, the general theorem, that every mass, whatever be its form, has three axes of permanent rotation, is not invalidated by finding that masses of particular forms have an unlimited number of axes of permanent rotation. Hence the fact, that when the central force is  $\frac{\mu}{r^2} + \frac{\mu'}{r^3}$ ,

the motion of the apse is uniform whatever be the eccentricity, is no argument against the general proposition, unless it should appear that in this instance the eccentricity can under no circumstances be a function of the disturbing force. This, however, is not the case; for the equation which gives the apsidal distances for that law of force is

$$h^2 - \mu' - 2\mu r + Cr^2 = 0,$$

from which it follows that

$$e^2 = 1 - \frac{h^2 C}{\mu^2} + \frac{\mu' C}{\mu^2},$$

and consequently that  $e$  is a function of  $\mu'$  if  $h^2 C = \mu^2$ .

I think that I have now established the truth, in all essential particulars, of the new views respecting the moon's orbit, which were first published in the *Philosophical Magazine* for April 1854; and that I have pointed out a step towards a great simplification of the lunar theory. I have not yet had time to carry the method through the third approximation.

Cambridge Observatory,  
January 11, 1855.

P.S. In the reasoning of the foregoing article it has been assumed, in accordance with the view generally taken by mathematicians, that analysis fails to give the true developments of the moon's radius-vector and longitude when the approximation

commences from a fixed ellipse. Since the article was written, I have ascertained that no failure occurs *on that account* in the approximations to the first and second orders of small quantities, the results being so far the same, whether the approximation commences from a fixed ellipse, or is conducted by the process which I have shown to be necessary if  $\frac{dr^2}{dt^2}$  contain the disturbing force as a factor. This statement may be verified as follows.

Let the force be wholly central, and equal to  $\mu u^2 - \frac{\mu'}{u}$ ,  $u$  being the reciprocal of the radius-vector. Also let  $\mu'$  be a small quantity whose powers above the first are neglected, and the orbit be nearly circular. By a known equation, we have

$$\frac{d^2u}{d\theta^2} + u - \frac{\mu}{h^2} + \frac{\mu'}{h^2u^3} = 0.$$

Omitting the small term, let the result of the integration be

$$a(1 - e^2)u = 1 + e \cos \theta.$$

Substituting this value of  $u$  in the small term, expanding, and omitting powers of  $e$  above the first,

$$\frac{d^2u}{d\theta^2} + u - \frac{\mu}{h^2} + \frac{\mu'a^3}{h^2} = \frac{3\mu'a^3e}{h^2} \cos \theta;$$

or, putting for shortness' sake  $w$  for  $u - \frac{\mu}{h^2} + \frac{\mu'a^3}{h^2}$ ,

$$\frac{d^2w}{d\theta^2} + w = \frac{3\mu'a^3e}{h^2} \cos \theta.$$

Now if this equation be integrated by the ordinary method applicable to exact equations, the integral will contain a term which may increase indefinitely with the time, just as in the lunar theory. But no such result follows if the two integrations be performed in succession, and regard be had to the rules of approximation. After multiplying the equation by  $2dw$ , and putting for  $dw$ , on the right-hand side, its approximate value

$-\frac{e}{a} \sin \theta d\theta$ , we get by integration,

$$\frac{dw^2}{d\theta^2} + w^2 = C - \frac{3\mu'a^2e^2}{h^2} \sin^2 \theta.$$

Hence, since approximately,

$$d\theta \sqrt{C - w^2} = -dw \text{ and } C \sin^2 \theta = C - w^2,$$

it follows that

$$d\theta = - \frac{dw}{\sqrt{C - w^2}} - \frac{3\mu'a^2e^2}{2h^2C} d\theta.$$

But  $h^2 = \mu a$  and  $C = \frac{e^2}{a^2}$  nearly. Therefore, by integration,

$$w = \sqrt{C} \cos \left\{ \theta \left( 1 - \frac{3\mu' a^3}{2\mu} \right) \right\},$$

which is the true approximation. As both approximations commenced from a fixed ellipse, it thus appears that the failure of the first is *wholly due to the mode of integration*. The occurrence of a like failure in the lunar theory is to be explained in the same manner.

This reasoning shows that the truth of my theorem respecting the eccentricity of the moon's orbit will be tested only by proceeding to the third approximation, which takes account of the effect of the tangential force on the motion of the apsides.

Cambridge Observatory,  
January 22, 1855.

XVII. *On the Adjustment of Crystals for Measurement with the Reflective Goniometer.* By W. H. M.

THE Philosophical Magazine for December last contains a description of a contrivance for adjusting a crystal for measurement on Wollaston's goniometer. Any observer who has taken the trouble to read the very clear directions for using the reflective goniometer, given more than thirty years ago by the late W. Phillips in his 'Mineralogy,' and by Mr. Brooke in his treatise on Crystallography, will find in this instrument, as usually constructed, ample provision for making the intersection of any two faces parallel to the axis of the graduated circle. The descriptions of the use of this instrument, to which reference has been made, are accompanied by figures. In these figures it will be seen that the pin which carries the plate to which the crystal is cemented, is represented in a position nearly parallel to the plane of the graduated circle. In this position, the rotation of the pin round its own axis, and the angular motion of that part of the branch into which the pin is inserted, are sufficient and necessary for the adjustment of the crystal. But when the branch is turned so that the axis of the pin coincides with that of the graduated circle, as represented in the figure in the Philosophical Magazine, that part of the power of adjusting the crystal which depends upon the rotation of the pin round its own axis is entirely lost. In addition to this inconvenience, the branch intercepts the vision of one of the signals, and thus renders an observation impossible, through no less than about  $75^\circ$  out of  $360^\circ$ . When the instrument is properly handled, the vision of the signals is uninterrupted through an entire revolution of the circle.



XVIII. *On Galvanic Circuits in which Perchloride of Iron enters as an Element.* By Professor BUFF\*.

**S**OLUTION of perchloride of iron has, as far as I know, not hitherto been used as a liquid conductor in the formation of galvanic circuits. The facility with which this body furthers the oxidation of hydrogen and the electro-positive metals, combined with the possibility of producing it at a cheap rate, led to the supposition that it might be very advantageously used as an element in a constant electric circuit. Occasional observations which Liebig had made appeared to confirm this supposition, and gave rise to the following investigation of the subject.

The first experiments, made with a very pure, almost neutral perchloride of iron, did not answer the expectations formed. An ordinary charcoal zinc pair was used, in which nitric acid was replaced by solution of perchloride of iron of syrupy consistence. To measure the current, a tangent galvanometer was employed, the simple ring of which was 200 millims. in diameter. A stream regulator was inserted in order to obtain currents of different intensities. It was found that even the weakest, which only just produced an action on the needle, showed no constancy. At the same time the charcoal was covered with metallic iron mixed with oxide of iron.

Metallic iron dipped in solution of perchloride changes it, as is well known, into protochloride, being at the same time dissolved. This occurred with the iron deposited on the charcoal when allowed to stand some time in the solution. During the progress of the electric decomposition the time was too short for this; the charcoal polarized itself with the iron, and no constant current could result.

When the neutral, saturated solution of perchloride was decomposed between platinum poles, chlorine was received at the positive pole. At the negative pole iron was deposited in minute, hard, crystalline laminae, accompanied by a very scanty evolution of hydrogen. With platinum poles of greater dimensions, the hydrogen disappeared. A small addition of hydrochloric acid to the solution prevented the deposition of the iron perfectly, even with a current of considerable strength. Hydrogen was only perceptible when the negative pole had a comparatively small surface. On the other hand, the evolution of hydrogen increased as soon as the solution was much diluted with water.

The perchloride, in a concentrated solution containing hydrochloric acid, does not appear to be electrolyzed. Its presence seems to serve only to convey fresh chlorine to the hydrogen separated from the hydrochloric acid. As long as this occurs

\* From the *Annalen der Chemie und Pharmacie*, vol. xcii. p. 117.

with sufficient rapidity, no hydrogen is visible, and the negative polar surface is not polarized.

Supported by these experiments, from four to five per cent. of hydrochloric acid was added to the perchloride of iron, and the circuit established as before. A small quantity only of HCl is required, as it is always renewed during the progress of the experiment.

The circuit thus composed showed a satisfactory constancy as long as the deviation did not exceed  $14^\circ$ . Currents of  $18^\circ$  showed, a short time after the circuit had been closed, a slight decrease. The inconstancy of the current increased slowly but gradually with a deviation of  $30^\circ$  or  $40^\circ$ . The time was manifestly too short to allow the hydrogen separated on the charcoal to be changed again into hydrochloric acid; for when the circuit was opened a short time, the original strength of the current was regularly established. The deviation of  $14^\circ$  remained unchanged even when the circuit was kept closed longer than two hours. A current which deviated the needle  $45^\circ$  would, according to experiment, have separated in 31 seconds 1 milligramme of hydrogen. By a deviation of  $14^\circ$  the same quantity requires accordingly 124 seconds. The constancy of this electromotive couple extends therefore only to those currents whose chemically decomposing power does not exceed 1 milligramme hydrogen in 124 seconds.

Manifestly this only holds good for an element of the dimensions employed. They were the usual ones; a charcoal cylinder of 5.7 centimetres interior width by 13.5 centimetres high, of which, however, only 8.5 centimetres were in contact with the solution; the zinc cylinder, 9 centimetres high and 4 centimetres wide, dipped in sulphuric acid of 1.25 spec. grav., contained in a porous cell which almost filled the cylindrical excavation of the charcoal. Of course with a greater extent of the electromotor, particularly when a greater surface of the charcoal was placed in contact with the liquid, constant streams of greater strength would be obtained.

A still more favourable result presented itself when the sulphuric acid in the porous cell was changed for a concentrated solution of common salt.

The constancy extended now to a deviation of  $22^\circ$  (one milligramme of hydrogen in 77 seconds), and the retrograde motion of the needle by higher deviations was smaller than in the preceding case. There was, further, neither deposition of iron, nor evolution of gas at the charcoal cylinder; not even when the circuit was directly closed by a short, thick, copper wire. A fine platinum wire was kept in a state of incandescence.

Compared with other galvanic elements, the perchloride of iron element, as far as power and constancy is concerned, is

inferior to Bunsen's, but superior to Daniell's. Without replacing the former, it may be preferred in many cases to the latter. A Daniell's battery of equal surface did not show an equal constancy above a deviation of  $12^{\circ}$ .

In the latter, a gradual intermixture of the liquids by endosse occurred more rapidly, and was attended with greater disadvantages than with the perchloride of iron circuit. For in the sulphate of copper circuit, the zinc covers itself by degrees with copper, and loses thereby a part of its exciting force; while the perchloride of iron which gradually penetrates into the porous cell, although it assists to dissolve some of the zinc, produces no precipitate of iron on the surface of the zinc cylinder, and consequently leaves its activity unchanged.

In order to measure the force of the chloride of iron circuit, I employed a method which is of easy execution, without being on that account less accurate. It is founded on the proposition which directly follows from Ohm's law, that "when, in electric circuits which are subject to one and the same very great resistance, alterations in this resistance occur, of such smallness that they exert no perceptible influence on the strength of the current, then the strengths of these various currents is directly proportional to the electromotive force which produces them."

In the *Annalen der Chemie und Pharmacie*, vol. lxxxvi. p. 1, I described a tangent galvanometer with a long multiplication coil, the fine copper wire of which offers a resistance to conduction, which is measured by a German silver wire, 28,000 metres long by 1.5 millim. thick. With this resistance, of itself very great, others were combined, by rolling on long and thin wires, which amounted to 2.9 to 3.1 times as much. Against this amount of resistance, that of a constant element of ordinary size manifestly vanishes, for this is seldom greater than one or two metres German silver wire. If, then, various constant elements, all of inconsiderable resistance, are closed with that long wire, and the multiplication coil united with it, we may assume that their electromotive forces are as the strengths of the currents which set them in motion, and by which the needle of the tangent galvanometer is deviated.

It is stated in the paper above referred to, under what conditions and within what limits the tangents may be taken as a satisfactory expression of the strength of the current. The numbers are, however, only then sufficiently accurate to be compared, when they relate to observations made simultaneously, or, at least, almost so. For the intensity of the force by which the deviated galvanometer needle is recalled to its position of rest is subject to certain fluctuations, whose disturbing influence often exceeds the limits of the accuracy to be obtained by this kind of

measurement. These fluctuations appear only in a smaller degree to be dependent on the changes of the earth's magnetism, or on the changeable elasticity of the thread by which the needle hangs; in a greater measure, on the contrary, on such magnetic actions as are neither to be avoided, nor entirely governed, in a building not expressly erected for magnetic observations. I have avoided, I think, all such disturbances, by referring all measurements of electromotive force to the force of a constant element of particular construction, which, as I have shown (Liebig's *Annalen*, vol. lxxxv. p. 1), maintains, for months, an absolute constancy. In order to show the use of this proceeding, I add some experiments which were made on four consecutive days in the month of February 1853, with the view of comparing the force of Bunsen's charcoal zinc element with those of the before-mentioned constant element.

The actions of a zinc-charcoal element on the needle are marked with  $\beta$ , those of the constant element, whose force served as unity of measurement, with  $\alpha$ . Each value of  $\alpha$  and  $\beta$  is the mean of two deviations caused by the same current; the one east, and the other west of zero. The numbers standing in the same horizontal line relate to observations which were made in immediately successive times.

	$\beta$ .	$\alpha$ .	$\beta$ .	$\alpha$ .	$F = \frac{\log \beta}{\log \alpha}$ .
1.	64.45	49.05	64.45	49.03	1.8159
2.	64.60	49.175	64.55	49.175	1.8175
3.	64.20	48.75	64.25	...	1.8160
4.	64.85	49.50	64.85	49.55	1.8175
					1.8167

The charcoal cylinder in these four experiments was always the same, but the apparatus was put together each time afresh. The force remained, as is seen, unaltered. From the direct results of the deviations, small fluctuations might have been calculated upon. These fluctuations affected, however, the two constant pairs equally, and depended consequently on external influences.

In the same manner we obtained for a Daniell's pair,—

$\alpha$ .	$\beta$ .	$\alpha$ .	$\beta$ .	$F = \frac{\log \beta}{\log \alpha}$ ;
49°·0	49°·7	49°·15	49°·65	1.0201

and for another element, when the amount of resistance in the long wire was changed,—

50°·1	50°·975	50°·1	1.0316.
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The investigation of the perchloride pair (slight addition of HCl, with sulphuric acid in the porcelain cell) led to these results:—

	$\alpha$ .	$\beta$ .	$\alpha$ .	F.
1.	50.1	58.3	50.1	1.3538
2.	30.3	58.5	50.35	1.3536

When more hydrochloric acid was added to the solution of perchloride of iron, the electromotive force of the element sunk to

$$F = 1.3250.$$

Finally, with perchloride of iron and a little hydrochloric acid, and solution of salt in the porous cell, we obtained,—

$$F = 1.3908.$$

This constant galvanic apparatus stands thus almost exactly in the middle between the constant charcoal element and the constant copper element. Before the latter, it has the preference of greater force and constancy; while in common with it, it has the advantage that it may be used in confined apartments without evolving injurious vapours.

XIX. *On the new Hygrometer or Dew-point Instrument.* By A. CONNELL, F.R.S.E., Professor of Chemistry in the University of St. Andrews.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

St. Andrews, Nov. 29, 1854.

I BEG to request that you will do me the favour of giving insertion in your Journal to the following observations of Dr. Buist of Bombay, contained in a letter which he was so good as to address to me, after making use, on his voyage to India, of the hygrometer described by me in your Number for last August. Dr. Buist is well known as a distinguished meteorologist, both theoretically and practically, and presided over the Meteorological Observatory at Bombay for a considerable time, and in that capacity published many highly valuable reports on this science. I may therefore well consider any opinion of his in regard to a meteorological instrument as possessing peculiar value. Unluckily his hand-writing is occasionally so indistinct and so full of contractions, that the task of reading his letters, however interesting they may be in their details, is frequently a very difficult one; and on the present occasion I have found it to be impossible to make out many portions of his letter, and an individual here accustomed to his hand was equally unsuccessful. What, however, I have quoted below has been distinctly made out; and it is quite sufficient to establish two points; *first*, that Dr. Buist found this hygrometer fully to answer its purpose as a dew-point instrument; and *secondly*, that he prefers it to the elegant hy-

grometer of the late Professor Daniell, partly on the ground of its less liability to accident. The other grounds I have not been able to make out. The passages distinctly made out are as follows:—

“Malta, July 19, 1854.

“ . . . . . I have been using it (the hygrometer) on board and found it to answer beautifully, till I broke the thermometer the day before yesterday by the slightest possible slip of the finger. . . . . The superiority of your instrument over Daniell’s is, that you can make the . . . . .

	Therm.	Wet-bulb.	Dew-point.	Barom.
12th July, Gibraltar, Noon .	70	65·9	60	30·164
3 P.M.	72	68		
13 .. ..	77	72	68	
14 .. ..	76	72	69	

I have no hesitation in pronouncing your instrument as superior to that of Daniell, inasmuch as it is stronger and less liable to accident, while it admits of a thermometer of . . . . . If yours can be constructed as cheap as Daniell’s, there can be no doubt of its preferability.

“Ever yours, most truly,  
“GEO. BUIST.”

In one part of the letter which I could not fully decipher, Dr. Buist appears to make some suggestions for strengthening the thermometer against the risk of accident in travelling, as he mentions that it had been broken in a short journey in the neighbourhood of London, although subsequently repaired. It appears to me, however, that there must have been either some defect in the packing of the thermometer in the case which Dr. Buist had obtained, or that it had been accidentally exposed to some unusual violence; the result of my own experience being, that it is in all respects quite strong enough to stand all the ordinary concussion of travelling. I carried several of the instruments, with their thermometers, during last summer from this place to London, and afterwards to Hastings and back again here, a distance in all of more than 1000 miles, without any thermometer or any part of the instruments being broken; and during the whole journey they were simply placed in their proper cases in my portmanteau, and underwent the usual not very gentle handling of the railway officials. I have also made repeated innocuous journeys between this and Edinburgh with the instrument.

It may, however, be a very proper precaution for anyone going to a distance from a town where good instrument makers are to be found, to carry with him one or more spare thermometers duly packed in their proper cases. One or two spare ivory

collars might also be useful in case of accidental fracture of that part of the instrument.

If an accident happens to Daniell's instrument, the injury is irreparable; but if a thermometer or collar happens to give way in the hygrometer under consideration, the damage is easily set right at a trifling expense. The same brass stopper will always answer again.

I ought to have mentioned formerly, that a little key of brass, to be fitted, when required, into the ivory collar at O, is employed in screwing and unscrewing the ivory collar; and that the other extremity of this key may be inserted with advantage into the neck of the little brass bottle, to aid in the same operations regarding the bottle.

In cold and dry weather in winter it is very essential that the æther should be the very best commercial æther, and probably fresh æther used for each observation. In summer, the superior excellence of the æther is by no means so important; and the used æther of winter might be kept in a properly stopped bottle for summer use, with a slight addition of fresh æther, as formerly explained. Spare leather washers ought to accompany every instrument. These often require change, to adjust the relative positions of the brass bottle and syringe.

I omitted also to mention formerly, that when an accurate observation has been accidentally missed at first, or when from any cause it is wished to repeat an observation, the temperature ought to be allowed to rise  $8^{\circ}$  or  $10^{\circ}$  above the point to which it has fallen, before it is again slowly reduced; because as soon as the exhaustion is stopped, the comparatively warmer air heats the exterior surface of the bottle faster than the liquid and the thermometer are heated; and if the cooling is not again begun from  $8^{\circ}$  or  $10^{\circ}$  higher than the temperature has fallen to, the dew-point would be got *too low*, the surface taking some time to cool again.

I have the honour to remain,  
Your very faithful Servant,  
A. CONNELL.

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XX. *On the Products of the Destructive Distillation of Animal Substances.*—Part III. By THOMAS ANDERSON, M.D., *Regius Professor of Chemistry in the University of Glasgow*.\*

**I**N the preceding parts of the investigation of the products obtained by the destructive distillation of animal substances, I have entered fully into the method of treating the raw material,

\* From the Transactions of the Royal Society of Edinburgh, vol. xxi. part 1; read April 17, 1854. [Part I. appeared in the September Number of this Journal for 1848; Part II. in our Number for December 1851.]

and have shown the existence in it of not less than three different series of bases; one, that of which methylamine is the type; a second, of which picoline is an example; and a third series, not yet further examined, to which the provisional name of pyrole bases has been applied. Besides these, aniline is also met with, but whether as an isolated substance, or accompanied by the other members of its series, cannot be determined, as none of them possess sufficiently distinctive reactions to permit their detection in a complex mixture.

To the series of which picoline is a member my attention has hitherto been specially directed, and chiefly owing to the interest attaching to these bases from their identity in composition with the corresponding members of the aniline series, aniline and picoline being the first instance in which the isomerism of two organic bases, of which we have now so many examples, was distinctly made out. In the second part of the investigation, three members of the series in question are described, namely,—

Pyridine . . . .	$C^{10} H^5 N$
Picoline . . . .	$C^{12} H^7 N$
Lutidine . . . .	$C^{14} H^9 N$

of which the two latter are isomeric with aniline and toluidine. It was further remarked, that the phenomena observed seemed to indicate that the members of this series present in Dippel's oil did not terminate with lutidine, but that bases of higher atomic weight and boiling-point manifestly existed in it. The object of the present paper is to show that this statement was well founded, by giving a description of another member of the series, and further to define their true constitution.

On pursuing the distillation of the different fractions of basic products obtained by the process described in the second part of this investigation, and distilling at temperatures above  $305^{\circ}$ , which is about the boiling-point of lutidine, it was found that, when converted into platinum salts, the per-centage of platinum gradually diminished as the boiling-point rose. Taking advantage of the well-known empirical law, that the boiling-points of homologous substances rise by  $34^{\circ}$  of Fahrenheit for every addition of  $C^2 H^2$  to the atom, and from which the boiling-points of pyridine, picoline, and lutidine do not greatly differ, I directed my attention to the portion of mixed bases boiling about  $340^{\circ}$ , in which it was reasonable to expect that the next base of the series should be found. But even after repeated rectifications, the base distilling at this temperature still gave a very powerful reaction of aniline with chloride of lime, and the per-centage of platinum in its double compound was but little lower than that of the lutidine salt, or at all events never reached the number



required by theory for the higher base. Being convinced that the separation of two bases approximating so closely in their boiling-points as aniline and the substance I expected to find, could not be effected by fractionated distillation, or at least only by an expenditure of time, labour and material, altogether out of proportion to the importance of the object to be attained, I endeavoured to accomplish it by crystallization. Having observed that the other members of the picoline series gave highly soluble and even deliquescent oxalates, I conceived that by converting the mixed bases into salts of that acid, it would be easy to separate the rather sparingly soluble and highly crystallizable oxalate of aniline, and obtain the oxalate of the other base in a state of purity. But this expectation was not confirmed by experiment; for neither from the portion boiling about  $340^{\circ}$ , nor even from that collected at  $360^{\circ}$ , and corresponding therefore with the boiling-point of pure aniline, could the slightest trace of crystallized oxalate of aniline be obtained, although both fractions gave the reaction of that base in the most powerful manner. The experiment was varied in every possible way by the use of water, spirit, and absolute alcohol, but by allowing these fluids to evaporate spontaneously only a thick syrup was obtained, without the slightest indication of crystallization. Even the addition of æther to its alcoholic solution gave only a syrupy fluid, and no crystals; and I was forced to conclude, that, even in the portion of the mixed bases corresponding to the boiling-point of aniline, its quantity was so small in proportion to the other substances, that the properties of its salts were entirely masked by them.

Not succeeding in obtaining the pure aniline, and so separating it from the other base, the question came to be, how to get rid of the former substance in the best possible way. For this purpose I availed myself of the extreme stability of the bases of the picoline series mentioned in my former paper, which is so great that they resist even the action of strong nitric acid, by which aniline is entirely destroyed. When the base, boiling between  $340^{\circ}$  and  $345^{\circ}$ , is mixed rapidly and in large quantity with nitric acid, much heat is evolved, and a brisk action takes place; and if the portion boiling about  $360^{\circ}$  is employed, the action is so violent as to be almost explosive, and it is requisite to add the base drop by drop to the acid, which must be kept carefully cool. The acid fluid acquires a deep red colour, and on boiling, red fumes are abundantly evolved, accompanied by an odour resembling that of bitter almonds. After the action has ceased, the fluid becomes muddy when mixed with water, and a thick reddish-yellow oil is deposited, which has exactly the odour of nitrobenzide, and resembles it in many of its properties. The quantity of this substance produced is by no means large,

and it is evidently mixed with some resinous substance. Owing to this circumstance I have not been able to submit it to purification and analysis, so as to ascertain whether it really is nitrobenzide; but though that substance has not yet been obtained by a similar action on pure aniline, it is quite possible that it may be produced, and the reason why it has not hitherto been observed is probably because no one has had occasion to sacrifice large quantities of aniline in this manner. The acid solution of the undecomposed base is passed through a wet filter in order to separate the oil, and the fluid boiled for some time to expel the last traces. On saturation with potash and distillation, an oily base passed over with the water and collected on the surface. This base, on being converted into a platinum salt, was found still to give a result greatly above that required by theory for the substance of which I was in quest. On submitting it to distillation, it was found to commence boiling at about  $320^{\circ}$ , and hence to contain a large quantity of lutidine; and it was only the very last portion which gave a platinum salt corresponding with theory. It was clear that a large quantity of lutidine had been retained at a boiling-point above that which naturally belonged to it, by the presence of aniline, and that substance being destroyed, it came over at its natural boiling-point. The higher fractions of the oil containing aniline were therefore treated in a similar manner, and the undecomposed bases, which amounted to from a half to two-thirds of the original quantity acted on by nitric acid, were submitted to fractionated distillation. The product was found to spread over a considerable number of degrees, and a quantity of that collected between  $340^{\circ}$  and  $345^{\circ}$  was converted into a platinum salt and analysed, but the results indicated the presence of much lutidine. The product being still obviously impure, was submitted to a systematic fractionation, and it was observed that the thermometer remained remarkably steady about  $354^{\circ}$ . The portion boiling between  $350^{\circ}$  and  $360^{\circ}$  was collected apart, and after several rectifications a fraction was obtained which distilled entirely between  $352^{\circ}$  and  $356^{\circ}$ , and proved to be the pure base, to which I give the name of collidine.

#### *Collidine.*

Collidine is obtained in the form of a transparent and colourless oil, which may be preserved for a long time in bottles only partially filled with it, without acquiring colour. A rod dipped in hydrochloric acid brought in contact with it gives abundant white fumes. It is insoluble in water, and floats on its surface, without undergoing diminution. It dissolves a small quantity of water, which is readily separated by caustic potash. It is

highly soluble in alcohol, æther, and the fixed and volatile oils. It dissolves with great facility in the acids, but even when added in large excess it does not neutralize them. It precipitates alumina, chromium, zinc, and peroxide of iron from their solutions, but gives no precipitate with baryta, lime, magnesia, manganese or nickel. It throws down oxide of lead from the nitrate, but not from the acetate, a remarkable peculiarity, which it shares with methylamine and æthylamine. With corrosive sublimate it forms a double salt, but from salts of the suboxide of mercury it throws down the oxide. Its odour is strong, aromatic, and far from unpleasant. Its specific gravity is 0.921, and it boils at 354°. The following results were obtained by analysis:—

I. 4.075 grs. of collidine gave 11.800 grs. carbonic acid and 3.450 grs. water.

II. 4.079 grs. of collidine gave 11.800 grs. carbonic acid and 3.393 grs. water.

III. 4.124 grs. of collidine gave 11.980 grs. carbonic acid and 3.560 grs. water.

	Experiment.			Calculation.	
	I.	II.	III.		
Carbon .	78.97	78.89	79.22	79.33	C <sup>15</sup> 96
Hydrogen	9.40	9.24	9.58	9.09	H <sup>11</sup> 11
Nitrogen	11.63	11.87	11.20	11.58	N 14
	100.00	100.00	100.00	100.00	121

These numbers correspond with the formula C<sup>15</sup> H<sup>11</sup> N. Collidine forms, therefore, another member of the picoline series, and corresponds in constitution with the base described by Cahours, under the name of xylidine, in the aniline series, with which, however, it is isomeric only, and not identical, its properties being different in all respects.

The salts of collidine are for the most part highly soluble and deliquescent. When evaporated, they form uncrystallizable gummy masses, some of which, on standing, show traces of crystallization. They are soluble also in alcohol, but not in æther. The only highly crystallizable compounds are the mercury and platinum double salts.

The mercury double salt is thrown down in the form of a curdy-white precipitate, on the addition of a solution of corrosive sublimate to a solution of the hydrochlorate of collidine. It dissolves in boiling spirit, and is deposited, on cooling, in needles. It could not be obtained of definite composition.

*Platinochloride of Collidine* is obtained when strong solutions of hydrochlorate of collidine and bichloride of platinum are mixed. It is slowly deposited in the form of orange-yellow prisms or needles, according to the degree of concentration of

the fluids. It is readily soluble in water, but insoluble in alcohol and æther. Its analysis gave the following results:—

I. 6.013 grs. of platinochloride of collidine gave 6.345 grs. carbonic acid and 1.937 gr. water.

II. 5.040 grs. of platinochloride of collidine gave 5.360 grs. carbonic acid and 1.650 gr. water.

I. 5.620 grs. of platinochloride of collidine gave 1.705 gr. platinum.

II. 4.535 grs. of platinochloride of collidine gave 1.362 gr. platinum.

III. 5.097 grs. of platinochloride of collidine gave 1.657 gr. platinum.

	Experiment.			Calculation.	
	I.	II.	III.		
Carbon	28.77	29.00	...	29.33	C <sup>16</sup> 96
Hydrogen	3.57	3.63	...	3.66	H <sup>12</sup> 12
Nitrogen	...	...	...	4.31	N 14
Chlorine	...	...	...	32.54	Cl <sup>3</sup> 106.5
Platinum	30.33	30.03	29.89	30.16	Pt 98.7
				100.00	327.2

These results correspond with the formula C<sup>16</sup>H<sup>11</sup>N, HCl+PtCl<sup>2</sup>, and entirely confirm the constitution of the base. The rest of its salts have not been particularly examined, as they did not present anything of interest.

[To be continued.]

### XXI. Notices respecting New Books.

*A Treatise on Frictional Electricity.* By PETER THEOPHILUS RIESS.  
Berlin: 1853.

THE materials for a treatise on electricity lie in part scattered through former treatises, and, in detached portions, through the pages of scientific journals. A portion of these materials is doubtful; a part false; another part true, and therefore destined to live. To choose wisely from such a mass—to preserve the true, reject the false, and assign to the doubtful its proper value, requires a correctness of judgment which a practical cultivation of this portion of science can alone bestow. How many of our scientific books are compiled by persons who themselves have no clear conception of the things concerning which they write, and who therefore introduce confusion and dismay into the minds of their readers! For it is not by the mere withholding of knowledge that such writers do harm; this would be a negative evil, but it is not the worst; it is the sapping of the reader's faith in his own ability, the forcing upon him the conviction that he has no natural capacity for such studies, that does most mischief. If a scientific hand-book be defective in clearness or unsound in argument, the reader who comes to it for instruc-

tion, not possessing a sufficient amount of culture to enable him to form an independent judgment, accepts the fallacy, and attributes the absence of clearness to his own, rather than to his teacher's want of understanding, whereas the latter is often the true cause of failure. Now there is no task more responsible than that of the man who undertakes to write a book for the instruction of the community in science, and no man ought to embark in such an undertaking who is not prompted by higher motives than the mere hope of commercial success. He must love his subject. Years of alliance with it must have made him master of it up to its boundaries, and enabled him to detach the hypothetical from the true. He must know the true value of those images by which natural truths are connected, and rendered intelligible to the human mind; a knowledge necessary to preserve him from confounding the symbols of science with its verities. He must himself be an investigator, and his skill as such must be ratified by his own discoveries and his manner of communicating them, before his qualifications are complete.

Few, we imagine, possess the requirements to which we have alluded, in a greater degree than the writer of the book before us. His lifetime has been spent in the investigation of the subject on which he writes. On almost every page we find evidence that the man is not writing from hearsay merely, but that the experiments he records have been repeated by himself, and come to us with the sanction of his own authority. There is no portion of frictional electricity which has been left untouched: on all points the reader will obtain ample information; and while he reads, he may carry with him the comforting assurance that he possesses a teacher worthy of his confidence. There are many things introduced into this treatise which we have observed in no other; many, indeed, that we owe to the author himself, and which constitute the most important contributions to this portion of science which have been recently made.

To the man who wishes to pursue electricity as an earnest study this work will prove of great value, abounding, as it does, with practical suggestions which are founded on the experience of a lifetime. To the mathematician who desires a safe basis for his calculations the book will prove eminently useful. Many portions are beyond the range of the mere popular reader; they are addressed to the student; but even the popular reader, by omitting the portions thus referred to, will find all he needs remaining. He must not, indeed, expect to meet in the work an encyclopædia of electrical amusements: the writer has applied himself earnestly to his task, and has chosen those experiments which illustrate the principles of his science, and which prompt to further inquiry. His vocation is to teach, not to amuse; and to him who desires to obtain a comprehensive view of the laws and phenomena of this portion of science, we are acquainted with no work which we can recommend with greater confidence than the treatise before us.

The work consists of two volumes, large octavo; it is amply illustrated, and the plates are beautifully executed. The manner, indeed, in which the work is presented to the public reflects great credit upon its publisher.

XXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 73.]

Nov. 16, 1854.—Colonel Sabine, R.A., V.P. and Treas., in the Chair.

THE following communication was read:—

Letter from Lieutenant Maury to Admiral Smyth, For. Sec. R.S.

“National Observatory, Washington,  
October 21, 1854.

“SIR,—I have the honour to state, for the information of the Royal Society, that a new asteroid was discovered here by Mr. James Ferguson, Assistant Astronomer, at 11 P.M., 2nd of Sept. 1854.

“He was observing Egeria at the time, and found that, the 13th, and this, the 31st, in the field together.

“I have delayed this communication, waiting to ascertain whether the planet might not have been discovered by observers in other parts of the world; and it appearing that it had not, the priority of the discovery, therefore, belongs to the National Observatory; and this new star is added to the family of asteroids as the first representative of America among them, and a memorial of her zeal in the cause of astronomy.

“As a testimony of the high appreciation in which the talents and the industry of Mr. Ferguson are held, the honour of naming this planet was left to him. Following the rule adopted by astronomers with regard to the asteroids, he has selected the graceful name of Euphrosyne.

“Its approximate ephemeris, with the last observations, are herewith enclosed.

“I have the honour to be,

“Respectfully, &c.,

“M. F. MAURY,

“Rear-Admiral W. H. Smyth, R.N.”

“Lieut. U.S.N.”

Ephemeris of Euphrosyne.

M. T. Washington.	$\alpha$ .	$\delta$ .
1854, Oct. 19.	$\begin{matrix} h & m & s \\ 9 & 26 & 41\cdot9 \end{matrix}$	$\begin{matrix} h & m & s \\ 1 & 12 & 15\cdot11 \end{matrix}$
		$-1^{\circ} 56' 7\cdot21''$

Elements of Euphrosyne, computed by Prof. Keith, from observations of Sept. 2nd, 6th and 10th.

M.	$13^{\circ} 36' 33\cdot3''$	Sept. 2·721	M. T. Greenwich.
$\Pi$	$352^{\circ} 5' 50\cdot6''$		M. Equ. 1854 0.
$\Lambda$	$33^{\circ} 29' 21\cdot7''$		
$i$	$22^{\circ} 39' 13\cdot6''$		
$\phi$	$4^{\circ} 22' 30\cdot2''$		
log $a$	0·469530		
log $\mu$	2·845712		

Ephemeris for October.

M. T. Berlin.	$\alpha$ .	$\delta$ .	log $r$ .	log $\Delta$ .
1854, Oct. 19·5	$\begin{matrix} h & m & s \\ 1 & 12 & 0 \end{matrix}$	$\begin{matrix} \circ & ' & '' \\ 1 & 59 & 21 \end{matrix}$	0·43828	0·24622
23·5	$\begin{matrix} 1 & 7 & 49 \end{matrix}$	$\begin{matrix} 1 & 47 & 29 \end{matrix}$	0·43850	0·24937
27·5	$\begin{matrix} 1 & 3 & 49 \end{matrix}$	$\begin{matrix} 1 & 33 & 49 \end{matrix}$	0·43873	0·25345
31·5	$\begin{matrix} 1 & 0 & 3 \end{matrix}$	$\begin{matrix} 1 & 18 & 18 \end{matrix}$	0·43897	0·25861

Nov 23, 1854.—Thomas Bell, Esq., V.P., in the Chair.

The following communications were read :—

“On the Impregnation of the Ovum in the Stickleback.” By W. H. Ransom, M.D.

I purpose placing before the Royal Society in this communication, the principal results of experiments made during the months of June and July last, on the impregnation of the ovum in *Gasterosteus leiurus* and *G. pungitius*, and hope to be able to furnish a more detailed account of my observations on a future occasion.

The ovarian ovum of these fishes, at a very early stage of its development, is provided with a proper investing membrane, the future chorion. At a later period, one portion of this membrane presents a number of cup-shaped pediculated bodies scattered over its surface, and in the centre of this part of the chorion there is a funnel-shaped depression, pierced by a canal which leads towards the centre of the egg.

In the nearly ripe ovum, the germinal vesicle occupies an excentric position with respect to the egg as a whole, but imbedded in the centre of a semi-solid accumulation of fine granular matter at that part of the surface which corresponds to the funnel-shaped depression; so that the apex of the funnel, projecting inwards beyond the level of the inner surface of the chorion, makes a depression in the centre of the layer of granular matter, and comes nearly into contact with the germinal vesicle.

For convenience of description, the funnel-shaped depression will now be called *micropyle*, and the layer of granular matter before impregnation, *discus proligerus*.

The germinal vesicle disappears before the ovum leaves the ovary, and no remnant of it or its spots can be seen.

A very delicate membrane invests the yelk within the chorion; this membrane is more distinct after impregnation, or after the action of water upon an unimpregnated egg; it may be isolated, and then exhibits a remarkable degree of elasticity. It is not a *yelk-membrane*, and it will be spoken of as the *inner membrane*.

The layer of the yelk immediately internal to the inner membrane passing over the *discus proligerus*, is formed by yellowish highly refractive drops which disappear in water, undergoing some remarkable changes, and by a fluid substance which water precipitates in a finely granular form.

The principal mass of the yelk consists of a clear and very consistent albumen. The oil is collected into a few very large drops which come up to the surface.

When the ovum escapes from the ovary, it enters a cavity which may be considered as the ovarian extremity of the oviduct, in which a considerable quantity of clear viscid fluid is previously secreted and collected, to be expelled with the ova.

More exact observation of the micropyle in the free eggs proves that the inner end of the canal is either open, or at most closed by a very delicate membrane. When looking into the funnel from the wide mouth, the apex being in focus, a bright, clear, round or oval

spot, such as an aperture would produce, is always visible. If a section be made of the egg, and the apex brought into focus from within, the same clear spot is well seen, and the fine and regularly dotted structure of the chorion is seen to cease suddenly at the margin of the clear spot.

The general form of the egg after deposition is round, but it is rendered irregular by indentations caused by the pressure of other eggs. It is inelastic, and retains impressions made in it by a needle; and when placed in water, these characters remain for a long time if it be not impregnated,—a fact which indicates that water does not pass through the micropyle, or by imbibition through the chorion. The viscid secretion of the oviduct which invests the eggs may defend them against the action of water, in which it does not readily diffuse or dissolve. This secretion has an alkaline reaction. The substance of the yolk has a decidedly acid reaction,—more than enough to neutralize the alkalinity of the viscid secretion. This reaction is, I believe, due to a peculiar organic acid, but the experiments relating to this question are not yet complete. The seminal particles of the male continue to move for a considerable period in the viscid secretion which envelopes the ripe ova, but they very quickly become still in water.

In the act of impregnation one or more (as many as four have been seen) spermatozooids pass into the micropyle, and probably by their proper motion overcome the obstruction which prevents the entrance of water. Actively moving spermatozooids may remain in contact with the chorion for eighteen minutes at least without producing any sensible change in the ovum, provided none of them enter the micropyle, but when one is seen to enter, in about a quarter of a minute a change is observable.

The changes which are observed to follow the entrance of the spermatozooids into the micropyle are the following:—In about a quarter of a minute the tube is shortened, and very soon a clear space becomes visible within the chorion near the micropyle: this space, or respiratory chamber, gradually extends to the opposite pole of the egg and increases in diameter, as does also the whole ovum. During the formation of this space the surrounding fluid enters through the micropyle, and this gradually retracts and is at length closed. This entrance of fluid into the egg effaces the depressions, restores the round form, and makes it firm and elastic; but does not cause any such precipitation of granular matter as is produced by its artificial introduction.

While the respiratory chamber is yet in progress of formation, the yellow drops of the superficial layer of the yolk grow pale and disappear; the change beginning near the micropyle. As a result of this, the whole egg becomes clearer, and the *discus proligerus*, which may be now more correctly denominated the *germinal mass*, is more distinct.

The yolk now very slowly alters its form, one surface becoming flattened; but about fifteen or twenty minutes after impregnation a remarkable and more vivid contraction begins, causing the yolk to pass through a series of regularly recurring forms. The contraction



begins on one side near the equator, and soon forms a circular constriction which gives the yolk the figure of a dumb-bell, the longer axis of which is the polar axis of the egg. The constriction travels towards the germinal pole, and next produces a flask-shaped figure; this is at length lost by the constriction passing on, and the round form is regained in about a minute. This wave reappears and travels forward again without any distinct period of rest, and I have seen these movements continue for forty-five minutes, though towards the latter part of this period they are less distinct and more limited in extent. The germinal mass has itself during these contractions, which strongly resemble the peristaltic movements of the intestine, undergone changes in form, and has increased in bulk and distinctness. These movements are unaffected by weak galvanic currents.

During the passage forward of each wave of contraction there is an oscillation of the whole mass of the yolk, so that its germinal pole passes once to the right and once to the left of the micropyle, to which it at first corresponded. The plane of this oscillation may be vertical, horizontal, or inclined, but always cuts the micropyle; it begins and ceases with the contractions already mentioned, and would seem to be a mechanical result of them.

For some time before cleavage begins, the only changes of form are the appearance of wave-like elevations and depressions along the under surface of the germinal mass, and its alternate concentration and diffusion. Cleavage begins in about two hours after impregnation; no embryonic cell was observed before it began, nor in any of the cleavage masses.

The inner membrane is folded in during cleavage; it is easily seen thrown into folds at the cleft, and for this reason I do not consider it a *yolk-membrane*, which term would be better applied to the chorion.

“On the Applicability of Gelatine Paper as a Medium for Colouring Light.” By Horace Dobell, Esq.

The object of this communication is threefold.

(1.) To point out the properties of a material called Gelatine Paper, which render it applicable as a medium for colouring light.

(2.) Through the means of gelatine paper, to introduce the use of coloured light in the arts for the preservation of the sight of artisans.

(3.) To introduce the use of gelatine paper for the relief of persons suffering from impaired vision; for the preservation of the sight of travellers, and of all those who are much engaged in reading.

This material was invented in 1829 by the late M. Grenet, of Rouen, and was exhibited by him in its present state of perfection at the Great Exhibition of 1851. But up to the present time it has not been successfully applied to any more useful purposes than the manufacture of artificial flowers, address-cards, tracing-paper, wafers, wrappers for confectionary, and the like.

It is commonly manufactured in sheets, measuring 22 inches in length and 16 inches in diameter, which are sold at a small price; but the sheets can as easily be made of any dimensions not exceeding those of which plate-glass is capable. It can be made of

any thickness, from that of the finest tissue paper upwards. It may be obtained as transparent as the best glass, and more free from colour, or of all colours and shades of colour, without interfering with its transparency. It is exceedingly light, and may be bent or rolled up without injury. It can be cut with scissors like ordinary paper, and may easily be stitched with a needle and thread. By means of an aqueous solution of gelatine, it can be made to adhere accurately to plates of glass without any interference with its transparency. When varnished with collodion it becomes perfectly waterproof, more pliable, capable of bearing a considerable degree of heat without injury, and its transparency is not affected.

Hence it appears, that, in addition to its transparency and susceptibility to various colours and forms, gelatine paper is cheap, portable, and durable.

Such being the properties of the material, the following are enumerated by the author as some of the forms in which he suggests that it may be employed, and in which it has already been found useful.

1. A small sheet of very pale green or blue gelatine paper, to be used in reading. The sheet is simply to be laid upon the page of the book, and the reading to be conducted through the coloured medium. If used in a faint light, the reading paper is to be raised a little from the book to admit more light beneath it.

2. A sheet of gelatine paper of pale green set in a light frame, and placed like a screen before the window or lamp of the engraver, the watchmaker, the jeweller, and the like; thus providing a light of genial colour in which they may pursue their occupations.

3. A similar appliance to the last-mentioned for the use of needlewomen. For this purpose screens are to be provided, both of green and of blue gelatine paper; so that the white materials employed in needlework may be changed to a pleasant green, by the screen of that colour, the yellow materials to a green by the blue screen, and by one or other of these screens the reds softened down into violets or browns.

4. For either of the two last purposes on a larger scale, the gelatine paper may be attached to the window glass of the apartment, thus colouring, if necessary, all the light admitted during daylight.

5. Shades for the eyes in certain affections of the sight, to take the place of the green or blue silk and card shades worn by many persons. The gelatine paper being transparent, will allow the wearer to see his way about, at the same time that the eyes are protected from a glaring light. This may be especially useful in cases where it is desired not only to shade a diseased eye, but also to protect its nerves from strong light admitted by the sound eye. When not only coloured light but a certain degree of darkness is required, this can be readily and delicately graduated by employing shades of different depths of colour.

6. Masks of gelatine paper for protecting the eyes of travellers against the glare of snow-fields and of sandy deserts.

XXIII. *Intelligence and Miscellaneous Articles.*

ON THE LIMIT OF VAPORIZATION OF MERCURY. BY C. BRAME.

FROM the numerous experiments which I have made on this subject, it appears,—1, that gold, which was employed for the purpose by Mr. Faraday, is not a sufficiently sensitive reagent for the detection of very small quantities of the vapour of mercury; and that if, as Faraday has done, we are able by this means to prove that at  $68^{\circ}$  to  $77^{\circ}$  F. mercury gives off vapour to a height of more than  $0^{\text{m}}\cdot 100$ , and if the formation of this vapour can no longer be shown at more than  $0^{\text{m}}\cdot 020$  when the temperature is below  $32^{\circ}$  F., this does not prove that mercury has a limited atmosphere. 2. On the contrary, it is proved that the vapour of mercury rises easily to 1 metre at temperatures comprised between a few degrees below  $32^{\circ}$  F and  $86^{\circ}$  F. Moreover, it has been ascertained in M. Chevreul's laboratory, that when a tube of a diameter of  $0^{\text{m}}\cdot 050$ , open at both ends, was placed over mercury at a temperature of  $68^{\circ}$  to  $78^{\circ}$  F. for a fortnight, the vapour of the latter gave a brown colour to vesicles of sulphur placed at a distance of  $1^{\text{m}}\cdot 440$  from the surface of the mercury. In the vaults of the observatory, vesicles of sulphur placed at a distance of  $1^{\text{m}}\cdot 760$  from the mercury were rendered entirely brown in four months; the sulphur was placed in a small tube open at both ends, and supported by a spiral iron wire in a larger tube closed at one end, and reversed over the mercury. The larger tube was  $0^{\text{m}}\cdot 021$  in diameter, and  $1^{\text{m}}\cdot 80$  in height. In twenty days, in the same situation, the vapour of 30 grms. of mercury, placed at the bottom of tubes from  $1^{\text{m}}\cdot 100$  to  $1^{\text{m}}\cdot 200$  in height, gave a brown colour to vesicles of sulphur deposited on the sides of little flasks which served as stoppers. The same result was obtained several times, the temperature sometimes remaining constant, as in the vaults of the observatory, sometimes varying  $18^{\circ}$  F. or more, as in the laboratory, &c.; the air in the tubes was sometimes more or less free, sometimes confined, without the appearance of any sensible difference. At all temperatures it is always sulphuret of mercury that is formed; at  $32^{\circ}$  F. and above that temperature it is of a deeper or paler brown, but at  $18^{\circ}$  F. the vapour of mercury gave a yellow or red colour to vesicles of sulphur placed at a distance of several centimetres; the vapour of soft amalgams had often the same effect. In the former case, by continuing the experiments for a sufficiently long time, ponderable quantities ( $0\cdot 006$  to  $0\cdot 0230$  gr.) of mercury were condensed in the vesicles of sulphur, and the same was the case with mercurial ointment, amalgams of silver, tin, &c. Vesicular sulphur, the needles produced by fusion and hardened sulphur, which contain vesicles, take up mercury, even when placed at the bottom of tubes of half a metre in length resting upon mercury.

3. When, under the circumstances already indicated, vapour of iodine (produced in very small quantity at the ordinary temperature by a few centigrammes of iodine volatilized and condensed on the walls of a small flask serving as a stopper for the tube) descending from the upper part of the tube containing the mercury was substituted for the vesicular sulphur, the vapour of mercury appeared to form a limited atmosphere above the liquid metal. In fact, in this case, it

did not appear to rise beyond  $0^m\cdot036$  to  $0^m\cdot038$  at  $69^\circ$  F., and only to  $0^m\cdot020$  to  $0^m\cdot022$  at  $53^\circ\cdot6$  F., as appeared from the iodides of mercury which were deposited upon the walls of the tube. In the vaults of the observatory, it is true, the red iodide formed a very thin ring, and this ring was only at a distance of a few millimetres from the mercury; this, which could not be foreseen from the previous results, must be attributed to the moisture, the iodine tubes having been fixed against the wall. In fifteen days, at a temperature of  $72^\circ\cdot5$  F. in the meridian chamber of the observatory, an amalgam of silver gave rise to a much larger ring with vapour of iodine. This ring was formed of very distinct crystals of determinable form; it was at  $0^m\cdot015$  from the amalgam. In most of the experiments with iodine, the ring formed consisted of several iodides of the metal, but the red crystallized iodide formed the greater portion, or existed alone when the vapour of iodine being in slight excess, the experiment was stopped at the proper time. By means of a tube of 1 metre in length placed vertically in a larger tube resting upon mercury, John Davy's experiment was verified, proving that iodine, placed at a distance from mercury at the ordinary temperature, absorbs the vapour of the latter. This agrees with the result furnished by vesicular sulphur, and shows well that if the mercurial atmosphere appears to be limited when the iodine is in the state of vapour, this arises solely from the circumstance, that the vapour of iodide of mercury, and even that of iodine, have a considerable density compared with the air. Hence it results that the vapour of mercury is repelled to a certain limit which varies with the temperature, whilst the rings formed by the iodides of mercury are at a tolerably constant height at the same temperature. The vapour of bromine at ordinary temperatures has the same action as that of iodine, as also the vapour of turpentine and gaseous chlorine at  $167^\circ$  F. &c.

#### *Conclusions.*

1. Vapour of mercury appears to be subjected, like other vapours, to the law of mixture of gases and vapours, at all temperatures between a few degrees below  $32^\circ$  F. and  $86^\circ$  F.; and all the experiments performed with sulphur in various states concur to show that the limit set to the vaporization of mercury in consequence of the exclusive employment of gold as a reagent, is exceeded beyond anything that could have been foreseen from Faraday's experiments, as it has been ascertained that the vapour of mercury can rise to a height of  $1^m\cdot76$  at a constant temperature of  $52^\circ\cdot7$  F.

2. If vapour of iodine, under certain circumstances, gives a result which at first sight appears to indicate that vapour of mercury forms a limited atmosphere, this appears to depend entirely upon the considerable density, compared with the air, of the vapour of iodine and of that of the iodide or iodides formed, and upon the slight volatility of the latter. The same conclusion applies to vapour of bromine and gaseous chlorine.

3. On the other hand, the volatility of the product formed by the combination of vapour of mercury with sulphur, and the very small quantity of vapour of comparatively small density furnished by sul-

phur at ordinary temperatures, remove all difficulties in explaining the results obtained with this substance. The vesicles of sulphur absorb the vapour of mercury with great energy, and when sulphur is employed, nothing prevents the mixture of air with the vapours produced. The principal result obtained in this case may therefore be considered the normal result.

4. By bringing together the fact observed by Faraday, which is otherwise perfectly correct, and the views of Poisson upon the state of the last strata of the terrestrial atmosphere, the limitation of the vapour of mercury has been employed in the highest discussions of general physics, both in treating of the limitation of the atmosphere of the earth and of the divisibility of matter; questions which, since the time of Wollaston, have been regarded as correlative. The results of my experiments appear to me to prove that it is impossible to refer to the limitation of the vaporization of mercury in speculations of this nature.—*Comptes Rendus*, Nov. 20, 1854, p. 1013.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1854.

*Chiswick*.—December 1. Clear and fine. 2. Overcast: very large halo round the moon. 3. Cloudy: clear. 4. Clear and fine. 5. Fine: cloudy: clear at night: rain. 6, 7. Fine: cloudy. 8. Densely overcast. 9. Cloudy. 10. Clear and cold. 11. Sharp frost: fine. 12. Very fine. 13. Hazy: overcast: rain. 14. Densely clouded. 15. Dusky flying clouds: overcast: rain. 16. Cloudy: fine. 17. Clear: fine: rain. 18. Rain: heavy clouds: clear and cold: rain. 19. Very fine: rain. 20. Stormy and wet. 21. Fine: rain: overcast. 22. Cloudy and boisterous: rain at night. 23. Fine: cloudy: overcast: rain. 24. Fine: rain. 25. Densely clouded: rain: clear and fine. 26. Clear and fine. 27. Fine. 28. Clear and frosty: very fine: frosty. 29. Overcast. 30. Cloudy: fine. 31. Overcast: cloudy: boisterous.

Mean temperature of the month ..... 39°·35

Mean temperature of Dec. 1853 ..... 32·49

Mean temperature of Dec. for the last twenty-eight years ... 39·64

Average amount of rain in Dec. .... 1·50 inch.

*Boston*.—Dec. 1, 2. Fine. 3. Cloudy: rain P.M. 4. Fine. 5. Cloudy. 6. Fine: rain A.M. and P.M. 7. Fine. 8. Cloudy. 9. Fine: rain and snow P.M. 10, 11. Fine. 12. Fine: rain P.M. 13. Cloudy. 14. Cloudy: rain A.M. 15. Fine. 16. Fine: rain A.M. 17. Fine. 18. Rain A.M. and snow P.M. 19. Fine: rain P.M. 20. Rain A.M. 21. Cloudy: rain A.M. 22. Cloudy. 23, 24. Fine. 25. Cloudy: rain P.M. 26. Cloudy. 27. Fine: rain A.M. and P.M. 28. Fine. 29—31. Cloudy.

*Sandwick Manse, Orkney*.—Dec. 1. Snow-showers A.M.: hail-showers P.M. 2. Snow-showers A.M.: cloudy P.M. 3. Rain A.M.: showers P.M. 4. Showers A.M. and P.M. 5. Showers A.M.: cloudy P.M. 6. Showers A.M. and P.M. 7. Cloudy A.M.: sleet-showers P.M. 8. Showers A.M.: sleet-showers P.M. 9. Showers A.M. and P.M. 10. Bright A.M.: snow P.M. 11. Cloudy A.M.: showers P.M. 12. Sleet-showers A.M.: showers P.M. 13. Cloudy A.M.: showers, thunder and lightning P.M. 14. Sleet-showers A.M.: showers P.M. 15. Sleet-showers A.M. and P.M. 16. Hail-showers A.M.: sleet-showers P.M. 17. Sleet-showers A.M.: hail-showers, frost P.M. 18. Hail-showers, frost A.M. and P.M. 19. Cloudy, frost A.M.: rain P.M. 20. Sleet-showers A.M.: clear P.M. 21. Drizzle A.M.: showers P.M. 22. Damp A.M.: sleet-showers P.M. 23. Hail-showers A.M.: sleet-showers P.M. 24. Showers A.M.: sleet-showers P.M. 25. Showers A.M.: hail-showers P.M. 26. Snow A.M.: hail-showers P.M. 27. Snow A.M.: snow, clear P.M. 28. Snow, cloudy A.M.: rain P.M. 29. Drizzle A.M. and P.M. 30. Sleet-showers A.M.: hail-showers P.M. 31. Showers A.M.: rain P.M.

Mean temperature of Dec. for twenty-seven previous years . 41°·10

Mean temperature of this month ..... 39·13

Mean temperature of Dec. 1853 ..... 38·97

Average quantity of rain in Dec. for fourteen previous years 3·95 inches.

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

Days of Month.	Barometer.				Thermometer.				Wind.			Rain.	
	Chiswick.		Boston		Orkney, Sandwick.		Chiswick.		Boston	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
	Max.	Min.	9 a.m.	8½ p.m.	Max.	Min.	9 a.m.	8½ p.m.	Chiswick 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.
1854. Dec.													
1.	29.730	29.422	29.12	29.30	51	30	32	32	W.	W.	HW.	.....	.....
2.	29.951	29.768	29.42	29.64	46	37	32	31½	HW.	HW.	SE.	.....	.....
3.	29.937	29.762	29.18	28.92	50	41	46½	44½	HW.	W.	WSW.	.....	.....
4.	30.025	29.894	29.15	29.11	50	38	42	42	W.	WNW.	S.	.....	.....
5.	29.509	29.318	28.57	28.57	51	32	47.5	44	SW.	SW.	SSW.	.....	.....
6.	29.656	29.424	28.12	29.76	45	31	40	41½	W.	HW.	HW.	.....	.....
7.	30.137	30.035	30.00	29.42	42	23	34	37	W.	WNW.	S.	.....	.....
8.	29.892	29.483	29.13	29.02	50	33	42	45½	SW.	WSW.	W.	.....	.....
9.	29.634	29.442	29.23	29.69	45	30	36	37	W.	HW.	HW.	.....	.....
10.	30.057	29.833	29.91	29.70	39	19	33	38	D.	HW.	SSE.	.....	.....
11.	30.045	29.856	29.24	29.15	44	27	47	40	SW.	SW.	WSW.	.....	.....
12.	30.057	30.002	29.42	29.22	45	30	34	40½	SW.	W.	SW.	.....	.....
13.	30.190	29.967	29.22	29.24	52	42	37	41	W.	SW.	W.	.....	.....
14.	30.017	29.945	29.40	29.50	55	49	51	42½	SW.	W.	W.	.....	.....
15.	30.112	29.808	29.35	29.50	52	42	51	38	W.	W.	HW.	.....	.....
16.	29.869	29.657	29.50	29.53	47	26	39	37½	W.	W.	WNW.	.....	.....
17.	29.939	29.752	29.35	29.27	45	33	39	39	HW.	HW.	WNW.	.....	.....
18.	29.464	29.002	29.28	29.61	39	29	38.5	35	W.	W.	WNW.	.....	.....
19.	29.750	29.701	29.40	28.94	44	33	29	35	W.	W.	WNW.	.....	.....
20.	29.955	29.406	29.46	28.94	44	33	29	35	HW.	HW.	SE.	.....	.....
21.	30.125	29.953	29.72	29.48	51	39	36	47	W.	W.	HW.	.....	.....
22.	29.882	29.828	29.23	29.37	54	28	53	41	W.	W.	HW.	.....	.....
23.	29.895	29.778	29.50	29.50	47	26	37	38	SW.	W.	WNW.	.....	.....
24.	29.879	29.768	29.32	29.19	52	40	34	41½	SW.	W.	WNW.	.....	.....
25.	29.778	29.528	28.66	28.94	54	33	51	36	SW.	W.	WNW.	.....	.....
26.	29.818	29.722	29.17	29.29	46	32	37	33½	SW.	W.	WNW.	.....	.....
27.	29.948	29.811	29.62	30.02	41	25	34	25	HW.	HW.	W.	.....	.....
28.	30.431	30.313	30.02	29.94	40	20	30	32	HW.	HW.	S.	.....	.....
29.	30.443	30.396	29.77	29.86	41	34	33	48½	SW.	WNW.	SW.	.....	.....
30.	30.444	30.354	29.97	29.76	47	32	44	41	SW.	W.	HW.	.....	.....
31.	30.328	30.101	29.60	29.21	47	35	44	46	HW.	W.	HW.	.....	.....
Mean.	29.963	29.775	29.392	29.402	47.03	31.67	39.0	39.45	HW.	W.	HW.	1.27	1.71
								38.81					7.75

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[FOURTH SERIES.]

MARCH 1855.

XXIV. *Further Observations on associated cases, in Electric Induction, of Current and Static Effects.* By PROFESSOR FARADAY, D.C.L., F.R.S. &c.\*

MELLONI, whose loss science must deeply feel, was engaged in the latter part of his life in investigations relating to static electricity, especially concerning induction, conduction, &c. He desired, in reference to these and the results I had published respecting the charge of, and conduction by, subterraneous and subaqueous insulated wires †, to know whether there was any difference in the *time* of transmission through such wires, of currents having greater or less intensity, *i. e.* of currents from batteries of different numbers of plates. I applied to Mr. Latimer Clark on the subject; and he with the same earnestness as on the former occasion, sought and seized the opportunity of making experiments of the like kind, and gave me the results, which I sent to Melloni. The latter published them with some observations in an Italian Journal (whose title is not on the paper which he sent to me), and soon after he was suddenly removed from us by death. As Mr. Clark's results are not yet known in this country, I have thought that a brief account of them would be valuable. His process records, by the printing telegraph of Bain, the results obtained with 768 miles of copper wire covered with gutta percha, and laid in the ground in four lines between London and Manchester, so connected that the beginning and the end of the whole length was in London. The following are his words, dated May 31, 1854:—

“I have tried a few experiments on the relative velocities of currents of different intensities, and I enclose you some strips of

\* Communicated by the Author.

† Royal Institution Proceedings, i. 345; or *Phil. Mag.* 1854, vii. p. 197.

paper showing the results. I was unable to equalize the deflections of a galvanometer by currents of intensity with small plates as compared with currents from a few large plates, for no size of plate would make up for the deficiency in intensity. I allude to the form of experiment suggested by Melloni;—but I believe they will be of interest to him.

“The experiments were made through 768 miles of gutta percha wire, viz. from London to Manchester and back again twice, with our ordinary sulphate of copper batteries, plates 3 inches square, and with intensities varying from 31 cells to sixteen times 31 cells, or 500 cells.

“In the accompanying strips the upper line indicates the time during which the current was sent, being made by a local arrangement.

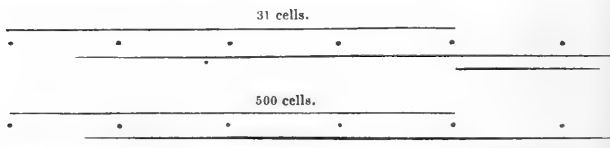
“The second line (of dots) indicates *time by seconds*, being made by a pendulum vibrating seconds, and striking a light spring at the centre of its arc of vibration.

“The third line indicates the time at which the current appeared at (what we may call) the distant end of the line, 768 miles off.

“The fourth line merely shows the residual discharge from the near end of the wire, which was allowed to communicate with the earth as soon as the batteries were disconnected; this has no reference to the subject of our inquiries.

“It will be seen by the *third line*, that about two-thirds of a second elapsed in every case before the current became apparent at the distance of 768 miles, indicating a velocity of about 1000 miles a second; but the most interesting part appears to be, that this velocity is *sensibly uniform for all intensities* from 31 cells to 500.”

Melloni has then given a copy of the records made when 31 pair and 500 pair of plates were employed; unfortunately the copy is inaccurate, since it makes the fourth line commence as to time at the termination of the third, whereas it ought to correspond with the termination of the first; also the third line on each does not thin off as those upon the record do. The following is a copy from other slips obtained at the same time from the Bain's printing apparatus. Experiments with 62, 125, and 250 cells, gave like results with those of 31 and 500 cells.



After certain observations, which are mainly upon the manner



of the experiments, and the way in which practical difficulties were avoided, Melloni says, "It appears, then, that when the electric current possesses sufficient force to overcome the sum of the resistance offered by a given conductor, whatever its length may be, an augmentation of its intensity ten or twentyfold does not alter the velocity of its propagation. This fact is in open contradiction with the general meaning attributed to the denominations of *quantity* and *intensity*; since the first compares the mass of electricity to that of a fluid, and the second represents its elasticity or tendency to motion. The equal velocity of currents of various tension offers, on the contrary, a fine argument in favour of the opinion of those who suppose the electric current to be analogous to the vibrations of air under the action of sonorous bodies. As sounds, higher or lower in pitch, traverse in air the same space in the same time, whatever be the length or the intensity of the aerial wave formed by the vibration of the sonorous body; so the vibrations, more or less rapid or more or less vigorous, of the electric fluid excited by the action of batteries of a greater or smaller number of plates, are propagated in conductors with the same velocity. Every one will see how the hypotheses imagined by us to give a reason for natural phenomena, will serve to suggest certain experimental investigations, the results of which will test their validity or insufficiency."

Melloni then says that he shall shortly have occasion to publish facts which clearly demonstrate the errors of certain conclusions admitted up to the present time respecting electro-static induction; and I am aware, from written communications with him, that he considered the results arrived at by Coulomb, Poisson, and others since their time, as not accordant with the truth of nature\*. In the mean time he died, and whether his researches are sufficiently perfected for publication or not, I do not know.

The uniformity in the time and appearance of currents of different intensities at the further end of the same wire in the same inductive state, is a very beautiful result. It might at first be supposed to be in opposition to the views I set forth some years ago on induction and conduction, and the statements more

\* He says, "I deceive myself much, or else the fundamental theorem of electrical induction, as we find it ordinarily announced, ought to be modified so as not to confound two effects completely distinct—the electric state during induction, and after the contact and separation of the inducing body. We know perfectly what occurs in the latter case, but not in the former," &c. Again, "In my last letter I raised doubts with regard to the consequences which have up to the present been deduced from the experiments serving as a base for the fundamental theorem of electro-static induction. These doubts have passed to a state of certitude in my mind, . . . . and behold me at this time thoroughly convinced that the enunciation of that theorem ought to be essentially modified." (July 1854.)

recently made with regard to *time*. That, however, does not appear to me to be the case, as a few further observations on Mr. Clark's recent experiments will perhaps show. When the smaller battery is used, much less electricity passes into the wire in a given time, than when the larger one is employed. Suppose that the batteries are so different that the quantities are as 1 to 10; then, though a pulse from each would take the same time for transmission through the wire, still it is evident that the wire would be a tenfold better conductor for the weak current than for the strong one; or in other words, that a wire having only one-tenth of the mass of that used for the greater current should be employed for the smaller one, if the resistance for *equal quantities* of electricity having different intensities is to be rendered equal.

My views connect the retardation of the transmitted current with the momentary induction set up laterally by the insulated and externally coated wire. The induction will be proportionate to the intensity, and therefore its especial effect on the time of retardation proportionately diminished with the less intense current,—a result of action which will aid in rendering the time of retardation of the two currents equal.

The difference of *time* in the former experiments with air wires, and earth or water wires, very clearly depends upon the difference of lateral induction; the air wire presented a retardation scarcely sensible, the earth wire one amounting to nearly two seconds. If the insulating layer of gutta percha could be reduced from 0.1 to 0.01 of an inch in thickness, and mercury could be placed on the outside of that instead of water or earth, I do not doubt that the time would be still more increased. Yet there is every probability that in any one of these varying cases, electric currents of high and of low intensity would appear at the end of the same long wire after equal intervals of time.

Mr. Clark's results may be stated thus:—A given quantity of electricity at a high intensity, or a smaller quantity at a proportionally lower intensity, will appear at the further end of the same wire after the lapse of the same period of time. My statement assumed the discharge of the *same* quantity at *different* intensities through the *same* wire, and the quantities in the illustrative experiments were measured by a Leyden jar. In the consideration and further development of these results, it must be remembered that it is not the difference either in time, velocity, or transmission of a *continuous* current which constitutes the object in view, for that is the same both for an air wire and a subterraneous wire, but it is the difference in the *first appearance* only of the same current when wires under these different conditions are employed. After the first appearance both wires are

alike in power unto the end of the current, and then a difference again appears which is complementary to the first.

There are many variations of these experiments which one would wish to make, if possible, and perhaps by degrees the possibility, or else equivalent experiments in other forms, may occur. If the wire employed were changed from a cylinder to a flat ribbon of equal weight, or to several small wires, all being equally coated with gutta percha and submerged, differences would probably arise in the time of delay with the same current; and I think that the ribbon, presenting more induction surface than the cylinder, would cause more delay; but probably any one of these, or of like varieties, would cause the *same* delay for currents of different intensities. Again, one can scarcely doubt that with different conducting substances, as iron and copper, the delay would vary, as is the case in the transmission of sound and light. That the delay for currents of high and low intensity should be the same for the same wire in any one of such cases may still be expected, but it would be very interesting to *know* what would be the fact.

The prosecution of these results and the principles concerned in them, through the various forms they may assume by such like variations of the conductors and also of the currents, offers, as Melloni has observed, most extensive and interesting inquiries: even the power of a current to induce a current in neighbouring wires and conductors is involved in the inquiry, and also the phenomena and principles of magneto-electric induction.

Royal Institution, Feb. 7, 1855.

XXV. *On certain Propositions in Algebra connected with the Theory of Probabilities.* By GEORGE BOOLE, LL.D., Professor of Mathematics in Queen's College, Cork\*.

**B**EFORE entering upon the immediate subject of this paper, I wish to state the connexion in which it stands with my previous papers on the theory of probabilities, published in this Journal.

If the reader will refer to what I have said in the conclusion of my second paper, published in the August Number of this Journal for 1854, he will find that the claims of the doctrine which I have put forth are made to rest upon its satisfaction of the following requirements:—

1st. That the principles upon which its methods are founded should be of an axiomatic nature.

\* Communicated by the Author.

2nd. That they should lead to results capable of verification, wherever verification is possible.

3rd. That they should be capable of a systematic development consistent in all its parts and processes, and neither acknowledging nor imposing any limitations but those which exist in the nature of things.

Now the principles in which, and in the mode of the application of which, that doctrine consists, have been fully stated in this Journal (December 1854). In so far as they may be considered novel, they are certainly in no proper sense mathematical. I do not indeed conceive that they are in any respect novel, except that they are brought into a novel connexion, and are made the basis of a new application. I do not think that any person competent to form an opinion upon these subjects would dispute such positions as the following: viz. 1st, that probability is *relative*—that it depends, not upon the actual connexions of things, but upon those connexions *as known* to us; 2nd, that the distinction of events into simple and compound is relative, and the selection of simple terms arbitrary and the offspring of language; 3rd, that merely logical connexions which are founded upon definition possess only a derived necessity, and that from the contemplation of things or events *defined*, and subject *as such* to relations founded upon definition, we may ascend to the contemplation of things free, and only to be expressed by signs of larger comprehension, but from which, by introducing the limitation again, the conception of the more restricted class may be derived. I say, for instance, that between the term “king” and some other term which we will suppose to designate a member of a particular legislative body, relations founded upon definition of the respective terms might exist, which relations would cease to exist if we ascended to the larger terms of “ruler” and “legislator,” and would be restored when we imposed upon those terms the former limitations. Now these are principles which are not in any proper sense mathematical. They would seem to belong far more to the psychologist and to the inquirer into the nature of language than to the mathematician. I have, however, shown that in connexion with received principles they form a valid basis for the theory of probabilities; and in my treatise on the Laws of Thought numerous verifications of the results of that theory will be found. I proceed now to that which I consider the most important of all, the proof that this theory satisfies the third and last of the conditions above adverted to.

The argument which I purpose to develop is the following. In the demonstration of the general method in probabilities, published in this Journal (December 1854) and in the Laws of Thought, we ascend, in accordance with the third of the princi-

ples above stated, from the events whose probabilities are given, and whose logical connexion is explicitly determined from their *definition*, to events which are free, and from which by limitation the former are derived. Of those "free" events, the unknown probabilities *s, t, &c.* are involved in the final algebraic equations to which the method conducts. As *probabilities* they ought to admit of positive fractional values. Now it will be shown that they do admit of such values when the data represent a possible experience, and not otherwise. The conditions of possibility in the data are identically the conditions of mathematical consistency in the method. Other important consequences are connected with this demonstration, to which attention will be directed.

*Proposition I.*

Let *V* be a rational and integral function of the *n* unknown quantities *x, y, ... z*, consisting wholly of positive terms, and involving all such terms as can be formed without introducing powers of *x, y, ... z* higher than the first; also let *V* represent the sum of those terms in *V* which contain *x* as a factor, *V<sub>y</sub>* the sum of those which contain *y* as a factor, and so on. Then the system of equations

$$\frac{V_x}{V} = p, \quad \frac{V_y}{V} = q \dots \frac{V_z}{V} = r, \quad . . . . \quad (1)$$

*p, q, .. r* being positive fractions, admits of one solution, and of only one, in positive values of the quantities *x, y, .. z*.

To exemplify this proposition, let us suppose

$$V = axyz + byz + cxz + dxy + ex + fy + gz + h,$$

then it is affirmed that the system of equations

$$\frac{axyz + cxz + dxy + ex}{axyz + byz + cxz + dxy + ex + fy + gz + h} = p \quad . . \quad (2)$$

$$\frac{axyz + byz + dxy + fy}{axyz + byz + cxz + dxy + ex + fy + gz + h} = q \quad . . \quad (3)$$

$$\frac{axyz + byz + cxz + gz}{axyz + byz + cxz + dxy + ex + fy + gz + h} = r, \quad . . \quad (4)$$

all the coefficients *a, b, c, &c.* being positive and the quantities *p, q, r* positive and fractional, admits of one, and only one, solution in positive values of *x, y, z*.

It is evident that the proposition is true when *n=1*; for then we have *V=ax+b*, and the system (1) is reduced to the single equation

$$\frac{ax}{ax + b} = p,$$

whence

$$x = \frac{bp}{a(1-p)};$$

and this single value is positive if  $a$ ,  $b$ , and  $p$  are positive, and the last fractional.

Now the general proof of the proposition consists in showing, that if it is true for a particular value of  $n$ , it is true for the next greater value, and so on *ad infinitum*. Wherefore, since it is actually true for the case of  $n=1$ , it is true universally.

We shall therefore begin by assuming that the proposition is true for  $n-1$  variables. And making this assumption, let us suppose the first  $n-1$  equation of the system (1) to be satisfied while  $z$  varies from 0 to  $\infty$ , and seek under these circumstances the nature of the variation which the first member of the final equation of the system undergoes.

Now if a particular positive value be given to  $z$ , the  $n-1$  first equations of the system (1) will assume the form of a corresponding system relative to the  $n-1$  variables  $x, y, \dots$  which remain. Thus if in the equations (2) and (3) we give to  $z$  a particular positive value, and then make

$$az + d = A, \quad cz + e = B, \quad bz + f = C, \quad gz + h = D,$$

those equations will become

$$\frac{Axy + Bx}{Axy + Bx + Cy + D} = p,$$

$$\frac{Axy + Cy}{Axy + Bx + Cy + D} = q,$$

$A, B, C, D$  being known positive constants; and this is a binary system derived from the function

$$V = Axy + Bx + Cy + D,$$

just as the system (2), (3), (4) was derived from the expression for  $V$  by which it was preceded.

Hence if the proposition be true for a system involving  $n-1$  unknown quantities, it will be true for that system which is formed by giving to  $z$  in the  $n-1$  first equations of (1) a particular positive value. And therefore to each value of  $z$  there will correspond a single set of positive values of the  $n-1$  unknown quantities  $x, y, \dots$  determined by the solution of that system. That set of values will of course vary as  $z$  varies. Moreover, when  $z=0$ , the first member of the final equation will be 0. This is evident from the form of that member as developed in (4), or in any other special instance. When  $z = \infty$ , the first member of the final equation is 1, as is in like manner made evident. Hence as  $z$  varies from 0 to  $\infty$ , while the  $n-1$  first

equations of the system (1) are satisfied, the function  $\frac{V^z}{V}$  in the final equation varies continuously from 0 to 1. In such variations it must at least once coincide with the value  $r$ . Hence there must be at least one solution of the entire system (1) in positive values of  $x, y, \dots z$ . It remains then to show that there cannot be *more* than one such solution. If there be more than one such solution, the value of  $\frac{V^z}{V}$  must, in the variation above described, more than once coincide with  $r$ ; and therefore, varying continuously, it must between such points of coincidence admit of a maximum or minimum value. But it may be shown that there exists no such value. The nature of the proof of this proposition will be best illustrated by a particular example.

Let us take the particular case in which  $n=2$ . Our equations then are

$$\frac{axy + bx}{axy + bx + cy + d} = p, \quad \dots \dots \dots (5)$$

$$\frac{axy + cy}{axy + bx + cy + d} = q; \quad \dots \dots \dots (6)$$

and we are to inquire if it is possible to satisfy, by positive values of  $x$  and  $y$ , the conditions

$$\frac{axy + bx}{axy + bx + cy + d} = p,$$

$$\frac{axy + cy}{axy + bx + cy + d} = \text{maximum or minimum.}$$

Differentiating in the usual way, we find, after slight reductions,

$$(ay + b)(cy + d)dx + (ad - bc)xdy = 0,$$

$$(ad - bc)ydx + (ax + c)(bx + d)dy = 0,$$

from which, eliminating  $dx$  and  $dy$ ,

$$(ay + b)(cy + d)(ax + c)(bx + d) - (ad - bc)^2xy = 0;$$

or developing,

$$a^2bcx^2y^2 + ab(ad + bc)x^2y + ac(ad + bc)xy^2 + ab^2dx^2 + 2abcdxy + ac^2dy^2 + bd(ad + bc)x + cd(ad + bc)y + bcd^2 = 0. \quad (7)$$

Now this equation, since its first member consists wholly of terms of positive sign, cannot be satisfied by any set of positive values of  $x$  and  $y$ . Hence, by the previous reasoning, the system (5), (6) admits of one, and of only one, solution in positive values of  $x$  and  $y$ .

The class of functional determinants, to use Jacobi's expression, exemplified in the first member of (7), and obtained in like

manner from the ternary, quaternary, and higher systems derived from (1) by the assigning of the successive values of 3, 4, &c. to  $n$ , is possessed of remarkable properties. Representing by  $H_n$  the function thus formed from the system of  $n$  equations, I have obtained by a laborious analysis, the relation

$$H_n = V^{n-1}K, \dots \dots \dots (8)$$

where  $K$  is a rational and integral function of the  $n$  unknown quantities  $x, y, \dots z$ , consisting wholly of positive terms. Thus, in the above case of  $n=2$ , where  $H_2$  is expressed in full by the first member of (7), it would follow that

$$H_2 = V(abcxy + abdx + acdy + abd), \dots \dots (9)$$

wherein

$$V = axy + bx + cy + d, \dots \dots \dots (10)$$

as may easily be verified. In this case the functions  $V$  and  $K$  are *reciprocally* related. If to  $V$  we assigned the actual form of  $K$ , we should find for  $K$  the form of  $V$ , with the addition of a constant factor.

When  $n=3$ , we have, in like manner,

$$H_3 = V^2K, \dots \dots \dots (11)$$

wherein

$$V = axyz + byz + cxz + dxy + ex + fy + gz + h;$$

and here  $K$  is a wholly positive function of the sixth degree, which, if we exhibit only its initial and final terms, may be thus expressed,

$$K = abcdx^2y^2z^2 + (adfc + abdc)x^2y^2z + (abce + acdg)x^2yz^2 + (abcf + abdg)xy^2z^2 \dots + efgh. \dots \dots \dots (12)$$

The proof which I have obtained of the simply positive character of the function  $K$  is rigorous up to the case of  $n=4$ , and it is further of such a nature as to leave upon my mind no doubt of its *general* truth. Assuming, as I venture to do, that it is generally true, the argument stands thus :

Since  $H_n$  is wholly positive, the equation

$$H_n = 0 \dots \dots \dots (13)$$

admits of no solution in positive values of  $x, y, \dots z$ . Hence the function  $\frac{V_z}{V}$  in the final equation of the system (1) admits of no maximum or minimum value, while  $x, y, z$  are positive, and the  $n-1$  first equations of the system are satisfied. Hence in the variation of  $z$  from 0 to  $\infty$ , there is one point, and only one, at which the entire system (1) is satisfied by positive values of  $x, y, \dots z$ . Hence, finally, that system admits of one, and only one, solution in positive values of those quantities.



*Proposition II. with connected problem.*

If  $V$  be a rational and integral function of the  $n$  variables  $x, y, \dots, z$ , consisting wholly of positive terms, but involving no powers of the variables higher than the first, and if  $V_x$  represent the aggregate of those terms in  $V$  which contain  $x$  as a factor,  $V_y$  the aggregate of those which contain  $y$  as a factor, and so on; then if we form the system of  $n$  equations

$$\frac{V_x}{V} = p, \quad \frac{V_y}{V} = q \dots \frac{V_z}{V} = r, \text{ \&c., } \dots \dots \quad (14)$$

that system cannot have more than one solution in positive values of  $x, y, \dots, z$ . And in order that it may have one such solution, certain conditions must in general be satisfied by the quantities  $p, q, \dots, r$ ; which conditions it is required to investigate.

Of the general proposition I shall not attempt to give here a fully developed proof. I purpose only to give such an account of the proof, together with special illustrations, as may place the reader in a position to supply deficiencies.

I shall begin with the investigation of the conditions between  $p, q, \text{ \&c.}$

When  $x, y, z$  are positive, the first member of each equation of the system (14) is in general the sum of a series of proper fractions. Thus if

$$V = axyz + byz + cxz + dxy + e,$$

in which case the system (14) becomes

$$\left. \begin{aligned} \frac{axyz + cxz + dxy}{axyz + byz + cxz + dxy + e} &= p \\ \frac{axyz + byz + dxy}{axyz + byz + cxz + dxy + e} &= q \\ \frac{axyz + byz + cxz}{axyz + byz + cxz + dxy + e} &= r \end{aligned} \right\}; \dots \dots \quad (15)$$

and if we make

$$\frac{axyz}{V} = \lambda, \quad \frac{byz}{V} = \mu, \quad \frac{cxz}{V} = \nu, \quad \frac{dxy}{V} = \rho, \quad \frac{e}{V} = \sigma,$$

it is evident that  $\lambda, \mu, \nu, \rho,$  and  $\sigma$  will be proper fractions, and that the system will assume the form

$$\left. \begin{aligned} \lambda + \nu + \rho &= p \\ \lambda + \mu + \rho &= q \\ \lambda + \mu + \nu &= r \\ \lambda + \mu + \nu + \rho + \sigma &= 1 \end{aligned} \right\}, \dots \dots \quad (16)$$

the last equation resulting from definition. And to these equa-

tions we may add the inequations

$$\lambda \geq 0, \quad \mu \geq 0, \quad \nu \geq 0, \quad \rho \geq 0, \quad \sigma \geq 0. \quad . \quad . \quad (17)$$

The mode of eliminating  $\lambda, \mu, \nu, \&c.$  from the above combined systems is given in my paper 'On the Conditions by which the Solutions of Questions in the Theory of Probabilities are limited,' published in this Journal (August 1854). And the actual elimination leads to the following results: viz. 1st, that  $p, q,$  and  $r$  must be positive fractional quantities; 2nd, that they must satisfy the conditions

$$q + r \geq p, \quad p + r \geq q, \quad p + q \geq r.$$

These conditions, then, *at least* must be satisfied in order that the system (15) may admit of solution in positive values of  $x, y, z.$  To the quantity  $r$  they assign the inferior limits  $p - q$  and  $q - p,$  and the superior limits  $p + q$  and 1.

Exactly the same method is applicable to every form and variety of the system (14), the final result always being a system of linear inequations connecting the fractional positive quantities  $p, q, \dots r.$

The conditions above determined are, moreover, the *only* ones which are necessary in order that the system (14) may admit of a solution in positive finite values of the variables  $x, y, \dots z,$  that case alone excepted in which one of the variables appears as a common factor in, or is wholly absent from, the terms of  $V,$  such variable admitting then the values 0 and  $\infty.$  This proposition is true in the case of  $n = 1,$  as is obvious; and it may be shown that if it is true for any particular value of  $n,$  it is true for the next greater value, and so on *ad infinitum.* Whence its general truth follows. The most important steps of the proof I subjoin.

Assigning to  $z$  in the  $n - 1$  first equations of the system (14) any particular value, those equations degenerate into a system of equations among the  $n - 1$  variables  $x, y, \&c.,$  from which, by assumption, a single positive set of values of those  $n - 1$  variables may be determined. These values substituted, together with the assumed value of  $z$  in the first member of the last equation of the system  $\frac{V}{V^z},$  will give to that member a corresponding value  $t.$  In order, then, that the system (14) may admit of a solution in positive values of  $x, y, z,$  it suffices that the limits of the quantity  $t,$  as  $z$  varies through the whole extent of positive magnitude from 0 to  $\infty,$  should not transcend those of  $r.$  For if the limits of  $r$  be wider than those of  $t,$  then  $r$  will admit of values, which, while they satisfy the linear conditions

before assigned, do not permit us to satisfy the system (14) by positive values of  $x, y, z$ .

Now as  $z$  varies from 0 to  $\alpha$ ,  $t$  will vary continuously from the highest of the inferior to the lowest of the superior limits of  $r$ . Of this proposition I am not prepared to offer at present a perfectly general proof, though I have no doubt that I possess all the elements of such a proof. I am content to state that I have met with no special case in which I have not been able to prove it, and by a general method; but that a difficulty, probably of notation, intervenes at present between this and the general proof. The following particular verification may easily be applied to all the cases in which  $n=3$ . I apply it to the system (15).

If we clear of fractions the two first equations of that system, and arrange the result with reference to  $x$  and  $y$ , and if at the same time we equate the first member of the last equation of that system to  $t$ , clear of fractions, and arrange in like manner, we shall have, writing for simplicity  $p', q', t'$  for  $1-p, 1-q, 1-t$  respectively,

$$\begin{aligned} (ap'z + dp')xy + cp'zx - bpzy - ep &= 0 \\ (aq'z + dq')xy - cqzx + bq'zy - eq &= 0 \\ (at'z - dt)xy + ct'zx + bt'zy - et &= 0. \end{aligned}$$

The elimination of  $x$  and  $y$  from this system will conduct to a final equation between  $z$  and  $t$ , the solution of which will determine the value of  $t$  when  $z$  is given. Hence it will determine the value of the first member of the final equation of the system (15) when a particular value is assigned to  $z$ , while the two first equations of the system are satisfied.

Effecting, then, the required elimination, we obtain a result which may be thus written,

$$AD - BC = 0, \dots \dots \dots (18)$$

the functions A, B, C, D being eliminants\* having the following expressions:

$$\begin{aligned} A &= \begin{Bmatrix} ap'z + dp', & cp'z, & -bpz \\ aq'z + dq', & -cqz, & bq'z \\ at'z - dt, & ct'z, & bt'z \end{Bmatrix}, \\ D &= \begin{Bmatrix} cp'z, & -bpz, & -ep \\ -cqz, & bq'z, & -eq \\ ct'z, & bt'z, & -et \end{Bmatrix}, \end{aligned}$$

\* I use this term with Professor De Morgan in preference to the term "determinants." Mr. De Morgan's reasons, which appear to me conclusive, will be found in a note to a very able memoir 'On some points in the Theory of Differential Equations,' in a recent Number of the Cambridge Philosophical Transactions.

$$B = \left\{ \begin{array}{l} -bpz, -ep, ap'z + dp' \\ bq'z, -eq, aq'z + dq' \\ bt'z, -et, at'z - dt \end{array} \right\},$$

$$C = \left\{ \begin{array}{l} -ep, ap'z + dp', cp'z \\ -eq, aq'z + dq', -cqz \\ -et, at'z - dt, ct'z \end{array} \right\}.$$

From the consideration of these forms, we see that (18) may be expressed in the form

$$Pz^5 + Qz^4 + Rz^3 + Sz^2 = 0, \dots \dots \dots (19)$$

P, Q, R, and S not involving  $z$ , but being functions of  $t$  and the constants of the original system (15).

The limiting forms of the above equation, as  $t$  approaches the respective limits 0 and infinity, are evidently

$$S = 0, \quad P = 0;$$

the former of these gives, on constructing the expression for S by means of (18) and of the expressions for A, B, C, D,

$$\left\{ \begin{array}{l} -bp, -ep, dp' \\ bq', -eq, dq' \\ bt', -et, -dt \end{array} \right\} \times \left\{ \begin{array}{l} -cp, dp', cp' \\ -eq, dq', -cq \\ -et, -dt, ct' \end{array} \right\} = 0;$$

whence, developing and dividing by the constant factors  $bed$  and  $edc$ , we get

$$(p + t - q)(q + t - p) = 0,$$

$$\therefore t = p - q \text{ or } q - p. \dots \dots \dots (20)$$

In precisely the same way the equation  $P = 0$  gives for the values of  $t$  corresponding to  $z = \infty$ , the relations

$$\left\{ \begin{array}{l} ap', cp', -bp' \\ aq', -cq, bq' \\ at', ct', bt' \end{array} \right\} \times \left\{ \begin{array}{l} cp', -bp, -ep \\ -cq, bq', -eq \\ ct', bt', -et \end{array} \right\} = 0;$$

which on development and division by the constant factors  $abc$  and  $cbe$ , gives

$$(1 - t)(p + q - t) = 0,$$

whence

$$t = 1 \text{ or } p + q. \dots \dots \dots (21)$$

But the values (20) and (21) thus determined for the first member of the final equation of the system (15), when  $z$  assumes the respective values 0 and infinity, are precisely the values before determined for the limits of  $r$ .

Further, it appears, as was to be shown, that if we assume  $x, y, z$  to be positive quantities satisfying the two first equations

of the system (15), the first member of the final equation will, when  $z=0$ , coincide with the highest of the inferior limits of  $r$  above determined, and when  $z = \infty$  with the lowest of the superior limits; for the highest inferior and the lowest superior limit are the true limits of  $r$ , as above determined, on the assumption that  $x, y, z$  are positive quantities. And therefore they are the true limits of the first member of the final equations under the same conditions.

Thus it has been shown that as  $z$  varies from 0 to infinity, while  $x$  and  $y$  are positive quantities constantly determined so as to satisfy the two first equations of the system (15), the first member of the final equation of (15) will vary from the highest of the inferior to the lowest of the superior limits of  $r$ . In the variation it must once, or more often, coincide with the actual value of  $r$ . But that it cannot more often than once coincide with  $r$  is evident from the constitution of  $H_n$  (Prop. I.), which does not permit that the function in question should ever resume a former value while  $z$  continues to increase. For if such resumption were possible, a maximum or minimum value of the function would intervene; and this, as  $H_n$  continues to consist of positive terms only, cannot happen. One solution therefore exists, and one only, of the system (15) in positive values of  $x, y, \dots z$  when the linear conditions of inequality among the quantities  $p, q, \dots r$  are satisfied, and then only.

The reasoning is general, and serves to establish the general truth of the proposition.

### Proposition III.

*The conditions which must be satisfied in order that the final algebraic system of equations*

$$\frac{V_s}{p} = \frac{V_t}{q} \dots = V, \quad \dots \quad (22)$$

*to which, by a general method, the solution of questions in the theory of probabilities is reduced (Laws of Thought, p. 270, Phil. Mag. December 1854), admits of a single solution or positive fractional values of  $s, t, \&c.$ , are identical with the conditions of possible experience in the data of the problem.*

The function  $V$  in this proposition is the sum of a series of terms which we may by analogy term algebraic constituents. If the number of variables  $s, t, \&c.$  be two, the possible algebraic constituents are

$$st, s(1-t), (1-s)t, (1-s)(1-t),$$

and  $V$  consists of some or the whole of these.  $V_s$  is the sum of those constituents of which  $s$  is a factor, and so on for the others. The algebraic system (22) is derived from a logical equation in

which  $st$ ,  $s(1-t)$ , &c. occur as the constituents, properly so called, of a logical development, having therein a logical, and not a quantitative interpretation. That the conditions of possible experience may be deduced from such a development has been shown in my last paper on the Theory of Probabilities, published in this Journal (Dec. 1854, note). It is most convenient, indeed, to deduce them from the immediate data. This is the course which I have adopted in my paper 'On the Conditions, &c.' (Phil. Mag. August 1854). But they may also be deduced from the final logical development; and the results are necessarily equivalent, inasmuch as the final development and the initial data are connected by a chain of analysis founded upon those laws of thought which are the basis of ordinary deductive reasoning.

And hence it will suffice to show, that the required conditions of algebraic solution of the system (22) are the same as the conditions of possible experience determined from the logical development from which the algebraic system is derived.

To determine the conditions of algebraic solution, let us first place the system in the form

$$\frac{V_s}{V} = p, \quad \frac{V_t}{V} = q, \quad \&c.,$$

and then assuming

$$\frac{s}{1-s} = x, \quad \frac{t}{1-t} = y, \quad \&c.,$$

we obtain a result of the form

$$\frac{V_x}{V} = p, \quad \frac{V_y}{V} = q, \quad \&c.,$$

agreeing with the form considered in Prop. II.

Thus, if we have

$$V = stu + st(1-u) + s(1-t)u + (1-s)tu + (1-s)(1-t)(1-u), \quad (23)$$

in which case the system (22) becomes

$$\left. \begin{aligned} \frac{stu + st(1-u) + s(1-t)u}{V} &= p \\ \frac{stu + st(1-u) + (1-s)tu}{V} &= q \\ \frac{stu + st(1-t)u + (1-s)tu}{V} &= r \end{aligned} \right\} \dots \dots \dots (24)$$

And if we therein make

$$\frac{s}{1-s} = x, \quad \frac{t}{1-t} = y, \quad \frac{u}{1-u} = z,$$

we obtain a system which agrees with (15), the constants  $a, b, c, d$  being made severally equal to 1.

The conditions which must be fulfilled in order that the transformed system may have a single solution in positive values of  $x, y, z$ , depend, as it has been seen, on the analysis of (16), (17). Let us then seek what would be the conditions of possible experience in a problem, in the solution of which we should be conducted by the general method in probabilities to the algebraic system (24).

That system must arise from a logical equation

$$V=1, \quad . . . . . (25)$$

and a connected system of data,

$$\text{Prob. } s=p, \quad \text{Prob. } t=q, \text{ \&c.},$$

$s, t$ , &c. being logical symbols. Moreover,  $V$  as a logical function is identical in *form* with  $V$  as an algebraic function (23). Now by virtue of (25) all constituents vanish except those which are found in  $V$ . Hence

$$\left. \begin{aligned} \text{Prob. } s &= \text{Prob. } \{stu + st(1-u) + s(1-t)u\} = p \\ \text{Prob. } t &= \text{Prob. } \{stu + st(1-u) + (1-s)tu\} = q \end{aligned} \right\}. \quad (26)$$

Moreover, as  $V=1$  denotes, as a logical equation, the *certainty* of the event  $V$ , we shall have

$$\text{Prob. } V=1. \quad . . . . . (27)$$

Hence if we make

$$\text{Prob. } stu = \lambda, \quad \text{Prob. } (1-s)tu = \mu, \quad \text{Prob. } s(1-t)u = \nu,$$

$$\text{Prob. } st(1-u) = \rho, \quad \text{Prob. } (1-s)(1-t)(1-u) = \sigma,$$

we shall have from (26) and (27)

$$\lambda + \nu + \rho = p$$

$$\lambda + \mu + \rho = q$$

$$\lambda + \mu + \nu = r$$

$$\lambda + \mu + \nu + \rho + \sigma = 1;$$

to which,  $\lambda, \mu, \nu$ , &c. being *probabilities*, we may add

$$\lambda \geq 0, \quad \mu \geq 0, \quad \nu \geq 0, \quad \rho \geq 0, \quad \sigma \geq 0.$$

This combined system is, moreover, identical with the combined system (16), (17); and the mode of elimination being the same, it will furnish the same final relations among  $p, q, r$ , for the conditions of possible experience, as are furnished by (16), (17) for the conditions of algebraic solution of (15) in *positive*, and as we have afterwards shown, of (24) in *positive fractional* values of the variables involved.

Now there is nothing special in this reasoning. It is equally applicable to every form of  $V$ , and establishes the identity of the conditions of possible experience in the data of a question of probabilities, and those of mathematical consistency in the final processes by which the question is solved.

Other remarkable consequences, to which I shall but slightly advert, flow from these investigations.

Thus it follows from them that the numerical value of any probability determined by the general method will always, provided that the problem be a *real* one, satisfy those necessary and sufficient conditions of limitation, the mode of determining which I have illustrated in the August Number of this Journal (1854).

It follows hence, that two modes of procedure are open to us for the limiting of the solutions of questions of probability. We can either determine simultaneously the conditions of possible experience and those of final limitation, by the special method for this purpose developed in this Journal (August 1854), or, dispensing with this preliminary inquiry, we can so order the process of solution by the general method as to cause the different sets of values of the quantities  $s$ ,  $t$ , &c. to be distinctly evolved. Then, if among those sets there be found one that is wholly positive, the conditions of possible experience will be implicitly satisfied, and the final value of the probability sought will itself also satisfy the conditions of limitation to which the preliminary inquiry would have conducted us. It is obvious that the former is the easier mode of procedure.

I would beg, in conclusion, to observe that I have in the analytical portion of this paper aimed at little more than to give an account of researches not yet quite finished. The general proof of the properties of the functions  $K$  and  $\frac{V_z}{V}$ , I hope at no distant time to be able to complete and lay before the public. In the meanwhile I do not desire that they should be received with any greater confidence than the verifications actually furnished may seem to warrant. Of the function  $K$  it seems not improbable that other and important applications may yet be made. The conclusions to which these researches point is this,—that the theory which they are designed to test is a rigorously consistent one; that the conditions of its mathematical validity are identical with the conditions of possible experience. Now I apprehend that this character is *peculiar* to the theory under consideration. Upon no *merely* mathematical basis could a doctrine possessing such capacity of verification *à posteriori* rest, because other elements than that of number are involved in the inquiry. Some attention must be paid to the philosophy of lan-



guage; so much at least as is necessary in order to apprehend the distinction between whatsoever is transitory or accidental in its structure, and whatsoever belongs to its foundations in thought,—foundations not less deep, and not less solid, than those of geometry itself.

Queen's College, Cork.

XXVI. *Analysis of the Mineral "Edingtonite."*

By M. FORSTER HEDDLE, M.D., F.R.P.S.\*

**T**HE unsatisfactory nature of Dr. Turner's analysis of Edingtonite,—unsatisfactory as to the little insight it affords us into the composition of the mineral, on account of the large loss sustained,—has long made it desirable that a second analysis should be undertaken; the extreme rarity of the substance, however, has hitherto rendered this impossible.

Several years ago I was so fortunate as to obtain a specimen of this mineral, and by inducing the dealer from whom it was procured to revisit the locality, several others were found, and I was thus enabled to sacrifice one. I have little to add to the mineralogical characters already known.

The associated minerals are *analcime*, *harmotome* in twins, *calc-spar*, *rubin-glimmer*, *green-earth* in pseudomorphs after *calcite*, and *cluthalite*.

Specific gravity (determined on 130 grs.) 2·694.

Translucent to opaque.

Colour—white, grayish-white, pink.

Powdered mineral absorbs 1·614 per cent. of moisture.

Occurs both crystallized † and massive ‡.

On finding that the crystals were invariably associated with *harmotome*, I was led to suspect the presence of baryta in this mineral, and my conjecture was verified by the qualitative analysis, which showed silica, baryta, alumina, water, and traces of lime and soda.

The quantitative analysis (made on 25 grs.) afforded,—

	Per cent.	Oxygen.		
Silica . . .	36·98	19·229	21·14	21
Alumina . . .	22·63	10·555	11·62	12
Baryta . . .	26·84	2·724	3	3
Lime . . .	trace			
Soda . . .	trace			
Water . . .	12·46	11·077	12·12	12
	<u>98·91</u>			

\* Communicated by the Author.

† New figures, showing an additional face, will be given in the forthcoming "British Mineralogy" of Messrs. Greg and Lettsom.

‡ One specimen in my possession, partly massive, weighs 2½ oz.

which gives the formula—



the calculated per-centages of which are,—

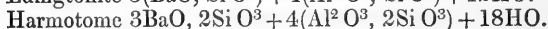
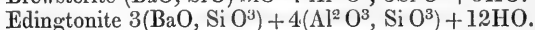
	Atoms.	Per cent.
Silica . . . .	7	37·274
Alumina . . . .	4	23·751
Baryta . . . .	3	26·514
Water . . . .	12	12·461

In another analysis I got ·08 of strontia (?) and ·22 of lime.

For the sake of comparison, Dr. Turner's analysis is appended in a foot note\*; it will be observed that the *lime* and *loss* taken together approximate the quantity of *baryta* obtained in the new analysis.

I would account for Dr. Turner's mistaking baryta for lime, and also so far for the loss sustained, by supposing that, from having merely a few chips of the mineral, he was unable to institute a preliminary qualitative analysis; and that he (under the impression that, like most zeolites, it would be found to contain lime) added oxalate of ammonia, and so got the partially soluble oxalate of baryta, which in the washing passed to a great extent through the filter, and if it was again noticed, was set down as "probably potash or soda." I would explain the mistake thus, because I believe, not only that Dr. Turner was a most correct analyst, but that he never willingly shirked any of the intricacies; at the same time a readier explanation might be offered in the fact, that at this time baryta was *often* taken for lime. Dr. Thomson, for instance, was detected by Connel mistaking baryta and strontia for lime in *Brewsterite*, and by Damour in a similar mistake with regard to "*Morvenite*."

In virtue of its composition, as above stated, Edingtonite will take a most interesting place in the system, ranking with harmotome and Brewsterite as the third barytic zeolite; thus



The original specimen of this mineral was stated to have occurred in cavities of *Thomsonite*. I have never seen it thus associated.

Before leaving *Edingtonite*, we would venture to throw out the

\* By Turner:—

Silica . . . .	35·09
Alumina . . . .	27·69
Lime . . . .	12·68
Water . . . .	13·32
Loss . . . .	11·22

hint that the "*Glottalite*" of Thomson, of which only one specimen has been found, and about which nothing is known, may turn out to be nothing but an impure mixture of *Edingtonite* and *harmotome* (which, as we have stated, invariably accompanies it); the locality and characters are much the same. Turner mistook lime for barytes, and why not Thomson, who had before made the same mistake. Granting this error, both contain the same elements; the quantity of silica, which no one could mistake, agrees, and a person not very conversant with crystallography might fancy he saw both octohedrons and cubes in the faces of *Edingtonite*, especially when the "crystals were so mixed together that only a small portion of each could be seen." We may also mention that "*Glottalite*" was procured from the same dealer from whom we purchased our *Edingtonites*.

XXVII. *On the Law of Absorption of Gases*. By R. BUNSEN.

[Concluded from p. 130.]

9. *Oxygen in Water*.

**O**XYGEN gas, prepared in the usual manner from chlorate of potash, gave,—

No.	° C.	Coefficient.
1	6·0	0·04609
2	8·3	0·04186
3	11·6	0·03921
4	18·1	0·03715
5	22·8	0·03415

During the agitation in the absorptiometer the water became turbid from the formation of a black powder, and it was feared that the metals dissolved in the mercury had been oxidized at the expense of the oxygen in the water, and hence too large a coefficient obtained. The mercury employed was therefore purified from all foreign metals, as perfectly as several long digestions with concentrated nitric acid would permit. The experiment conducted with the purified mercury gave the following results:—

No.	° C.	Coefficient.	Difference.
1	19·4	0·03109	
2	19·6	0·03199	+0·00090
3	19·4	0·03202	+0·00003
4	19·5	0·03254	+0·00052
5	19·5	0·03245	—0·00009
6	19·5	0·03292	+0·00047
7	19·0	0·03513	+0·00221
8	19·0	0·03456	—0·00057

The experiment gave therefore, in fact, rather a smaller coefficient. In spite, however, of the most careful purification, the mercury always caused a black turbidity in the water, which perceptibly increased with agitation. This circumstance, and the fact that the coefficients determined one after the other in the same liquid and at the same temperature always regularly increased, showed that this method was not to be relied upon for exact results. I have preferred, therefore, to determine the coefficients by an indirect method.

If atmospheric air perfectly free from carbonic acid and ammonia be passed into boiled water, the amount of oxygen absorbed  $V_0$ , and the amount of absorbed nitrogen  $V$ , is found from the following equations formerly deduced:—

$$V_0 = \frac{\alpha P O V_1}{0.76(N + O)}; \dots \dots \dots (3)$$

$$V = \frac{\beta P N V_1}{0.76(N + O)}. \dots \dots \dots (4)$$

The first of these equations divided by the second gives  $\frac{N V_0}{O V} \beta = \alpha$ . As the composition of atmospheric air, *i. e.* the proportion  $\frac{N}{O}$ , as well as the absorption-coefficient  $\beta$  of nitrogen is known, we only require to determine the proportion  $\frac{V_0}{V}$ , or the composition of the air dissolved in the water, in order to calculate  $\alpha$ , or the absorption-coefficients of oxygen. The elements for this calculation are found in the following experiments. The air, perfectly freed from carbonic acid and ammonia, was passed in a strong stream for half a day through the water, previously well boiled, which was kept at a constant temperature by immersion in a water-bath. The purification of the water must be conducted with the greatest care. It must not be distilled in any vessel previously used for any organic preparation, as the slightest trace of volatile organic matter present is sufficient to convert a part of the oxygen into carbonic acid. It is therefore necessary to prove the absence of carbonic acid in the air boiled out from the water by a special experiment. The gases dissolved in this water were collected by a method which I made use of in my researches upon the gases of the islandic springs, and more fully described by Dr. Baumert, in his admirable research upon the respiration of the *Cobitis fossilis*.

*Air from Water saturated at 1° C.*

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Volume of gas employed ...	216·85	0·2614	9·7	55·374
+ hydrogen .....	376·16	0·4170	9·8	151·414
After the explosion .....	289·83	0·3340	9·9	93·420

Oxygen . . . . . 34·91  
 Nitrogen . . . . . 65·09  
100·00

*Air from Water saturated at 13° C.*

	Volume.	Pressure*.	° C.	Vol. at 0° and 1 m.
Volume of gas employed ...	165·99	0·2198	9·0	53·324
+ hydrogen .....	346·28	0·3914	9·5	130·994
After the explosion .....	288·78	0·3371	9·2	94·18

Oxygen . . . . . 34·73  
 Nitrogen . . . . . 65·27  
100·00

*Air from Water saturated at 23° C.*

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Volume of gas employed ...	206·67	0·2577	9·3	51·497
+ hydrogen .....	400·29	0·4437	9·2	171·828
After the explosion .....	323·95	0·3746	8·7	117·608

Oxygen . . . . . 35·08  
 Nitrogen . . . . . 64·92  
100·00

From these experiments, it is seen that the composition of the air dissolved in water at various temperatures is always constant. The mean composition is,—

Oxygen . . . . . 34·91 =  $V_0$   
 Nitrogen . . . . . 65·09 =  $V$   
100·00

The curve which represents the increase of the absorption-

\* In all statements of pressure in this research the correction for tension of the vapour of water is included.

coefficient of oxygen for decreasing temperatures is therefore parallel to the corresponding curve for nitrogen.

If we take the following as the true composition of atmospheric air,—

Oxygen . . . .	0·2096=O
Nitrogen . . . .	0·7904=N
	<u>1·0000</u>

and substitute the values of  $V_1$ ,  $V$ ,  $O$ , and  $N$  in the former equation, we obtain the value of the absorption-coefficient of oxygen  $\alpha = 2·0225\beta$ , when  $\beta$  is the absorption-coefficient of nitrogen.

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·04114	0·00107	7	0·03465	0·00079	14	0·03034	0·00048
1	0·04007	0·00100	8	0·03389	0·00076	15	0·02989	0·00045
2	0·03907	0·00097	9	0·03317	0·00072	16	0·02949	0·00040
3	0·03810	0·00093	10	0·03250	0·00067	17	0·02914	0·00035
4	0·03717	0·00089	11	0·03189	0·00061	18	0·02884	0·00030
5	0·03628	0·00084	12	0·03133	0·00056	19	0·02858	0·00026
6	0·03544		13	0·03082	0·00051	20	0·02838	0·00020

### 10. Atmospheric Air in Water.

We have seen in the former part of the paper that the relative proportion in which the constituents of a mixture of gases are absorbed by water does not alone depend upon their several absorption-coefficients, but also upon the relative proportions in which they are mixed. If the absorption-coefficients are different, the gases dissolved in the water are not in the same relation as those in the free gas. This last undergoes therefore a change in its composition, which varies with the relation of the mass of the water to that of the gas. For this reason the absorption-coefficient of a mixed gas can only be calculated from the relative proportions of the constituents and their several absorption-coefficients, when the volume of the gas is so great in comparison with the mass of the absorbing liquid, that the alteration effected by the absorption in the composition of the residual gas is inappreciable. The true absorption-coefficient of air is therefore found only in those phenomena of absorption in which these conditions are fully carried out.

If we take, as above, for the mean atmospheric composition,—

Oxygen . . . .	0·2096=O
Nitrogen . . . .	0·7904=N
	<u>1·0000</u>

by substituting the value 1 for  $N + O$ , and for  $V$  and for  $P$  the value 0·76, and lastly, for  $N$  and  $O$  the above values, we obtain

the required coefficient  $c$  for air and water from the following equation:—

$$V_0 = \frac{\alpha PO V_1}{0.76(N+O)}, \text{ and } V = \frac{\beta PNV_1}{0.76(N+O)},$$

which give

$$c = 0.2096\alpha + 0.7904\beta.$$

The following table is calculated by the aid of this formula:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0.02471	0.00065	7	0.02080	0.00048	14	0.01822	0.00029
1	0.02406	0.00061	8	0.02034	0.00046	15	0.01795	0.00027
2	0.02345	0.00058	9	0.01992	0.00042	16	0.01771	0.00024
3	0.02287	0.00055	10	0.01953	0.00039	17	0.01750	0.00021
4	0.02232	0.00053	11	0.01916	0.00037	18	0.01732	0.00018
5	0.02179	0.00051	12	0.01882	0.00034	19	0.01717	0.00015
6	0.02128		13	0.01851	0.00031	20	0.01704	0.00013

Fig. 5 (Plate I.) gives a graphic representation of the solubility in water of some of the gases examined.

The following absorption-coefficients of chlorine, sulphurous acid, and sulphuretted hydrogen, were not determined in the absorptiometer, but by chemical methods, which will be fully explained by Dr. Schönfield in his research upon this subject.

° C.	SO <sub>2</sub> .	SH.	Cl.	° C.	SO <sub>2</sub> .	SH.	Cl.
0	68.861	4.3706		21	34.986	2.8430	2.1148
1	67.003	4.2874		22	33.910	2.7817	2.0734
2	65.169	4.2053		23	32.847	2.7215	2.0322
3	63.360	4.1243		24	31.800	2.6623	1.9912
4	61.576	4.0442		25	30.766	2.6041	1.9504
5	59.816	3.9652		26	29.748	2.5470	1.9099
6	58.080	3.8872		27	28.744	2.4909	1.8695
7	56.369	3.8103		28	27.754	2.4357	1.8295
8	54.683	3.7345		29	26.780	2.3819	1.7895
9	53.021	3.6596		30	25.819	2.3290	1.7499
10	51.383	3.5858	2.5852	31	24.873	2.2771	1.7104
11	49.770	3.5132	2.5413	32	23.942	2.2262	1.6712
12	48.182	3.4415	2.4977	33	23.025	2.1764	1.6322
13	46.618	3.3708	2.4543	34	22.122	2.1277	1.5934
14	45.079	3.3012	2.4111	35	21.234	2.0799	1.5550
15	43.564	3.2326	2.3681	36	20.361	2.0332	1.5166
16	42.073	3.1651	2.3253	37	19.502	1.9876	1.4785
17	40.608	3.0986	2.2828	38	18.658	1.9430	1.4406
18	39.165	3.0331	2.2405	39	17.827	1.8994	1.4029
19	37.749	2.9687	2.1984	40	17.013	1.8569	1.3655
20	36.216	2.9053	2.1565				

The coefficients of absorption of a number of gases having been thus obtained, we are enabled to verify experimentally the formulæ which were deduced from Dalton's hypothesis.

If a volume of a simple gas, whose absorption-coefficient is  $\alpha$ ,

be twice absorbed from the same volume  $h$  of water, at the same temperature, but under different pressures  $P$  and  $P_1$ , the amount of gas absorbed in the two cases is, according to formula (2),—

$$g = \frac{\alpha Ph}{0.76}, \text{ and } g_1 = \frac{\alpha P_1 h}{0.76}.$$

Hence follows

$$\frac{g}{g_1} = \frac{P}{P_1}.$$

The following determinations made with the absorptiometer show, within the limit of the experimental errors, that the amount of carbonic acid  $g_n$  absorbed in the same volume of water at the same temperature increases proportionally to the corresponding pressures  $P_n$ .

*Carbonic Acid at 19°·9 C.*

No.	P.	g.	$\frac{P}{P_1}$ .	$\frac{g}{g_1}$ .
1	0.7255	38.61		
2	0.5245	27.24	1.38	1.42
3	0.5237	27.08	1.39	1.43
4	0.5231	27.28	1.39	1.42

*Carbonic Acid at 3°·2 C.*

No.	$P_n$ .	$g_n$ .	$\frac{P}{P_1}$ .	$\frac{g}{g_1}$ .
1	0.5244	31.41		
2	0.6467	38.66	0.8109	0.8125
3	0.6470	38.49	0.8105	0.8161

This ratio between the absorbed gaseous volume and the pressure is more clearly seen in cases in which the partial pressures occur, *i. e.* in which alterations in the pressure are effected by dilution with another gas. The formula (14) gives a convenient statement of this relation,

$$\frac{x}{x+y} = \frac{W-B}{A-B} \cdot \frac{A}{W}.$$

By means of this formula the composition of a mixture of two gases can be calculated when the following quantities are given:  $\alpha_1$  the absorption-coefficient of the first gas,  $\beta_1$  that of the second;  $V$  the common volume of both gases before the absorption under the pressure  $P$ ;  $V_1$  the residual volume after the absorption under the pressure  $P_1$ ; and lastly, the volume  $h$  of the absorbing water. If the composition of the mixture calculated from these experimental elements coincides with that found directly by



eudiometrical analysis, it may be concluded that the formulæ based upon the original premises are true, and that the law is not only applicable for total, but also for partial pressures.

Mixtures of carbonic acid and hydrogen were employed in the following experiments:—

*Experiment I.*

*Eudiometric determination.*

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Hydrogen .....	120.6	0.7214	13.6	82.87
After addition of carbonic acid.	129.4	0.7269	13.5	89.63

Composition of the gas in 100 parts:—

Hydrogen . . . .	92.46
Carbonic acid . . .	7.54
	100.00

*Absorptiometric Determination of the same Gas.*

	Volume.	Pressure.	° C.	Vol. at 0°.
Gas employed.....	180.94	0.5368	15.4	171.29
After absorption.....	122.01	0.6809	5.5	119.61

Volume of absorbing water . = 356.7  
356.1

Mean . . . . 356.4.

From this the following elements of the calculation are obtained:—

$P=0.05368; P_1=0.6809; \alpha=1.4199; h=356.4;$

$V=171.29; V_1=119.61; \beta=0.0193;$

and hence the composition,—

	Absorptiometric.	Eudiometric.
Hydrogen . . . .	0.9207	$0.9246 = v_1$
Carbonic acid . .	0.0793	$0.0754 = v$
	1.0000	1.0000

By the help of the formulæ,

$$u = \frac{x}{x+y} = \frac{AB_1}{AB_1 + A_1B'} \quad u_1 = \frac{y}{x+y} = \frac{A_1B}{AB_1 + A_1B'}$$

the composition of the gas remaining after absorption over the water is obtained.

Hydrogen . . .	0·9829
Carbonic acid . . .	0·0171
	1·0000

As the gaseous mixture after absorption was subjected to a pressure  $P_1=0^m\cdot6809$ , the partial pressure upon the carbonic acid was in this experiment  $uP_1=0^m\cdot0116$ , that of the hydrogen  $u_1P_1=0^m\cdot6692$ .

*Experiment II.*  
*Eudiometric determination.*

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Hydrogen .....	62·2	0·6449	7·5	39·03
After addition of carbonic acid.	82·1	0·6649	6·4	53·34

Composition of the gas:—

Hydrogen . . .	0·7319
Carbonic acid . . .	0·2681
	1·0000

*Absorptiometric Analysis of the same Gas.*

	Vol. at 0°.	Pressure.	° C.
Gas employed.....	119·03	0·4951	6·8
First absorption .....	72·02	0·6116	5·1
Second absorption .....	60·39	0·7297	12·8
Third absorption .....	75·71	0·6020	23·3

Volume of absorbing water:—

- 206·83
- 206·61
- 206·61
- 207·11

Mean . . . . 206·79

Hence follows for the first absorption,—

$$P=0\cdot4951; P_1=0\cdot6116; a=1\cdot4434; h=206\cdot79;$$

$$V=119\cdot03; V_1=72\cdot02; \beta=0\cdot0193.$$

For the second absorption:—

$$P=0\cdot4951; P_1=0\cdot7297; a=1\cdot0726; h=206\cdot79;$$

$$V=119\cdot03; V_1=60\cdot39; \beta=0\cdot0193.$$

For the third absorption:—

$$P=0\cdot4951; P_1=0\cdot6020; a=0\cdot8555; h=206\cdot79;$$

$$V=119\cdot03; V_1=75\cdot71; \beta=0\cdot0193.$$

The calculation of the first absorption gives,—

	Absorptiometric.	Eudiometric.
Hydrogen . . . . .	0·7343	0·7319
Carbonic acid . . . . .	0·2657	0·2681
	1·0000	1·0000

The composition of the residual gas remaining after absorption, calculated from the eudiometric analysis, is,—

Carbonic acid . . . . .	0·0699
Hydrogen . . . . .	0·9301
	1·0000

Hence the partial pressure of the hydrogen is 0<sup>m</sup>·5688, and of the carbonic acid 0<sup>m</sup>·04275.

From the second absorption experiment we obtain,—

	Absorptiometric.	Eudiometric.
Hydrogen . . . . .	0·7372	0·7319
Carbonic acid . . . . .	0·2628	0·2681
	1·0000	1·0000

Also the residual gas after absorption :—

Carbonic acid . . . . .	0·07712
Hydrogen . . . . .	0·92288
	1·00000

Hence follow the partial pressures under which the absorption took place, for the carbonic acid 0<sup>m</sup>·0563, for the hydrogen 0<sup>m</sup>·6734. The third absorption gives the following composition :—

	Absorptiometric.	Eudiometric.
Hydrogen . . . . .	0·7285	0·7319
Carbonic acid . . . . .	0·2715	0·2681
	1·0000	1·0000

This gives a residual gas of the composition,—

Carbonic acid . . . . .	0·1036
Hydrogen . . . . .	0·8964
	1·0000

The pressure of the carbonic acid is here 0<sup>m</sup>·06236, and of the hydrogen 0<sup>m</sup>·5396.

The mean from these three determinations, compared with the eudiometric analysis, gives,—

	Absorptiometric analysis.	Eudiometric analysis.
Hydrogen . . . . .	26·67	26·81
Carbonic acid . . . . .	73·33	73·19
	100·00	100·00

How far beyond the examined limits the law may be considered as true cannot be *à priori* determined. It is, however, more than probable that in this law, as in the law of Mariotte, a limit exists beyond which the regularity of the action is disturbed by varying molecular influences. The limits of the exact action of the law are however quite extensive enough to enable us to draw some very interesting conclusions from the subject. Eudiometry, for example, gains from the law of absorption an entirely new sphere of action, enabling it not only to determine, without any chemical experiments, the simple or complex constitution of a gas, but also to recognize the nature of the component parts, even indeed to estimate their several proportions, when once for all the absorption-coefficients of the gases are known. To show that such an absorptiometric determination can serve as a reaction for the detection of gases, I chose an experiment with marsh gas, which satisfactorily proves that results are attainable even when the values of the absorption-coefficients employed in the calculation differ but little from each other.

Relying on the results of eudiometrical analysis, it has been hitherto supposed that the gas obtained by the action of a hydrated alkali upon an alkaline acetate at a high temperature was marsh gas. Although this supposition has scarcely ever been questioned, still all positive proof of the fact fails. Frankland and Kolbe have shown that two volumes of marsh gas by eudiometrical explosion react exactly as a mixture of equal volumes of hydrogen and methyle. Both give for every volume a volume of carbonic acid, and require for their combustion the double amount of oxygen. Eudiometric analysis leaves it then undecided whether the gas evolved from the alkaline acetates is to be considered as marsh gas, or as a mixture of methyle and hydrogen. By means of absorptiometric analysis this question is very readily and surely answered. If we start from the supposition that the gas in question is a mixture of equal volumes of methyle and hydrogen, a volume  $V$  of the gas, at  $0^\circ$  and under  $0.76$  pressure, measured in the absorptiometer would consist under the pressure  $0.76$  of,—

$$\frac{PV}{2 \times 0.76} \text{ methyle, and } \frac{PV}{2 \times 0.76} \text{ hydrogen.}$$

If this gas be agitated with  $h_1$  volumes of water, the observed volume of the residual unabsorbed gas being  $V_1$  under the pressure  $P_1$ , the sum of the absorption-coefficients at the temperature of absorption (for hydrogen  $\alpha_1$  and for methyle  $\beta_1$ ) can be calculated from the observations. If we call the residual hydrogen  $x_1$ , and the residual methyle  $y_1$  (both reduced to  $0^\circ$  and  $0.76$ ), this  $x_1$  will, in consequence of its dilution with methyle, be

subject to the partial pressure  $\frac{x_1 \cdot 0.76}{V_1}$ . It is however absorbed under this pressure by the volume  $h_1$  of water. The absorbed volume of hydrogen, reduced to 0.76 pressure and 0° C., is therefore, according to the law of absorption,  $\frac{x_1}{V_1} \alpha_1 h_1$ . This absorbed hydrogen + unabsorbed  $x_1$  is equal to the hydrogen originally present, namely,—

$$\frac{PV}{2 \times 0.76} = x_1 \left( 1 + \frac{\alpha_1 h_1}{V_1} \right) \text{ or } x_1 = \frac{PV}{2 \times 0.76 \left( 1 + \frac{\alpha_1 h_1}{V_1} \right)}$$

If the value of  $x_1$  is substituted in the expression  $\frac{0.76x_1}{V_1}$ , we obtain for the pressure of the hydrogen in the residual gas  $\frac{PV}{2(V_1 + \alpha_1 h_1)}$ , and for the pressure of the methyle by similar considerations  $\frac{PV}{2(V_1 + \beta_1 h_1)}$ .

It follows, however, from the law of absorption, that the sum of the two pressures is equal to the observed pressure  $P_1$ . We have then,—

$$P_1 = \frac{PV}{2(V_1 + \alpha_1 h_1)} + \frac{PV}{2(V_1 + \beta_1 h_1)}$$

or,

$$\alpha_1 + \beta_1 = \frac{PV}{2P_1 h_1} - \frac{2V_1}{h_1} \dots \dots (19)$$

An experiment made by Dr. Pauli with a gas prepared by heating acetate of potash with hydrate of potash, carefully freed from claye and carbonic acid with fuming sulphuric acid and potash, gave the following elements for calculation:—

Original volume of gas reduced to 0°	V = 116.42
The pressure on this volume	P = 0.5065
Volume of gas employed reduced to 0° after first absorption	V <sub>1</sub> = 75.18
Corresponding pressure	P <sub>1</sub> = 0.6615
Volume of absorbing water	h <sub>1</sub> = 318.11
Temperature of the experiment	12°·8 C.
Absorption-coefficient of hydrogen at 12°·8	α <sub>1</sub> = 0.01930
Absorption-coefficient of methyle gas at 12°·8	β <sub>1</sub> = 0.05446
Absorption-coefficient of marsh gas at 12°·8	γ <sub>1</sub> = 0.04106
The volume after second absorption reduced to 0°	V <sub>n</sub> = 79.04
Corresponding pressure	P <sub>n</sub> = 0.6561
Volume of absorbing water	h <sub>n</sub> = 325.05
Temperature of experiment	t <sub>n</sub> = 24°·6
Absorption-coefficient of hydrogen at 24°·6	α <sub>n</sub> = 0.01930
Absorption-coefficient of methyle at 24°·6	β <sub>n</sub> = 0.04181
Absorption-coefficient of marsh gas at 24°·6	γ <sub>n</sub> = 0.03166

When this is calculated, negative values for  $\alpha_1 + \beta_1$  and for  $\alpha_n + \beta_n$  are found from both absorption experiments, namely,  $-0.3325$  and  $-0.34807$ , instead of the sums of the coefficients found in the tables for methyle and hydrogen,  $+0.07376$  and  $+0.06111$ . Hence the gas in question cannot consist of equal volumes of hydrogen and methyle.

If, on the contrary, the same elements are used in the calculation of  $\gamma_1$  and  $\gamma_n$ , under the supposition that the gas is a simple one, two absorption-coefficients are obtained, which are almost exactly the same as those found in the table for marsh gas, at the temperatures  $12^\circ.8$  C. and  $24^\circ.6$  C. The formula

$$\frac{VP}{h_1 P_1} - \frac{V_1}{h_1} = \gamma_1 \quad \dots \quad (1)$$

gives for the absorption at  $12^\circ.8$   $\gamma_1 = 0.0439$  instead of the actual value  $0.0411$ , and for the absorption at  $24^\circ.6$   $\gamma_n = 0.0333$  instead of  $0.03166$ . From this agreement, we may conclude that the marsh gas prepared from acetate of potash is neither a mixture of hydrogen and methyle, nor a substance isomeric with natural marsh gas, but that it is actually the same substance which streams from the mud-volcanoes of Bulganak in the Crimea.

Any general reaction to distinguish between the constituents of a gaseous mixture has hitherto been wanting. The quantitative composition of a gas obtained by eudiometrical analysis, depends almost entirely upon certain suppositions concerning its qualitative constitution. If, for instance, eudiometrical analysis points out the presence of marsh gas, it remains quite undecided, as I have just shown, whether this gas is a mixture of equal volumes of methyle and hydrogen. If analysis shows the presence of a mixture of marsh gas and hydrogen, it is uncertain whether we are not experimenting upon mixtures of methyle and hydrogen, or of methyle, marsh gas, and hydrogen. All analyses in which the two latter gases occur together may be calculated according to either of these assumptions, without it having been hitherto possible to prove the accuracy of either one.

It is easy, by means of the law of absorption, to remove these doubts, for the absorption-coefficients serve as the reagents which fail in gas analysis, and they present also the peculiarity, that they not only show the qualitative, but at the same time the quantitative composition of the gas. Let us, for example, suppose that an unknown gas be mixed in an unknown volume  $x$ , with an unknown volume  $y$  of another unknown gas, we can then, by means of three absorptiometric experiments, determine, —1st, what gases are present in the mixture, 2nd, in what proportions they occur.

The following is the method of solving this problem:—A sufficient quantity of the gas to be examined is collected in the absorptiometer, and its volume, pressure and temperature observed.

If the originally observed volume reduced to 0° be called V, and its pressure P, we obtain the equation

$$1 = \frac{x}{VP} + \frac{y}{VP} \dots \dots \dots (6)$$

Three absorptions of the gas are made with the volumes of water  $h_1, h_2$  and  $h_3$ , and the corresponding volumes and pressures reduced to 0°,  $V_1P_1, V_2P_2, V_3P_3$ , observed for a constant temperature  $t$ . From these determinations, according to equation (6), we obtain the following, in which  $\alpha$  signifies the absorption-coefficient of the first gas, and  $\beta$  that of the second at the temperature  $t$ :—

$$1 = \frac{x}{(V_1 + \alpha h_1)P_1} + \frac{y}{(V_1 + \beta h_1)P_1},$$

$$1 = \frac{x}{(V_2 + \alpha h_2)P_2} + \frac{y}{(V_2 + \beta h_2)P_2},$$

$$1 = \frac{x}{(V_3 + \alpha h_3)P_3} + \frac{y}{(V_3 + \beta h_3)P_3}.$$

From these four equations the unknown quantities  $x, y, \alpha$ , and  $\beta$  are easily found. The two last are the ordinates of absorption of two gases for the temperature-abcissa  $t$ . If the numerical values of these are calculated, it is easy to find in the table the gas which has the same absorption-coefficient for corresponding temperatures, and thus the nature of the mixture is determined. The values of  $x$  and  $y$  give also the relative proportion between the constituents. The determination of  $\alpha$  and  $\beta$  is, in the case of two gases, not difficult. If we place  $PV = a, P_1V_1 = a_1, P_2V_2 = a_2, P_3V_3 = a_3$  and  $P_1h_1 = b_1, P_2h_2 = b_2, P_3h_3 = b_3$ , we obtain first,—

$$\alpha + \beta = \frac{a_1b_2b_3(a - a_1)(b_2 - b_3) - a_2h_1b_3(a - a_2)(b_1 - b_3) + a_3b_1b_2(a - a_3)(b_1 - b_2)}{b_1b_2b_3[a_1(b_2 - b_3) - a_2(b_1 - b_3) + a_3(b_1 - b_2)]}$$

$$\alpha\beta = \frac{a_2b_3(a - a_2) - a_3b_2(a - a_3) - b_2b_3(a_2 - a_3)\Lambda}{b_2b_3(b_2 - b_3)},$$

and when the expressions on the right of these equations be represented by  $\Lambda$  and  $B$ ,—

$$\alpha + \beta = \Lambda \dots \dots \dots (20)$$

$$\alpha - \beta = \pm \sqrt{\Lambda^2 - 4B} \dots \dots \dots (21)$$

The sum of these two equations gives the value of  $\alpha$ , their difference that of  $\beta$ .

By help of these values  $\alpha$  and  $\beta$ , we obtain, lastly,—

$$y = \frac{(a_2 - a + \alpha b_2)(a_2 + \beta b_2)}{b_2(\alpha - \beta)} \dots \dots \dots (22)$$

$$x = a - y \dots \dots \dots (23)$$

As an example of such a calculation, I select the qualitative and quantitative determination of the gas prepared by heating oxalic acid with concentrated sulphuric acid. As this gas always contains small quantities of sulphurous acid mixed with it, it was first passed through water in which binoxide of manganese was suspended, and the gas collected after the water had been saturated, and the atmospheric air contained in the apparatus displaced. A eudiometric analysis of the purified gas gave,—

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Original volume.....	142.9	0.6965	20.2	92.70
After absorption of carbonic acid	74.6	0.6637	19.0	46.29

	Found.	Calculated.
Carbonic oxide . . .	50.06	50.00
Carbonic acid . . .	49.94	50.00
	<u>100.00</u>	<u>100.00</u>

The absorptiometric determination, which was conducted in such a manner that the amount of absorbing water was increased after every observation, gave the following element:—

	Vol. at 0°.	Pressure.	° C.	Volume of water— <i>h</i> .
Gas employed .....	500.8	0.5760	19.0	0
After the first absorption .....	384.0	0.6882	19.0	81.6
After the second absorption.....	340.0	0.7015	19.2	186.9
After the third absorption .....	283.3	0.7415	19.0	335.5

From these elements, the value of  $\beta$  is found by means of formulæ (20) and (21) to be 0.9248. The tables of absorption show that carbonic acid gas has for the observed temperature a corresponding absorption-coefficient; for the table gives for this gas the number 0.9150, which closely approximates to the experimental coefficient  $\beta$ . We conclude from the constitution of a precipitate, from its colour, from its solubility, &c., that a certain substance is present. It is here not a precipitate, but a certain ordinate of a solubility-curve which serves as our test. But as there are certain precipitates produced by different substances resembling each other so closely that they cannot be



used as distinguishing tests, it is possible that these ordinates may differ so little by the temperatures at which the curves of absorption approach, cut, or are tangent to each other, that a second absorptiometric experiment is necessary. The foregoing experiments, for example, give a value for  $\alpha$ , at the temperature  $19^\circ$ , of 0.0204: this varies very slightly from the absorption-coefficient of carbonic oxide at  $19^\circ$ , which is found in the table to be 0.0233. The absorption-coefficient of hydrogen at  $19^\circ$  is, however, 0.0193, and that of æthyle gas at the same temperature is 0.0207, and these differ so slightly from the number found for carbonic oxide, that it must remain doubtful which gas is present. In this case the number derived from an experiment of absorption in water, is comparable to a reagent which indicates the presence of a group of substances. It only remains therefore to determine by further absorptiometric experiments, either at different temperatures or with other liquids, which of the gases indicated by the first experiment is really present. The determination of the absorption-coefficients of gases for alcohol, for solutions of salts and other liquids, forms therefore an important element in gas analysis, as from these any number of conditional equations may be obtained, each of which possesses the importance and value of a new reagent.

If the material nature of the gas has been determined by the method described by means of  $\alpha$  and  $\beta$ , it is only necessary to substitute the values for  $\alpha$  and  $\beta$  in the equations (22) and (23), in order to find the quantitative relation in which the two gases are mixed.

If this calculation is made for the above experiment with the values of  $\alpha$  and  $\beta$  as found in the absorption tables for carbonic acid and carbonic oxide, we obtain—

	Eudiometrical.	Absorptiometrical.			
		1st Exp.	2nd Exp.	3rd Exp.	Mean.
Carbonic acid.....	50.06	50.00	50.03	50.34	50.12
Carbonic oxide .....	49.94	50.00	49.97	49.66	49.88
	100.00	100.00	100.00	100.00	100.00

The same elements which have served to determine the qualitative nature of the mixture of gases, give therefore the quantitative composition with a degree of exactness scarcely surpassed by eudiometric analysis.

Another problem which may be easily solved by means of the absorptiometer, concerns the alterations which a mixture of gases undergoes by contact with water. The following example of a similar mixture of carbonic acid and carbonic oxide shows

how considerable such alterations, even with relatively small amounts of liquid, may become under certain circumstances, and to what considerable errors those eudiometric experiments may be subject in which the gases are confined over water, or liquids used as a means of absorption.

For this experiment, the mixture from oxalic acid and sulphuric acid was again employed. The elements for the calculation of formulæ (14) and (15), are taken from an experiment made with the absorptiometer by Dr. Atkinson; they are as follows:—

$$V = 388.4; P = 0.6557; h_1^* = 315.3; \alpha = 0.9124;$$

$$V_1 = 247.69; P_1 = 0.7395; \alpha_1^\dagger = 0.02326.$$

The following composition of the gas employed is obtained by substituting these values in formula (15):—

	Absorptiometric.	Theory.
Carbonic acid . . .	49.55	50.00
Carbonic oxide . . .	50.45	50.00
	100.00	100.00

The absorptiometric analysis gives therefore again the values—

$$V = 0.4955 \text{ and } V_1 = 0.5045.$$

From these is obtained, by means of formulæ (12) and (13), the following composition for the residual unabsorbed gas:—

Carbonic acid . . . .	31.87
Carbonic oxide . . . .	68.13
	100.00

A eudiometrical analysis of this residual gas, made by Dr. Atkinson, agrees sufficiently with this calculated composition. He found,—

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Original volume .....	96.1	0.6721	18.7	60.45
After absorption of carbonic acid	68.0	0.6556	19.3	41.64

Carbonic acid . . . .	31.12
Carbonic oxide . . . .	68.88
	100.00

The quantity of carbonic oxide contained in the mixture has therefore increased from 50.45 to 68.88, although the volume of the absorbing water was not so large as that of the gas.

The phenomena which accompany the evolution of gas in mineral springs, can only be fully understood by the help of the law of absorption. Among the non-alkaline springs, containing

\* Called  $h$  in formula (5).

† Called  $\beta$  in formula (5).

but a small quantity of dissolved salts, there are some whose absorption-coefficients differ but slightly from those of pure water, and contain carbonic acid gas alone in solution. If such springs, as is usually the case, are saturated with gas, a certain limit for the amount of contained carbonic acid may be found. This limit of the quantity of carbonic acid depends upon,—1st, the temperature of the spring; 2nd, the depth of the shaft of the spring; 3rd, the height of the spring above the sea. Springs of the above sort, which are saturated with a stream of chemically pure carbonic acid, and rise without pressure at the level of the sea, give according to their temperature very different amounts of gas. They contain in one litre of water the following amounts of gas for the corresponding temperatures:—

°C.	Cubic centimetres of gas in 1 litre of water.
0	1796·7
5	1449·7
10	1184·7
15	1002·0
20	901·4

If the same spring under otherwise similar circumstances rose at an elevation above the sea, where the average atmospheric pressure was only two-thirds of the mean height of the barometric column, it would contain only two-thirds of the above amount of dissolved carbonic acid.

Hence it will be perceived that the amount of gas in a spring which is saturated with pure carbonic acid, may be considerably augmented by deepening the spring shaft, and thus increasing the column of water under which the gas issues from the earth, as Bischoff has indeed already shown in his admirable researches on the phenomena of springs. If, for example, the depth of the shaft from the surface of the spring to the ground is 15 feet, the water where it bubbles out from the earth will contain one-third more carbonic acid than the above amounts show. The water in rising to the surface loses a part of the dissolved gas in proportion as the pressure diminishes, but the statical equilibrium which ensues in consequence of the law of absorption requires a certain time for its restoration. Thus the Peters spring in Petersthal in the Schwarzwald, which has a temperature of 10° C., contains at the surface of the spring, under a pressure 0<sup>m</sup>.735, 1270·4 cubic centimetres of carbonic acid in the litre, whereas, according to the absorption-coefficient of carbonic acid for 10°·3, it should only contain 1133·3 cubic centimetres under the same pressure. The water is therefore over-saturated with carbonic acid. This excess of gas is seen to

escape in small bubbles from the water when a vessel filled at the spring is allowed to stand. By agitation the equilibrium is restored in a few moments, and the gas dissolved in the water reduced to its normal amount. By similar considerations, it is easy to see that many of the statements, with regard to the amount of carbonic acid contained in springs, must be false. Thus, for instance, the amount of carbonic acid contained in the "Fürsten Quelle" in Imnau, is given by Sigwart to be 2500 cubic centimetres in the litre. Under the mean pressure and at the temperature of the spring  $6^{\circ}3$  C., the water can, however, according to the law of absorption, only contain  $1373\cdot2^*$  cubic centimetres after the equilibrium has been established. The amount of gas, 2500, requires a pressure of  $1^m\cdot3836$  of mercury, or a column of water of  $8^m\cdot449$ , to be added to the mean barometric pressure. As, however, it is impossible to suppose that the Imnau spring rises under the pressure of a column of water at least 25 feet high, and as a saturation of nearly double the amount of gas is as improbable, we are compelled to assume that Sigwart's experiments are based upon error. The falsity of many other similar statements may thus be easily shown.

The relations which are found to exist between the free and absorbed gases of a spring by means of the law of absorption, give a fixed starting-point from which to estimate the influence which an amount of nitrogen in the free gas in a spring exerts upon the quantity of carbonic acid dissolved in the water. The second and third columns of the following table, calculated from the preceding formulæ, show the per-centage amount of carbonic acid and nitrogen in the absorbed gas for the corresponding percentages of nitrogen in the free gas given in the first column. The temperature of the water is supposed to be  $15^{\circ}1$  C.

Amount of nitrogen in the free gas.	Gas absorbed in the spring water.	
	Nitrogen.	Carbonic acid.
I.	II.	III.
10 per cent.	1·613	98·387
20 "	3·558	96·442
30 "	5·949	94·051
40 "	8·958	91·042
50 "	12·861	87·139
60 "	18·127	81·873
70 "	25·623	74·377
80 "	37·123	62·877
90 "	57·052	42·948

\* The small amount of solid constituents contained in the water (not more than 9 grains in a pound) cannot appreciably alter the absorption-coefficients, certainly not increase them.

From this table it is plainly seen, that if the gas passing through a spring at 15°·1 contains only 10 per cent. of carbonic acid with 90 per cent. of nitrogen, the gas dissolved in the spring water will contain 42·948 per cent. of carbonic acid. In this way it is easy, in analyses of mineral waters, to calculate the composition of the gases contained in the water, if the composition of the gas which is set free in the spring is known by experiment. If the composition of both gases is directly determined, and the experimental composition agrees with that found by calculation, we have a most valuable confirmation of the correctness of both analyses.

All these deductions from the law of absorption are of course only applicable to cases in which a statical equilibrium between the free and dissolved gases can ensue. This is not only the case in springs through which gases pass, but particularly in rain and dew, and for these is the law applicable with its greatest precision.

The amount of carbonic acid contained in the air varies, according to Saussure's numerous experiments, in 10,000 parts of air between 5·74 and 3·15, giving a mean of 4·15. According to this mean value the air is composed of—

Oxygen . . . . .	20·9512
Nitrogen . . . . .	79·0073
Carbonic acid . . . . .	0·0415
	100·0000

From this analysis, by means of equations (7) and (8), we obtain the following table, representing the mean constitution of the gases contained in rain water for the temperatures given in the upper line.

	0° C.	5° C.	10° C.	15° C.	20° C.
Nitrogen .....	63·20	63·35	63·49	63·62	63·69
Oxygen .....	33·88	33·97	34·05	34·12	34·17
Carbonic acid .....	2·92	2·68	2·46	2·26	2·14
	100·00	100·00	100·00	100·00	100·00

It is directly seen from this table that the amount of carbonic acid which the plants receive from the rain decreases for increasing temperatures.

The mean yearly amount of rain which falls on the surface of the earth is, according to Berghaus, equal to a layer of water of only 1<sup>m</sup>·5 in height. By the help of formula (3) it is found, that with this stratum of water a volume of carbonic acid is conveyed to the earth which has a height of only 1·293 millimetre at 0° and 0·76 pressure; this gives for every square

metre, or about 9 square feet of land, only 1.293 litre or 2.569 grms. of carbonic acid. For every temperature above 0° this amount of course decreases. The carbonic acid carried down with rain from the atmosphere is therefore so inconsiderable, that it appears to be quite inadequate for the nourishment of plants. Still it must not be forgotten that this amount of carbonic acid originally present in the rain-water, in proportion as it is absorbed by the plants, is again taken up from the passing currents of air by the moisture of the leaves and ground. Dew, rain, and even the juices of plants serve then, as means by which the nourishing substances of the atmosphere are transferred to the bodies of the plants. The supply of substances capable of assimilation is therefore regulated by the law of absorption; it varies with the temperature and barometric pressure. The volume of carbonic acid which is contained in the unit-volume of the sap of the plant and moistening surface of water at the mean barometric pressure, as obtained from formula (3), varies with the temperature in the following ratio:—

° C.	Volume.
0 . . . . .	0.000744
5 . . . . .	0.000600
10 . . . . .	0.000490
15 . . . . .	0.000415
20 . . . . .	0.000373

The plant therefore finds in a volume of the water with which it is moistened at 15°, the same amount of carbonic acid which a like volume of air contains; above this temperature, however, there is less, and below more.

It is scarcely possible to overlook in these phenomena an endeavour of nature to assist the tardy vegetation of the cold north by a richer nourishment, and by a sparing supply to keep the luxurious growth of the tropics within limits.

Similarly to carbonic acid, oxygen plays an important part in these phenomena, for under the influence of the decomposing organic constituents of the ground it is converted into carbonic acid. The mixture containing the greatest amount of carbonic acid which can thus be formed is—

Nitrogen . . . . .	79.007
Carbonic acid . . . . .	20.993
	100.000

If we suppose this mixture of gases to remain constant by continual additions from without, according to formula (3), the unit-volume of the moisture contained in the ground surrounding the roots will take up of carbonic acid,—

Temperature.	Volume.
° C.	
at 0 . . . . .	0·00862
5 . . . . .	0·00760
10 . . . . .	0·00681
15 . . . . .	0·00626
20 . . . . .	0·00595

Hence it follows that the amount of carbonic acid which a cubic millimetre of the sap\* from the leaves exposed to the air is able to absorb at 15° and 0<sup>m</sup>·76 pressure, is never more than 0·000415 cubic millimetre, whilst the moisture in the loose earth around the roots may contain 0·00626 cubic millimetre, or nearly fifteen times as much. The great productiveness of mould rich in humus, and a part of the advantages which the agriculturist derives from breaking up his land, is easily explained from these phenomena.

The phenomena of the oceanic atmosphere, as well as of the absorption of air in the blood, which are deducible from the law of absorption, I reserve for my own future investigation.

It must also remain for future research to estimate again with all the exactitude necessary for normal determinations, the absorption-coefficients detailed in the present memoir, an absorptiometer of larger dimensions being made use of. When these experiments are carried out, it will be possible to determine the limits at which the law of absorption, similarly to that of Mariotte, begins to be disturbed by foreign influences.

In conclusion, my best thanks are due to Mr. Roscoe, who has kindly translated this memoir into English.

Heidelberg, February, 1855.

XXVIII. *The Theory of the Moon's Motion.—Explanation of the occurrence of non-periodic Factors in the Development of the Radius-vector.* By Professor CHALLIS †.

**I**N the Postscript to my communication to the Philosophical Magazine for February (p. 137), I have stated that the occurrence of terms containing factors which may increase indefinitely with the time in the development of the moon's radius-vector, is not owing to the circumstance that the approximation commences from a fixed ellipse, as has been generally admitted, but is wholly due to the mode of integration. I now propose to explain why one mode of integration rather than another leads

\* Here the average amount of carbonic acid in the air is taken, and the absorption-coefficients of the sap considered the same as those of water.

† Communicated by the Author.

to such terms, and to indicate processes of integration which are appropriate to the lunar problem.

It will suffice for this purpose to suppose the force to be wholly central, and to be equal to  $\mu u^2 - \frac{\mu'}{u}$ ,  $u$  being the reciprocal of the radius-vector. Thus we have the known equation,

$$\frac{d^2u}{d\theta^2} + u - \frac{\mu}{h^2} + \frac{\mu'}{h^2u^3} = 0. \quad (1)$$

Multiplying by  $2 \frac{du}{d\theta}$  and integrating,

$$\frac{du^2}{d\theta^2} + u^2 - \frac{2\mu u}{h^2} - \frac{\mu'}{h^2u^2} + C = 0.$$

Hence, making  $\frac{du}{d\theta} = 0$ , the equation for determining the apsidal distances is

$$u^4 - \frac{2\mu u^3}{h^2} + C u^2 - \frac{\mu'}{h^2} = 0. \quad (2)$$

Now, since  $C$  is positive for an elliptic orbit, or one approaching to an ellipse, if this equation contains two positive roots, it must, by the theory of equations, contain a third positive root, because the last term is negative. Hence there must be a *third* apsidal distance in addition to the two belonging to the approximate ellipse. The third apse is accounted for by considering that if the radius-vector be very large, and consequently  $u$  very small, the repulsive part of the force, viz.  $-\frac{\mu'}{u}$ , may exceed the attractive part  $\mu u^2$ , even when  $\mu'$  is supposed to be small. Hence the analysis embraces a separate curve, containing infinite branches and one apsidal distance, in addition to the eccentric orbit. If, therefore, it be required to integrate the equation (1) on the supposition that the orbit is of small eccentricity and approximately elliptical, some method must be adopted which will exclude the third apsidal distance. One method proper for this purpose is that indicated by Mr. Airy (Mathematical Tracts, 3rd edition, Art. 44\* of the Lunar Theory, p. 32), which consists in substituting  $b+w$  for  $u$ , and neglecting powers of  $w$  above the first. This is virtually the same as the method employed in Pratt's Mechanical Philosophy (Art. 334, p. 300), where  $b + \overline{u-b}$  is substituted for  $u$ , and powers of  $u-b$  above the first are omitted. Making the latter substitution, the equation (1) becomes,

$$\frac{d^2u}{d\theta^2} + u \left( 1 - \frac{3\mu'}{h^2b^4} \right) - \frac{\mu}{h^2} + \frac{4\mu'}{h^2b^3} = 0,$$



the integral of which is,

$$u = \frac{\mu}{h^2} - \frac{\mu' h^4}{\mu^3} + A \cos \left( \sqrt{1 - \frac{3\mu' h^6}{\mu^4}} \theta + B \right),$$

neglecting powers of  $\mu'$  above the first. It is plain that by supposing the arbitrary constant  $A$  to be small, we satisfy the condition that  $w$  shall be small for all values of  $\theta$ , and consequently that the orbit shall be nearly circular. In this mode of integrating we have excluded the third apsidal distance, by giving to  $u$  the general form  $b + w$ , which admits of satisfying, by one of the arbitrary constants, the case of approximate circularity.

But when a particular form is given to  $u$ , the analysis will include the case of a third apsidal distance, so far as the form admits of satisfying the condition that  $\frac{du}{d\theta}$  be a small quantity

for values of the radius-vector contiguous to that apsidal distance. Thus let  $u = b + be \cos(\theta + \gamma)$  nearly, the second term being supposed to be small compared with the other. Substituting this value of  $u$  in the last term of equation (1), and retaining only the first power of  $e \cos(\theta + \gamma)$ , we have

$$\frac{d^2u}{d\theta^2} + u - \frac{\mu}{h^2} + \frac{\mu'}{h^2 b^3} (1 - 3e \cos(\theta + \gamma)) = 0.$$

This equation gives by integration,

$$u = A \cos(\theta + B) + \frac{\mu}{h^2} - \frac{\mu'}{h^2 b^3} + \frac{3\mu' e}{2h^2 b^3} \theta \sin(\theta + \gamma).$$

Hence by comparison with the assumed value of  $u$ ,

$$b = \frac{\mu}{h^2} - \frac{\mu'}{h^2 b^3} = \frac{\mu}{h^2} - \frac{\mu' h^4}{\mu^3} \text{ nearly,}$$

$$be = A, \quad \text{and } \gamma = B.$$

Consequently the integral of equation (1) in this case is

$$u = \frac{\mu}{h^2} - \frac{\mu' h^4}{\mu^3} + A \cos(\theta + B) + \frac{3\mu' h^4}{2\mu^4} A \theta \sin(\theta + B).$$

Now since the application of this integral is limited by the condition that  $u$  must differ little from a constant value, it follows that the angle  $\theta$  must be taken within certain limits, otherwise that condition is violated. Putting for the sake of brevity  $q$  for  $\frac{3\mu' h^4}{2\mu^4}$ , it will be found that  $\frac{du}{d\theta} = 0$  gives

$$\tan(\theta + B) = \frac{q\theta}{1 - q}$$

$$\text{and } \frac{d^2u}{d\theta^2} = A \cos(\theta + B) \cdot \left\{ -1 + 2q - \frac{q^2 \theta^2}{1 - q} \right\}.$$

These results show, since  $\theta$  may be supposed positive, that  $\theta + B$  will be less than  $90^\circ$ , and that, as  $A$  is positive,  $u$  has a maximum value if  $2q$  be less than unity, but cannot in that case have a minimum value. Hence as  $q$  is by supposition a small quantity, it is clear that the maximum does not apply to the elliptical orbit, but to the third apse; for if it applied to the elliptical orbit,  $u$  would also have a minimum value.

If, however, the above approximation, viz.  $u = b + be \cos(\theta + \gamma)$ , be derived by integration from the approximate equation

$$\frac{d^2u}{d\theta^2} + u - \frac{\mu}{h^2} = 0,$$

the second approximation can have no reference to the third apsidal distance, and ought to be true for all values of  $\theta$ . But this cannot be the case, unless the above integral be considered to be derived by a diverging expansion from the equation,

$$u = \frac{\mu}{h^2} - \frac{\mu' h^4}{\mu^3} + A \cos \left( \sqrt{1 - \frac{3\mu' h^4}{\mu^4}} \theta + B \right).$$

The foregoing reasoning shows that when the integral of equation (1) contains a term which involves  $\theta$  as a factor, this circumstance is due to the particular process of integration, by which the values of the radius-vector for points contiguous to the third apse are taken into account, and that although the same integral approximates to an orbit of small eccentricity, it is attended with the inconvenience of presenting the approximation under a divergent form. I have already pointed out one method of obviating this inconvenience, and excluding the third apse from the investigation. The same purpose is answered by approximating to the two nearly equal roots of the apsidal equation (2), which is the principle of the process I have adopted for finding in a direct manner the mean motion of the apses of the moon's orbit. A third method will be seen in my article in the February Number (p. 132), in which the approximation commences from a fixed ellipse. The reason of the success of this method is, that by so commencing the approximation, the elliptic apsidal distances are taken account of, to the exclusion of the third apsidal distance.

All the above considerations, which for simplicity have been applied to the case of a central force, are equally applicable to the moon's orbit. I think I have now clearly pointed out the origin, in the Lunar Theory, of terms which admit of indefinite increase, and how they may be avoided. It appears that they have no relation whatever to the circumstance of commencing the approximation from a fixed ellipse. I confess that when I entered upon these researches, I little expected such a result.

In the February Number (p. 133), I have adduced an argument to prove, on the assumption of the illegitimacy of starting from a fixed ellipse, that the apsidal equation contains the disturbing force as a factor. It is now shown that the illegitimacy of that procedure cannot be assumed on the ground of its giving rise to non-periodic factors in the development of the radius-vector. I shall take a future occasion for showing that the argument, together with the new theorem respecting the eccentricity of the moon's orbit, which depends upon it, may still be maintained.

Cambridge Observatory,  
Feb. 16, 1855.

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XXIX. *On the Existence of a Magnetic Medium in Space.*

By JOHN TYNDALL, Ph.D., F.R.S.

MY DEAR MR. FARADAY,

FEW, I imagine, who read your memoir in the last Number of the Philosophical Magazine, will escape the necessity of reconsidering their views of magnetic action. We are so accustomed to regard the phænomena of this portion of science through the imagery with which hypothesis has invested them, that it is extremely difficult to detach symbols from facts, and to view the latter in their purity. This duty, however, is now forced upon us; for the more we reflect upon the results of recent scientific research, the more deeply must we be convinced of the impossibility of reconciling these results with our present theories\*. In the downfall of hypotheses thus pending, the great question of a universal magnetic medium has presented itself to your mind. Your researches incline you to believe in the existence of such a medium, and lead you, at the same time, to infer the perfect identity of magnetism and diamagnetism.

In support and illustration of your views, you appeal to the following beautiful experiments:—Three solutions of proto-sulphate of iron are taken; the first, *l*, contains 4 grains; the second, *m*, 8 grains; and the third, *n*, 16 grains of the salt to a cubic inch of water. Enclosed in hollow globules of glass, all these solutions, when suspended in the air before the pole of a magnet, are attracted by the pole. You then place a quantity of the medium solution, *m*, in a proper vessel, immerse in it the globule containing the strong solution *n*, and find that the latter is still attracted; but that when the globule containing the solution *l* is immersed, the latter is *repelled* by the mag-

\* Some of the reasons which induce the writer to hold this opinion are given in the Bakerian Lecture of the Royal Society for the present year.

netic pole. Substituting elongated tubes for spheres, you find that when a tube containing a solution of a certain strength is suspended in a weaker solution, between the two poles of a magnet, the tube sets from pole to pole; but that when the solution *without* the tube is stronger than that *within* it, the tube recedes from the pole and sets equatorially.

Here then, you state, are the phenomena of diamagnetism. It is maintained by some, that, to account for these phenomena, it is necessary to assume, in the case of diamagnetic bodies, the existence of a polarity the reverse of that of iron. But nobody will affirm that the mere fact of its being suspended in a stronger solution reverses the polarity of a magnetic liquid:—to account for the repulsion of the weak solution, when submerged in a stronger one, no such hypothesis is needed; why then should it be thought necessary in the case of so-called diamagnetic bodies? It is only by denying that space presents a medium which bears the same relation to diamagnetic bodies that the stronger magnetic solution bears to the weaker one, that the hypothesis of a distinct diamagnetic polarity is at all rendered necessary.

The effects upon which the foregoing striking argument is based are differential ones, and are embraced, as already observed by M. E. Becquerel, by the so-called principle of Archimedes. This principle, in reference to the case before us, affirms that the body immersed in the liquid is attracted by a force equal to the difference of the attractions exerted upon the liquid and the body immersed in it. Hence, if the attraction of the liquid be less than that of the immersed body, the latter will approach the pole; if the former attraction be the greater, the immersed body recedes from the pole, and is apparently repelled. The action is the same as that of gravity upon a body immersed in water: if the body be more forcibly attracted, bulk for bulk, than the water, it sinks; if less forcibly attracted, it rises; the mechanical effect being the same as if it were repelled by the earth.

The question then is, are all magnetic phenomena the result of a differential action of this kind? Does space present a medium less strongly attracted than soft iron, and more strongly attracted than bismuth, thus permitting of the approach of the former, but causing the latter to recede from the pole of a magnet? If such a medium exists, then diamagnetism, as you incline to believe, merges into ordinary magnetism, and “the polarity of the magnetic force,” in iron and in bismuth, is one and the same.

Pondering upon this subject a few evenings ago, and almost despairing of seeing it ever brought to an experimental test, a thought occurred to me, which, when it first presented itself,

seemed to illuminate the matter. Such illuminations vanish in nine cases out of ten before the test of subsequent criticism; but the thought referred to, having thus far withstood the criticism brought to bear upon it, I am emboldened to submit it to you for consideration.

I shall best explain myself by assuming that a medium of the nature described exists in space, and pursuing this assumption to its necessary consequences.

Let a cube, formed from the impalpable dust of carbonate of iron\*, which has been compressed forcibly in one direction, be placed upon the end of a torsion beam, and first let the line in which the pressure has been exerted be in the direction of the beam. Let a magnet, with its axis at right angles to the beam, and hence also at right angles to the line of pressure, be brought to bear upon the cube. The cube will be attracted, and the amount of this attraction, at any assigned distance, may be accurately measured by the torsion of the wire from which the beam depends. Let this attraction, expressed in degrees of torsion, be called  $a$ . Let the cube now be turned round  $90^\circ$ , so that the line of pressure shall coincide with the direction of the axis of the magnet, and let the attraction  $a'$  in this new position be determined as in the former instance. On comparison it will be found that  $a'$  exceeds  $a$ ; or, in other words, that the attraction of the cube is strongest when the force acts parallel to the line of compression†.

Instead of carbonate of iron we might choose other substances of a much feebler magnetic capacity, with precisely the same result. Let us now conceive the magnetic capacity of the compressed cube to diminish gradually, and thus to approach the capacity of the medium in which, according to our assumption, the carbonate of iron is supposed to be immersed. If it were a perfectly homogeneous cube, and attracted with the same force in all directions, we should at length arrive at a point, when the *magnetic weight* of the cube, if I may use the term, would be equal to that of the medium, and we should then have a substance which, as regards magnetism, would be in a condition similar to that of a body withdrawn from the action of gravity in Plateau's experiments. Such a body would be neither attracted nor repelled by the magnet. In the compressed cube, however, the magnetic weight varies with the direction of the force; supposing the magnetic weight, when the force acts along the line of compression, to be equal to that of the medium, then if the force acted across the line of compression, the mag-

\* For an ample supply of this most useful mineral, I am indebted to the kindness of J. Kenyon Blackwell, Esq., F.G.S.

† Phil. Mag. Sept. 1851; Pogg. Ann. 1851.

netic weight of the cube would be less than that of the medium. Acted upon in the former direction, the cube would be a neutral body; acted upon in the latter direction, it would be a diamagnetic body. If the magnetic capacity of the cube diminish still further, it will, according to your hypothesis, become wholly diamagnetic. Now it is evident, supposing the true magnetic excitement to continue, that the cube, when acted on by the magnet in the direction of compression, *will approach nearer to the magnetic weight of the medium* in which we suppose it immersed, than when the action is across the said line; and, hence, the repulsion of the cube, when the force acts along the line of compression, must be *less* than when the force acts across it.

Reasoning thus from the assumption of a magnetic medium in space, we arrive at a conclusion which can be brought to the test of experiment. So far as I can see at present, the assumption is negatived by this test; for in diamagnetic bodies the repulsion along the line in which the pressure is exerted is proved by experiment to be a *maximum*\*. An ordinary magnetic excitement could not, it appears to me, be accompanied by this effect.

The subject finds further, and perhaps clearer, elucidation in the case of isomorphous crystals. It is not, I think, questioned at present, that the deportment of crystals in the magnetic field depends upon their molecular structure; nor will it, I imagine, be doubted, that the molecular structure of a complete crystal of carbonate of iron is the same as that of an isomorphous crystal of carbonate of lime. In the architecture of the latter crystal, calcium simply takes the place which iron occupies in the former. Now a crystal of carbonate of iron is attracted most forcibly when the attracting force acts parallel to the crystallographic axis †. Let such a crystal be supposed to diminish gradually in magnetic capacity, until finally it attains a magnetic weight, *in a direction parallel to its axis*, equal to that of the medium in which we assume it to be immersed. Such a crystal would be indifferent, if the force acted parallel to its axis, but would be repelled, if the force acted in any other direction. If the magnetic weight of the crystal diminish a little further, it will be repelled in all directions, or, in other words, will become diamagnetic; but it will then follow, that the repulsion in the direction of the axis, if the nature of the excitement remain unchanged, will be less than in any other direction. In other words, a diamagnetic crystal of the form of carbonate of iron will, supposing magnetism and diamagnetism to be the same,

\* Phil. Mag. Sept. 1851. Pogg. Ann. 1851.

† Phil. Mag. Sept. 1851. Pogg. Ann. 1851.

be repelled with a *minimum* force when the repulsion acts parallel to the axis. Here, as before, we arrive at a conclusion which is controverted by experiment; for the repulsion of a crystal of carbonate of lime is a *maximum* when the repelling force acts along the axis of the crystal. Hence I would infer that the excitement of carbonate of iron cannot be the same as that of carbonate of lime.

Such are the reflections which presented themselves to my mind on the evening to which I have referred. I now submit them to you as a fraction of that thought which your last memoir upon this great question will assuredly awaken.

Believe me,

Dear Mr. Faraday,

Yours very faithfully,

JOHN TYNDALL.

Royal Institution,  
February 1855.

XXX. *Theorems on the Quadrature of Surfaces and the Rectification of Curves.* By the Rev. ROBERT CARMICHAEL, A.M., Fellow of Trinity College, Dublin, and Examiner in Mathematics for the year 1854 in the Queen's University in Ireland\*.

1. IT is well known that there are many plane curves whose equations are more easily expressed in polar than in rectangular coordinates, and for whose rectification we employ the formula

$$S = \int_{\theta_1}^{\theta_2} \sqrt{r^2 + (D_\theta r)^2} d\theta.$$

Of this class are, the spiral of Archimedes,

$$r = a\theta;$$

the lituus,

$$r^2 = \left(\frac{a}{\theta}\right)^2;$$

the lemniscate,

$$r^2 = a^2 \cos 2\theta;$$

the logarithmic spiral,

$$r = ce^{\frac{\theta}{a}};$$

and the cardioid;

$$r = a(1 - \cos \theta).$$

2. I am not aware that any mathematician has attempted to trace the surfaces analogous to these; but for the *quadrature* of such surfaces, when discovered, it is absolutely necessary that we should have a general expression in *polar coordinates* for the element of any surface. Such an expression is not found in the

\* Communicated by the Author.

ordinary works upon the differential and integral calculus. In the elaborate treatise upon this subject by M. L'Abbé Moigno (Paris, 1844, vol. ii. p. 235), the expression is investigated, by the usual analytical method, transformation of coordinates, from the well-known expression in rectangular coordinates,

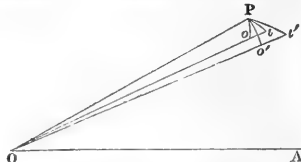
$$d\sigma = \sqrt{(1+p^2+q^2)} dx dy,$$

and is given in the following shape,

$$d\sigma = \sqrt{\{r^2 \sin^2 \theta + \sin^2 \theta (D_\theta r)^2 + (D_\phi r)^2\}} r d\theta d\phi.$$

A short geometrical deduction of this expression, whose merit I have great pleasure in sharing with my friend, Alexander Jack, Esq., A.B., may not be unacceptable to the student.

Let P be any point on the surface. Through the axis OA and OP describe a plane, and round the axis describe, with the same line, a cone. The surface may then be supposed to be divided into its elements by planes and cones consecutive to these respectively (the planes all passing through the axis and the cones round it), half of one such element being represented by  $\iota P \iota'$ . Then, remembering that the planes cut the cones orthogonally, we have



$$d\sigma = P\iota . P\iota' . \sin \iota P \iota' = P\iota . P\iota' . \sqrt{(1 - \cos^2 \iota P \iota')},$$

whence

$$d\sigma = P\iota . P\iota' . \sqrt{(1 - \sin^2 \iota P o . \sin^2 \iota' P o')} = \sqrt{(P\iota^2 . P\iota'^2 - o\iota^2 . o'\iota'^2)},$$

$o$  and  $o'$  being the points where the sphere described round the origin with radius  $OP$  intersects the consecutive radii vectors to the points  $\iota, \iota'$ ; or

$$d\sigma = \sqrt{\{r^2 \sin^2 \theta d\phi^2 + (D_\phi r)^2 d\phi^2\} \{r^2 d\theta^2 + (D_\theta r)^2 d\theta^2\} - (D_\theta r)^2 d\theta^2 (D_\phi r)^2 d\phi^2},$$

or finally,

$$d\sigma = \sqrt{\{r^2 \sin^2 \theta + \sin^2 \theta (D_\theta r)^2 + (D_\phi r)^2\}} r d\theta d\phi.$$

3. From this expression we may readily derive that for the perpendicular from the origin upon the tangent plane, in *polar* coordinates. In *rectangular* coordinates it is known to be

$$P = \frac{z - px - qy}{\sqrt{(1+p^2+q^2)}};$$

but the transformation of this to polar coordinates would be troublesome and tedious. We may easily derive the required expression from the volume of the elementary cone, for

$$P d\sigma = r^3 \sin \theta d\theta d\phi,$$



and therefore

$$P = \frac{r^2 \sin \theta}{\sqrt{\{r^2 \sin^2 \theta + \sin^2 \theta (D_\theta r)^2 + (D_\phi r)^2\}}}$$

4. As an example of the application of the formula for the quadrature of surfaces, let us suppose that it is required to investigate the quadrature, between given limits, of the surface

$$r = me^{-\phi} \cos \theta.$$

Then

$$D_\theta r = -me^{-\phi} \sin \theta, \quad D_\phi r = -me^{-\phi} \cos \theta;$$

therefore

$$d\sigma = \sqrt{(m^2 e^{-2\phi} \cos^2 \theta \sin^2 \theta + m^2 e^{-2\phi} \sin^4 \theta + m^2 e^{-2\phi} \cos^2 \theta)} r d\theta d\phi,$$

or

$$d\sigma = m^2 e^{-2\phi} \cos \theta d\theta d\phi;$$

whence

$$\Sigma = m^2 \int e^{-2\phi} (\sin \theta_2 - \sin \theta_1) d\phi.$$

Let us suppose the limits to be given by the intersections, with the given surface, of the cones

$$\theta_2 = a\phi, \quad \theta_1 = b\phi,$$

and

$$\Sigma = m^2 \int_{\phi_1}^{\phi_2} e^{-2\phi} (\sin a\phi - \sin b\phi) d\phi,$$

an integral which is susceptible of easy reduction, since we know that

$$\int e^{-m\phi} \sin a\phi d\phi = -e^{-m\phi} \frac{m \sin a\phi + a \cos a\phi}{m^2 + a^2}.$$

5. As a second example, let it be proposed to investigate the quadrature, within given limits, of the surface

$$r = m \cos \phi \sin \theta.$$

Here

$$D_\theta r = m \cos \phi \cos \theta, \quad D_\phi r = -m \sin \phi \sin \theta,$$

and

$$d\sigma = m^2 \cos \phi \sin^2 \theta d\theta d\phi;$$

whence

$$\Sigma = m^2 \int_{\theta_1}^{\theta_2} (\sin \phi_2 - \sin \phi_1) \sin^2 \theta d\theta;$$

and, if the limits be given as before, there is no difficulty in determining the quadrature completely.

6. In the masterly treatise upon the Calculus of Variations by the Rev. Professor Jellett (Dublin, 1850, p. 262), it is shown that the surface which, within given limits, renders the double integral

$$\iint \sqrt{(p^2 + q^2)} dx dy,$$

212 The Rev. R. Carmichael on the Quadrature of Surfaces  
 or,  $\gamma$  being the angle made by the radius vector with the axis of  $z$ ,

$$\iint \sin \gamma \cdot d\sigma,$$

a minimum, is given by the partial differential equation

$$q^2r - 2pqs + p^2t = 0,$$

whose integral is known to be

$$xF_1(z) + yF_2(z) = 1,$$

representing the gauche surface generated by a right line which, gliding upon two fixed directrices, remains constantly parallel to the plane of the axes of  $x$  and  $y$ ; as indeed might be anticipated from a consideration of the question in its second form.

In the same manner it might be shown that the surface which, within given limits, renders the double integral

$$\iint \sqrt{\{(D_\theta r)^2 + (D_\phi r)^2\}} d\theta d\phi$$

a minimum, is given by the equation

$$\phi F_1(r) + \theta F_2(r) = 1.$$

If it be proposed to investigate the property of this surface corresponding to the character of the generation of the analogous surface in rectangular coordinates, as the latter character is exhibited by the supposition  $z = \text{const.}$ , so the former property may be investigated by the supposition  $r = \text{const.}$  Let, then, the surface be supposed to intersect a sphere described round the origin, and let the nature of the curve of intersection be examined. If we resolve any element into its rectangular components, one such component is  $r d\theta$ , and the other  $r \sin \theta d\phi$ . Let  $i$  be the inclination of the element to the meridional plane described through its extremity and the fixed axis, and it is evident that

$$\tan i = \frac{r \sin \theta d\phi}{r d\theta} = - \frac{F_2(c)}{F_1(c)} \sin \theta,$$

$c$  being the radius of the sphere; or the tangent of the angle of inclination of the curve to the meridional plane is proportional to the sine of the angle made by the radius vector with the axis.

7. It may be well here to indicate certain desiderata, the knowledge of which might lead to the discovery of some interesting properties of surfaces.

The measure of curvature at any point of a surface is expressed in rectangular coordinates by the formula

$$\frac{1}{R_1 R_2} = \frac{rt - s^2}{(1 + p^2 + q^2)^{\frac{3}{2}}};$$

we have no corresponding expression in polar coordinates. Such

might be discovered by the investigation of the analogue of the known formula for plane curves

$$\rho = r \frac{dr}{dp}.$$

Again, the *sum of the curvatures* at any point of a surface is expressed by the formula, in rectangular coordinates,

$$\frac{1}{R_1} + \frac{1}{R_2} = - \frac{(1+q^2)r - 2pqs + (1+p^2)t}{(1+p^2+q^2)^{\frac{3}{2}}};$$

we have no corresponding expression in polar coordinates. Other desiderata will readily suggest themselves.

8. With regard to the rectification of curves, it may be useful to make a few observations upon a subject which has recently attracted much attention among French mathematicians. In the Notes by M. Liouville to his valuable edition of the *Application de l'Analyse à la Geometrie* of the illustrious Monge, will be found (p. 558) the following remarks:—

“M. Serret a fait usage de certaines variables qu'il avait déjà employées au tome xiii. du *Journal de Mathématiques*, pour résoudre le problème suivant :  $x, y, z, s$  étant quatre fonctions d'une variable indépendante  $\theta$  assujetties à vérifier l'équation

$$dx^2 + dy^2 + dz^2 = ds^2,$$

exprimez sans forme finie et sans aucun signe d'intégration, les valeurs générales de ces fonctions. La solution de ce problème conduit, par exemple, à trouver des courbes à double courbure qui soient à la fois algébriques et rectifiables algébriquement, ou dont l'arc dépende d'une transcendante donnée. Le problème analogue pour les courbes planes dépend de l'équation plus simple

$$dx^2 + dy^2 = ds^2,$$

et se résout, comme on sait, par les formules

$$x = \psi'(\theta) \sin \theta + \psi''(\theta) \cos \theta,$$

$$y = \psi'(\theta) \cos \theta - \psi''(\theta) \sin \theta,$$

$$s = \psi'(\theta) + \psi''(\theta),$$

où la fonction  $\psi$  est arbitraire. Les formules de M. Serret pour l'équation

$$dx^2 + dy^2 + dz^2 = ds^2,$$

sont beaucoup plus compliquées, et, pourtant, beaucoup moins utiles.”

It appears to me that the integration of these equations may be effected directly, and with great simplicity, by employing the Calculus of Quaternions.

Thus, in the notation of this Calculus, the first equation

$$dx^2 + dy^2 = ds^2$$

is equivalent to

$$-(idx + jdy)^2 = -(d\rho)^2,$$

or

$$idx + jdy = d\rho;$$

whence

$$ix + jy = \rho + \alpha,$$

$\alpha$  being an arbitrary vector; or, between given limits,

$$i(x_2 - x_1) + j(y_2 - y_1) = \rho_2 - \rho_1,$$

an identity, as it ought to be.

Similarly, the second equation

$$dx^2 + dy^2 + dz^2 = ds^2$$

is equivalent to

$$-(idx + jdy + kdz)^2 = -(d\rho)^2,$$

or

$$idx + jdy + kdz = d\rho;$$

whence

$$ix + jy + kz = \rho + \alpha,$$

$\alpha$  being an arbitrary vector, or, between given limits,

$$i(x_2 - x_1) + j(y_2 - y_1) + k(z_2 - z_1) = \rho_2 - \rho_1,$$

an identity, as it ought to be.

5 Trinity College, Dublin,  
November 1854.

XXXI. *On the Products of the Destructive Distillation of Animal Substances.*—Part III. By THOMAS ANDERSON, M.D., *Regius Professor of Chemistry in the University of Glasgow.*

[Concluded from p. 150.]

*Constitution of the Bases of the Picoline Series.*

HAVING in this and the previous part of these researches accumulated sufficient evidence of the existence of a class of bases isomeric with that of which aniline is the type, it became important to determine to which of the three classes of volatile bases they belong. For this purpose, pyridine, picoline, and collidine were submitted to the action of iodide of æthyle. The experiments were carried out in considerable detail with picoline, but with the other two salts no more was done than sufficed to substantiate the fact, that iodide of æthyle acted on them in a similar manner.

*Action of Iodide of Æthyle on Picoline.*

Anhydrous picoline and iodide of æthyle were mixed, in the proportion of one volume of the former to two of the latter, and

sealed hermetically in a combustion tube. The two fluids mix readily, but if the tube containing them be gently heated, by plunging it for the space of half a minute into the water-bath, an action takes place attended with the evolution of much heat, the fluid becomes muddy, and separates into a thick oily stratum which rises to the surface, and a more fluid one which descends. On cooling, the former solidifies into a highly crystalline mass, and well-formed crystals appear in the latter, which consists of the excess of iodide of æthyle. Even without the application of heat the action takes place, though more slowly, and it is necessary to have them in contact for some days before the action is finished. When heat is applied, the action is complete in ten minutes, but in every instance the tubes were allowed to stand for twenty-four hours, so as to present the complete crystallization of the new compound. The tubes were then opened, the whole contents thrown upon a filter, and the crystals slightly washed with a mixture of alcohol and æther, pressed between folds of filtering paper, and dissolved in the smallest possible quantity of a mixture of boiling alcohol and æther. On cooling, the new substance is deposited in beautiful silvery plates. It is highly soluble in water, and though not deliquescent, it becomes slightly damp in moist air. Its aqueous solution, on evaporation, solidifies into a mass of crystals. It is readily soluble in alcohol, especially when boiling, and the hot solution on cooling becomes filled with crystals. It is less soluble in æther. It fuses below  $212^{\circ}$  into an oily fluid. Its analysis gave these results:—

5.350 grs., dried *in vacuo*, gave 7.570 grs. carbonic acid and 2.375 grs. water.

6.440 grs., dried *in vacuo*, gave by direct precipitation 6.065 grs. iodide of silver.

	Experiment.	Calculation.		
Carbon . . .	38.57	38.70	C <sup>16</sup>	96
Hydrogen . .	4.93	4.83	H <sup>12</sup>	12
Nitrogen . .	5.61	5.67	N	14
Iodine . . .	50.89	50.80	I	126
	<u>100.00</u>	<u>100.00</u>		<u>248</u>

These results agree with the formula C<sup>16</sup> H<sup>11</sup> N HI, which is that of hydriodate of æthylopicoline.

On the addition of a few drops of caustic potash to the solution of this salt no odour of a volatile base is evolved, nor is there any separation of an oily layer, but the addition of a large quantity of strong potash causes the precipitation of a viscid oil, which solidifies on standing for some hours into a mass of crystals, generally much coloured, and which proves to be the hydriodate partially altered by decomposition. When boiled

with strong potash, a volatile base is slowly formed, which is a product of decomposition, and will be afterwards referred to. From these characters it is obvious that æthylopicoline belongs to the ammonium class of bases, and hence picoline itself must be a nitryle base; and this being the case, the formula of the iodine compound might be written thus,  $C^{16}H^{12}N + I$ , representing it as the iodide of a base corresponding to ammonium, of which the constitution must be  $C^{16}H^{12}N$ , and whose oxide,  $C^{16}H^{12}NO$ , must exist in its oxygen acid salts. If we adopt the nomenclature proposed by Hofmann for the ammonium bases, we should have a very clumsy name for this substance, and I shall therefore continue to call it æthylopicoline, which, though not perfectly correct, is sufficiently distinctive.

Æthylopicoline, or rather its oxide, is readily obtained by agitating the aqueous solution of the iodide with moist oxide of silver, when iodide of silver is precipitated, and the base obtained in solution. In performing this process, heat must be avoided, as oxide of silver decomposes the base at a high temperature, fine violet streaks appearing in the fluid, which rapidly acquires a deep crimson colour. The same change occurs, though more slowly, in the cold, especially if the oxide of silver be added in large excess, and it is therefore desirable that the solution should be separated as rapidly as possible. If this be done, a colourless solution is obtained, having a faint peculiar odour and highly alkaline properties. It restores the blue of reddened litmus, and gives an intense brown with turmeric; it has a powerfully caustic taste, and produces a soapy sensation when rubbed between the fingers. It absorbs carbonic acid from the air, precipitates alumina, and redissolves it when added in excess. From a solution of corrosive sublimate it throws down the oxide, and with the metallic salts generally it reacts in precisely the same manner as potash or soda. On boiling, its solution acquires a deep red colour, and the odour of a volatile base becomes apparent. By evaporation *in vacuo*, a hard gummy mass with a green metallic lustre is left behind, which gives a magnificent blood-red solution with water, and deliquesces when exposed to the air. At first I entertained the hope that this substance, though coloured, might, when submitted to analysis, give the results of æthylopicoline; a very few experiments, however, sufficed to show that it had undergone decomposition, and no attempt was made to analyse it, but attention was directed to obtaining such double salts as might serve to confirm the constitution of the base.

*Platinochloride of Æthylopicoline.*—In order to obtain this salt, nitrate of silver was added to the iodide as long as a precipitate was formed, which was separated by filtration, and the excess of silver thrown down by hydrochloric acid. The filtrate was then mixed with a strong solution of bichloride of platinum

and set aside. In the course of a few hours the salt was deposited in orange-red tabular crystals of remarkable beauty, and often of considerable size. It is readily soluble in cold water, and still more so in hot, and is deposited unchanged from its solution. It possesses considerable stability, but by long-continued boiling it undergoes decomposition. Its analysis gave,—

6.015 grs. of platinochloride of æthylopicoline gave 6.430 grs. carbonic acid and 2.040 grs. water.

6.825 grs. of platinochloride of æthylopicoline gave 2.031 grs. platinum.

6.910 grs. of platinochloride of æthylopicoline gave 2.058 grs. platinum.

6.970 grs. of platinochloride of æthylopicoline gave 2.085 grs. platinum.

	Experiment.			Calculation.	
	I.	II.	III.		
Carbon .	29.15	...	...	29.33	C <sup>16</sup> 96
Hydrogen	3.76	...	...	3.66	H <sup>12</sup> 12
Nitrogen	...	...	...	4.31	N 14
Chlorine	...	...	...	32.54	Cl <sup>3</sup> 106.5
Platinum	29.75	29.78	29.91	30.16	Pt 98.7
				100.00	327.2

*Aurochloride of Æthylopicoline.*—This compound is readily formed by adding a solution of chloride of gold to the nitrate, with excess of hydrochloric acid, obtained from the iodide, in the manner employed for the production of the platinum salt. It is slowly deposited in the form of golden-yellow flattened prisms of great beauty. It is sparingly soluble in cold water, readily in hot, and is deposited unchanged on cooling. It is insoluble in alcohol and æther. Ammonia converts it into a cinnamon-brown powder, and it is instantly blackened on the addition of potash to its hot solution. The specimen analysed was dried at 212°, and burnt with chromate of lead.

6.745 grs. of aurochloride of æthylopicoline gave 5.093 grs. carbonic acid and 1.675 gr. water.

5.300 grs. of aurochloride of æthylopicoline gave 2.265 grs. gold.

	Experiment.	Calculation.	
Carbon . . .	20.59	20.83	C <sup>16</sup> 96
Hydrogen . . .	2.75	2.60	H <sup>12</sup> 12
Nitrogen . . .	...	3.06	N 14
Chlorine . . .	...	30.82	Cl <sup>4</sup> 142
Gold . . . .	42.73	42.69	Au 196.6
		100.00	460.6

Corresponding with the formula C<sup>16</sup> H<sup>12</sup> N Cl + Au Cl<sup>3</sup>.

It has been already mentioned that though æthylopicoline is fixed and inodorous, its iodide cannot be distilled with potash, or the base itself boiled or even evaporated *in vacuo*, without undergoing a decomposition, attended with the evolution of volatile base. In the latter case the decomposition is slow; and even after the ebullition has been continued for some hours, the odour is given off with undiminished intensity, till by long-continued boiling it at length becomes extremely faint, although it does not altogether disappear. When the iodide is boiled with potash, the change is more rapid, and after three or four hours' boiling a considerable quantity of base is found in the receiver. The product has a pungent and putrid odour, fumes strongly with hydrochloric acid, and forms with it a salt entirely soluble in absolute alcohol. Two analyses were made of the platinum compound of this base; the one from a portion collected at the commencement, the other towards the end of the distillation, which show that the product was of uniform composition throughout. The results were as follows:—

I. 6.440 grs. of platinochloride gave 2.430 grs. carbonic acid and 1.920 gr. water.

II. 11.775 grs. of platinochloride gave 4.210 grs. carbonic acid and 3.457 grs. water.

I. 4.385 grs. of platinochloride gave 1.705 gr. platinum.

II. 6.580 grs. of platinochloride gave 2.575 grs. platinum.

	Experiment.		Calculation.	
	I.	II.		
Carbon . . . .	10.29	9.75	9.55	C <sup>4</sup> 24
Hydrogen . . . .	3.31	3.26	2.78	H <sup>8</sup> 8
Nitrogen . . . .	...	...	5.99	N 14
Chlorine . . . .	...	...	42.39	Cl <sup>3</sup> 106.5
Platinum . . . .	38.88	39.23	39.29	Pt 98.7
			<u>100.00</u>	<u>251.2</u>

Its formula, therefore, is C<sup>4</sup> H<sup>7</sup> N, H Cl + Pt Cl, and the base itself is æthylamine.

The base obtained by the distillation of the æthylopicoline alone was found to have the same composition, for 6.177 grs. of its platinum salt gave 2.413 grs. of platinum, equal to 39.06 per cent.

The decomposition which thus occurs is very remarkable, and differs entirely from that observed by Hofmann in the ammonium bases examined by him. The oxide of tetraethylammonium, for instance, is not decomposed when evaporated *in vacuo*. Even at 212° it undergoes no change until it becomes nearly dry, but then a base and a permanent gas are evolved, the former being triethylamine, and the latter olefiant gas. In this case, one out



of the four æthyle atoms which the complex base contained is decomposed, and the other three remain with the ammonia in the form of a nitrile base; in fact, we may fairly assume that the atom of æthyle added to the triæthylamine to convert it into tetraæthylammonium is decomposed, and the base which formed the starting-point of that action is regenerated. With meth-æthylopicoline the case is different; we start, indeed, from a nitrile base, but in place of reproducing it in the decomposition, the atom of æthyle which has been added takes possession of the ammonia, and produces an amide base, leaving the radicals, which we must assume to have replaced the three atoms of hydrogen in the ammonia from which the picoline was originally produced, in some other form of combination. In another point, also, the decomposition of æthylopicoline differs from that of tetraæthylammonium. According to Hofmann, the latter base is entirely converted into triæthylamine and olefiant gas; but æthylopicoline, even after long-continued boiling, gives an abundant residue on evaporation. The substance so obtained is amorphous, has an intense blood-red colour, and is a base forming a platinum salt insoluble in water. Although these experiments were made on a very small scale, and the slowness of the action rendered it impossible to say with certainty whether the decomposition was complete, this platinum compound was analysed, and the results were—

5·840 grs. of the platinum salt gave 8·550 grs. carbonic acid and 2·390 grs. water.

7·555 grs. of the platinum salt gave 1·652 gr. platinum.

Carbon . . . .	39·92
Hydrogen . . . .	4·54
Platinum . . . .	21·86

From a single analysis such as this it is impossible to deduce a formula; but it is obvious that a base of much higher atomic weight than æthylopicoline has been produced, the further examination of which must be deferred to a future paper, and which will probably lead to interesting results.

#### *Action of Iodide of Æthyle on Pyridine.*

When pyridine is treated with iodide of æthyle, the action, as might be expected, is in all respects similar to that which occurs with picoline. A homogeneous mixture is first formed, and then, on gently warming, the action takes place, with the evolution of much heat, and the hydriodate of æthylopyridine rises to the surface as an oily layer. The crystallization of this substance, as it cools, is an extremely beautiful phænomenon. Minute rhombs make their appearance here and there in the viscid fluid,

where they increase in size so rapidly that they may actually be seen to grow; and in a successful operation they sometimes increase to the size of from a quarter to three-eighths of an inch in diameter in the course of half an hour. By and by the crystals come into contact with one another, and the fluid is converted into a solid crystalline mass. The crystals are removed from the tube, pressed in folds of filtering paper, and crystallized from a mixture of absolute alcohol and æther. They then form fine silvery plates, highly soluble in water, and slightly deliquescent; in alcohol and æther they are also extremely soluble, though less so than in water. With reagents, their behaviour is so exactly the same as that of the æthylopicoline salts, that it is unnecessary to enter into any details. By analysis the following results were obtained:—

6.110 grs. of hydriodate of æthylopyridine gave 8.105 grs. carbonic acid and 2.525 grs. water.

5.445 grs. of hydriodate of æthylopyridine gave 5.395 grs. iodide of silver.

	Experiment.	Calculation.		
Carbon . . .	36.17	35.89	C <sup>14</sup>	84
Hydrogen . . .	4.59	4.27	H <sup>10</sup>	10
Nitrogen . . .	5.70	6.04	N	14
Iodine . . .	53.54	53.80	I	126
	100.00	100.00		234

The formula of the substance, therefore, is C<sup>14</sup> H<sup>10</sup> N I.

Æthylopyridine itself may be separated from the salt by the action of oxide of silver. It forms a highly alkaline fluid, which undergoes decomposition when heated, with the evolution of a base which is no doubt æthylamine, and agrees completely with it in properties, though the small scale on which the experiment was performed prevented my establishing this fact by analysis. It unites with acids, and forms salts, which are all crystallizable, and generally highly soluble. The platinum and gold salts are extremely beautiful compounds.

*Platinochloride of Æthylopyridine.*—This salt was prepared in the same manner as the corresponding æthylopicoline compound. It is sparingly soluble in cold water, and insoluble in a mixture of alcohol and æther. When slowly formed, it is obtained in beautiful garnet-coloured rhomboidal plates with bevelled edges, which are easily got of a quarter of an inch in diameter, even when operating on very small quantities. Its analysis gave—

7.152 grs. of æthylopyridine platinum salt gave 6.905 grs. carbonic acid and 1.885 gr. water.

6.435 grs. of æthylopyridine platinum salt gave 2.035 grs. platinum.

	Experiment.	Calculation.	
Carbon . . .	26.33	26.81	C <sup>14</sup> 84
Hydrogen . .	2.92	3.19	H <sup>10</sup> 10
Nitrogen . .	...	5.56	N 14
Chlorine . .	...	32.93	Cl <sup>3</sup> 106.5
Platinum . .	31.62	31.51	Pt 98.7
		<hr/>	<hr/>
		100.00	313.2

The formula of the compound is C<sup>14</sup> H<sup>10</sup> N Cl + Pt Cl<sup>2</sup>.

The gold compound of æthylopyridine is obtained in fine yellow plates of extreme beauty, sparingly soluble in cold water, and readily decomposed in boiling, especially if an excess of chloride of gold be present. They were not analysed.

*Action of Iodide of Æthyle on Collidine.*

Iodide of æthyle and collidine react upon one another in the same manner as the bases already mentioned. An oily layer separates on heating the mixture, which refuses to crystallize on cooling. After removal from the tube in which the action was effected, and separation from the excess of iodide of æthyle, the fluid was allowed to stand for some time, but no crystals appeared. It was then exposed to cold, in the hope of inducing crystallization, but without success; and no better result followed the attempts made by dissolving in the smallest possible quantity of absolute alcohol, and adding æther. As the properties of the compound did not appear promising, no further experiments were made with it; but it was converted into a platinum salt, for the purpose of ascertaining whether the collidine had actually combined with æthyle. The process employed was the same as that used for preparing the æthylopicoline salt. A sparingly soluble and scarcely crystalline compound was obtained, the platinum of which was determined by the following experiment:—

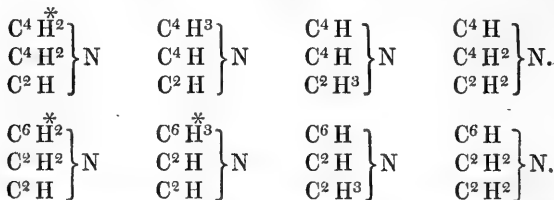
5.855 grs. of the platinum salt gave 1.618 gr. platinum.

	Experiment.	Calculation.	
Carbon . . .	...	34.06	C <sup>20</sup> 120
Hydrogen . .	...	4.50	H <sup>16</sup> 16
Nitrogen . .	...	3.68	N 14
Chlorine . .	...	29.98	Cl <sup>3</sup> 106.5
Platinum . .	27.65	27.78	Pt 98.7
		<hr/>	<hr/>
		100.00	355.2

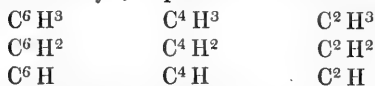
This corresponds completely with the platinum salt of æthyo-

collidine, but as that substance did not appear likely to give results of interest, I contented myself with this experiment as a sufficient proof of its existence.

The experiments described in the preceding pages sufficiently establish the fact that picoline and its homologues must be considered as nitrile bases, that is to say, bases capable of taking up only one additional atom of æthyle or any similar radical, by doing which they are converted into fixed compounds, of the class designated ammonium bases. If this be their constitution, we must, according to the views at present entertained, assume that these bases are formed from ammonia by the replacement of its three atoms of hydrogen by as many different radicals. Of the exact nature of these radicals, the experiments we at present possess afford no data for drawing definite conclusions; but a moment's consideration suffices to show that they must be substances remarkable for the simplicity of their constitution. If we confine our attention to pyridine, as the fundamental member of the series, it is obvious that the ten equivalents of carbon and five of hydrogen which it contains must be distributed among these three substances; and although we cannot, without further researches, determine how they are distributed, it is at least sufficiently obvious that the choice among different speculative arrangements is by no means large. In fact, our knowledge of the laws governing the constitution of organic compounds, enables us to see that the total number of possible permutations† of the elements of pyridine is only eight. They are as follows:—



Involving the existence of the following nine radicals, all, with the exception of methyle, at present unknown:—

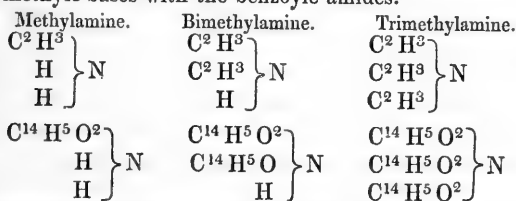


Of these, two at least,  $\text{C}^6 \text{H}$  and  $\text{C}^4 \text{H}$ , are so extremely

† I assume, with Gerhardt, that the number of atoms of carbon in any radical must always be divisible by two.

improbable, that we may, without much hesitation, pronounce against them; and if so, the probable formulæ of pyridine are reduced to those marked with an asterisk. The question for consideration is, whether even these can be supposed to represent the constitution of the base in a feasible manner. On this point no experimental evidence can at present be adduced; but taking into account all the circumstances connected with them, my impression is, that none of them give the rational expression of its constitution, and that pyridine and its homologues belong to a class of bases of which we have as yet no other examples.

In illustration of this opinion, it is necessary to enter into some details regarding the constitution of the bases generally. It is scarcely necessary to remind the reader that when Hofmann described his two new series of volatile alkaloids, he applied to those already known the name of amide, and to the new series those of imide and nitryle bases. This nomenclature, which has been more than once employed in the preceding pages, was founded on the analogy in constitution of those substances with the well-known amides, imides, and nitryles. A very little consideration, however, suffices to show that this analogy is by no means complete. The first series of bases may be correctly compared to the amides, but the other two have no close resemblance to the imides and nitryles. On the contrary, they are strictly comparable with the secondary and tertiary amides recently described by Gerhardt and Chiozza, which are formed from the primary amides by a process similar to that employed by Hofmann to produce his two classes of bases. The closeness of this analogy may be seen from the subjoined comparison of these methyle bases with the benzoyle amides.

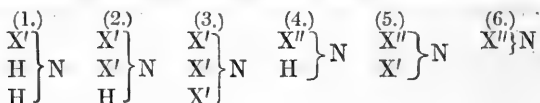


Primary benzamide.      Secondary benzamide.      Tertiary benzamide.

From which we see that in every case hydrogen is replaced, atom for atom, by a compound radical, the only difference being, that in the one set of substances the ammonia retains, in the other it loses its basic properties.

But the constitution of an imide or a nitryle is materially different. Of the former, indeed, we know too little to admit of any satisfactory conclusions regarding their constitution; but taking benzonitryle with the formula  $C^{14} H^5 N$ , as an example of

its class, and examining its constitution in the same point of view, we may consider it as an ammonia, in which three atoms of hydrogen have been replaced by a single radical  $C^{14}H^5$ . While, therefore, an amide is formed by the replacement of one or more atoms of hydrogen in ammonia by an equal number of molecules of a monobasic radical, a nitrile may be viewed as an ammonia with its three atoms of hydrogen replaced by one atom of a tribasic radical; and in the same manner there must exist a class of compounds, which for the present we may call imides, although they are not comparable with the substances known under that name, in which part of the hydrogen is replaced by a bibasic radical. The different forms of combination possible under this view may be best rendered intelligible if we make use of general formulæ, and take  $X'$ ,  $X''$ , and  $X'''$  as representing respectively a monobasic, a bibasic, and a tribasic radical. We have then the following expressions for the different classes:—



Of these the first three represent either the amides, or the bases described by Wurtz and Hofmann; the last is a nitrile, and the others are substances at present scarcely known.

Now as regards the first three classes, it is manifest that they prove amides or bases, according to the properties of the radicals replacing the hydrogen; and we may fairly argue from analogy that the members of the last may be also either basic or non-basic. The nitriles at present known are all non-basic, but it is my belief that the most probable explanation of the constitution of the bases of the pyridine series is to suppose that they are true basic nitriles, and that, for instance, in pyridine itself, the tribasic radical  $C^{10}H^5$  replaces three atoms of hydrogen in ammonia. The opinion thus expressed regarding the constitution of these bases, and even the possibility of such compounds existing, is speculative, but at the same time it is not altogether unsupported by facts, for though we have no bases in which a tribasic radical exists, there certainly are instances in which two atoms of hydrogen are replaced by a bibasic radical. A marked example is found in Gerhardt's platinamine, although there the replacing substance is not a compound but a simple radical. Its formula may be written thus:—

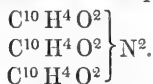


in which platinum is a bibasic radical replacing two equivalents

of hydrogen. Diplatinamine may in the same manner be represented, with its formula written thus:—

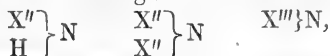


in which two equivalents of ammonia have been brought into play. Lastly, in furfurine we have a purely organic base, formed from two equivalents of ammonia by replacement of the whole of its hydrogen by three atoms of a compound radical, its formula being—



The view now expressed would make the constitution of the bases correspond very closely with that of the acids, as explained by Gerhardt. According to that chemist, a monobasic acid is formed from one atom of water (viewed as  $\text{H}^2 \text{O}$ ) by replacement of hydrogen by a monobasic radical, while a bibasic acid is formed from two atoms of water, by the replacement of two atoms of hydrogen by a bibasic radical.

I have been led into these observations by a desire to explain in a more satisfactory manner than our present knowledge of the bases will permit, the constitution of pyridine and its homologues; but I am now about to enter upon a series of experiments, with a view of obtaining some of the bases



the probable existence of which I have now indicated on theoretical grounds, which may probably form the subject of a future communication.

I may further mention, that I have found that the platinum salts of pyridine and picoline undergo a peculiar decomposition when boiled, platinum bases of very remarkable constitution being formed. I am extending this investigation to the other bases, and hope that my experiments will, at no distant date, be sufficiently advanced for publication.

XXXII. *On a more convenient form of applying Gallic Acid as a Developing Agent in Photography.* By WILLIAM CROOKES, Esq.\*

**A**MONG the more troublesome operations occurring in the practice of photography on paper, when pursued on any extensive scale, is the frequent preparation of large quantities of gallic-acid solution. I believe Mr. Spiller† was the first to suggest a means by which this difficulty may be practically

\* Communicated by the Author.

† Journal of the Photographic Society, vol. ii. p. 56.

overcome: he recommends the addition of a small proportion of alcohol or acetic acid to the aqueous solution, which will so arrest decomposition as to render it possible to keep in stock a quantity of this reagent, as we should of any other stable preparation.

Having undertaken the arrangement of the department in this Observatory in which photography is applied to the registration of the various meteorological phenomena, and having for many reasons decided upon the use of the wax-paper process\* as that best adapted to the purpose, I have naturally been anxious to avail myself of any improvements which may tend to diminish the labour or contribute to the general perfection of the result.

At first the solution of gallic acid for developing the latent image was prepared in large quantities at a time, and preserved from decomposition by the process just referred to; but the amount of liquid necessary to be prepared at once, in order to last any length of time, proved to be so bulky, that I was induced to try whether the method could not be so far modified as to admit of the water being entirely left out of the stock solution, and only added as required for use. This led to the employment of alcohol as a solvent for the gallic acid; and finding that under these conditions the character of the latter as a developing agent was greatly heightened, I resolved to make a few experiments on the subject, the result of which I beg leave to communicate, in the hope that the particulars will be found useful to others similarly situated to myself.

Two ounces of gallic acid are to be dissolved in 6 ounces of alcohol (60° over proof); to hasten solution, the flask may be conveniently heated by immersion in hot water; when cold, it should be filtered, mixed with half a drachm of glacial acetic acid, and preserved in a stoppered bottle for use; so prepared, it will keep unaltered for a considerable length of time. The gallic acid is not precipitated from this solution by the addition of water; consequently, if in any case desirable, the development of a picture may be effected with a much stronger bath than the one usually employed.

To obtain a solution of about the same strength as a saturated aqueous solution, half a drachm of the above would require to be added to 2 ounces of water; but for my particular purpose I prefer a weaker bath, which is prepared by mixing half a drachm with 10 ounces of water. In either case it will be found necessary to add solution of nitrate of silver in small quantities as the developing picture seems to require it.

Radcliffe Observatory, Oxford,  
February 1855.

\* Notes and Queries, vol. vi. p. 443.



XXXIII. *On the Gold Fish of Franklin.*

**I**N a note at the foot of page 250 of the 37th volume of the Philosophical Magazine, a question of interest both in a mechanical and electrical point of view is suggested by the following statement of Prof. W. Thomson:—"The phenomenon of a solid body," writes Mr. Thomson, "hovering freely in the air, in stable equilibrium, without any external support, has never, I am convinced, been witnessed as the result of any electrical or magnetical experiment." An experiment described by Franklin, and redescribed by Prof. Srtsczek of Pesth\*, without any knowledge of what Franklin had done previously, seems to furnish the precise fact alluded to by Prof. Thomson. If the knob of a charged Leyden jar be caused to approach a number of scraps of gold-leaf, the consequent jumping of the leaves is well known. If a certain shape be imparted to the gold-leaf, when the knob is approached the leaf is attracted and moves towards the knob; but, before it reaches the latter, it is arrested, and hovers in the air, like a fish in water. When the atmosphere is dry, the leaf can be preserved swimming for hours together "without any external support or constraint."

The following description of the experiment by Prof. Srtsczek is taken from Poggendorff's *Annalen*, vol. lxxxviii. p. 493.

Let a small Leyden jar which can be held conveniently in the hand be charged with, say positive, electricity. Let the knob be gradually brought near to several bits of gold-leaf lying upon clean paper, from 6 to 12 lines in length, and from a line to a line and a half in width: they may be rectangular, lozenge-shaped, trapezium-shaped or triangular.

A jumping of the leaves immediately commences, but soon the remarkable fact will be witnessed that one or more of the scraps will suddenly halt in their movement, and remain freely floating in the air; sometimes rotating round the knob of the jar, and showing a tendency to rotate round their own axes.

The knob must reach several inches above the insulated rim of the jar, so that scraps which hover at a considerable distance may not be attracted by the rim. At the conductor of a machine the swimming of the gold-leaf is also seen at a much greater distance (a foot for example), but the experiment does not succeed so quickly. The leaves must be smooth, and of an elongated shape.

With the same strength of charge, portions of gold-leaf, equally long, but of different shapes, hover at different distances from the knob.

\* And, we believe, exhibited in a modified form by Prof. Faraday in his public lectures.

With the same charge, and scraps tolerably alike in shape, the long ones hover at greater distances than the short ones. This is most plainly seen when the experimenter succeeds in causing several scraps to swim at the same time.

When the charge diminishes in intensity, the leaves slowly approach the knob of the jar; when, however, by means of the knob of a second jar, electricity is communicated to the former, the leaves again recede. In this manner, when the air is dry, a scrap of gold-leaf can be kept swimming for hours.

In some cases the rotation first begins when the floating leaf comes near to the jar, and the rotation is accelerated as the leaf approaches.

With scraps of a suitable shape, the equilibrium is so stable that the upright jar may be suddenly pulled downwards, without altering the hovering condition of the scrap of gold-leaf.

From the side of the leaf most distant from the jar electricity is given off of the same name as that wherewith the jar is charged; and it has the power of propagating itself to a considerable distance. When a gold-leaf electroscope is placed near the "fish," a permanent divergence is observed after the instrument has been removed.

When the swimming leaf is removed from the neighbourhood of the knob, by suddenly withdrawing the jar, it is found that those pieces which swim at about an inch distance are in a neutral condition; while those which swim at a less distance are negative. Only such as, while swimming, come very near the knob of the jar, show sometimes positive electricity.

In a note to this paper Prof. Poggendorff remarks:—"Although the more recent treatises on electricity do not mention the fact above described, still it is not new, the experiment having been made by Franklin in 1749 on the conductor of an electric machine (see *Experimental Observations on Electricity, &c.* London, 1769, p. 72). The free hovering of a piece of gold-leaf, in so stable a position, must always be regarded as a most remarkable fact, which is deserving of revival, particularly as very few appear to be acquainted with it."

In the following volume of the *Annalen*, p. 164, Prof. Riess makes the following remarks:—"A bit of gold-leaf, *differently* pointed at its ends, and with its blunter end towards the conductor of an electric machine, flies towards the latter, and remains floating at a distance from it, because the electric wind generated at the blunter end drives it back, while that generated at the sharper end and the electric attraction drive it towards the conductor. A point on the side of the strip of gold-leaf must cause an axial rotation. The same action may be observed with a sewing-needle which has its eye broken off. When it is

suspended horizontally and the knob of a Leyden jar is carefully brought near it, at a certain distance from the knob the pointed end is strongly repelled, while the blunt end is attracted."

We may add to these remarks, that in some cases we have succeeded in causing a bit of gold-leaf to flutter so speedily as almost to obscure its shape, and give it the appearance of an insect busily plying its wings. When the hand is approached, the fluttering ceases, and the little swimmer floats tranquilly in the atmosphere. So strong is the electric wind proceeding from the floating scrap, that it can be distinctly felt, as a cold draft, at a distance of 5 or 6 inches.

### XXXIV. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 156.]

Nov. 16, 1854.—Colonel Sabine, R.A., V.P. and Treas., in the Chair.

THE following communications were read:—

"Observations on the Respiratory Movements of Insects." By the late William Frederick Barlow, F.R.C.S. Arranged and communicated by James Paget, F.R.S.

This essay contains the greater part of a series of observations made between 1845 and 1850. The following are some of the conclusions which they plainly indicate:—

(1.) The respiratory movements of Dragon-flies (*Libellulæ*), and, probably, of other insects also, are naturally subject to considerable and frequent variations in force and rate, the causes of many of these variations being as yet unknown.

(2.) The respirations of these insects are always quickened by exercise, emotion, rise of temperature, galvanism, and mechanical irritation; and the last three agents quicken them in the decapitated, as well as in the perfect, insect.

(3.) The respiratory movements of each segment of the trunk are, in some measure, independent of those of the rest, although in the perfect insect they concur in all the segments. They continue to be performed, though feebly and slowly, in separated segments, provided their nervous cords and ganglia are entire: and they may be abolished in single and successive segments by the local action of chloroform.

(4.) The removal of the head, including the supra- and sub-œsophageal ganglia, does not, like the removal of the medulla oblongata of the vertebrate animal, put a stop to the respiratory movements of the insect; but it diminishes their frequency and force, and deprives them of all influence of the will and of mental emotions.

(5.) The shock inflicted by the sudden destruction of the head, or of the terminal part of the abdomen, generally stops all the respiratory movements of the insect for a time, and much enfeebles them during the remainder of its life.

(6.) The general tendency of the observations is to corroborate the opinion of the self-sufficiency of the several ganglia for the

movements of their appropriate segments, and, thus far, to maintain the belief in their essential independence. At the same time, the observations on the diffused influence of shocks accord with those of the coordinate similar movements of all the segments, in proving their close mutual relations and mutual influence.

Nov. 23, 1854.—Thomas Bell, Esq., V.P., in the Chair.

“On the Theory of Definite Integrals.” By W. H. L. Russell, Esq., B.A.

I propose in the following paper to investigate some new methods for summing various kinds of series, including almost all of the more important which are met with in analysis, by means of definite integrals, and to apply the same to the evaluation of a large number of definite integrals. In a paper which appeared in the Cambridge and Dublin Mathematical Journal for May 1854, I applied certain of these series to the integration of linear differential equations by means of definite integrals. Now Professor Boole has shown, in an admirable Memoir which appeared in the Philosophical Transactions for the year 1844, that the methods which he has invented for the integration of linear differential equations in finite terms, lead to the summation of numerous series of an exactly similar nature, whence it follows that the combination of his methods of summation with mine, leads to the evaluation of a large number of definite integrals, as will be shown in this paper. It is hence evident that the discovery of other modes of summing these series by means of definite integrals must in all cases lead to the evaluation of new groups of definite integrals, as will also be shown in the following pages. I then point out that these investigations are equivalent to finding all the more important definite integrals whose values can be obtained in finite terms by the solution of linear differential equations with variable coefficients. Again, there are certain algebraical equations which can be solved at once by Lagrange's series, and by common algebraical processes; the summation of the former by means of definite integrals affords us a new class of results, which I next consider. A continental mathematician, M. Smaasen, has given, in a recent volume of Crelle's Journal, certain methods of combining series together which give us the means of reducing various multiple integrals to single ones. The series hitherto considered are what have been denominated “factorial series;” but, lastly, I proceed to show that analogous processes extend to series of a very complicated nature and of an entirely different form, and for that purpose sum by means of definite integrals certain series, whose values are obtained in finite terms in the “*Exercices des Mathématiques*” by means of the Residual Calculus. The total result will be the evaluation of an enormous number of definite integrals on an entirely new type, and the application of definite integrals to the summation of many intricate series.

Dec. 7, 1854.—Colonel Sabine, V.P., in the Chair.

The following communications were read:—

“On the Attraction of the Himalaya Mountains, and of the elevated

region beyond them, upon the Plumb-line in India." By the Venerable John Henry Pratt, M.A., Archdeacon of Calcutta.

The author commences by observing that it is now well known that the attraction of the Himalaya Mountains, and of the elevated region beyond them, has a sensible influence on the plumb-line in North India. This circumstance was brought to light during the progress of the great trigonometrical survey of that country. It was found by triangulation that the difference of latitude between the two extreme stations of the northern division of the arc, namely Kalianpur and Kaliana, is  $5^{\circ} 23' 42'' \cdot 294$ , whereas astronomical observations show a difference of  $5^{\circ} 23' 37'' \cdot 058$ , which is  $5'' \cdot 236$  less than the former.

That the geodetic operations are not in fault, appears from this; that two bases, about 7 miles long, at the extremities of the arc having been measured with the utmost care, and also the length of the northern base having been computed from the measured length of the southern one, through a chain of triangles extending about 370 miles, the difference between the measured and the computed lengths was only 0·6 of a foot, which would produce, even if wholly lying in the meridian, a difference of only  $0'' \cdot 006$  in the latitude.

The difference  $5'' \cdot 236$  must therefore be attributed to some other cause. A very probable cause presents itself in the attraction of the superficial matter which lies in such abundance on the north of the Indian arc. It is easily seen that this disturbing force acts in the right direction, that is, it diminishes the difference of astronomical latitude between the two stations. Whether the cause here assigned will account for the error in the difference of latitude in *quantity* as well as in direction, is the question which the author proposes to discuss in the present paper.

It might seem at first sight that if mountain attraction were so influential as is here supposed, it would disturb the geodetic operations, since in observing the altitude or depression of one station as seen from another, the error in the plumb-line must come into calculation. The author shows, however, by mathematical calculation, that the effect of mountain attraction on the geodetic operations is perfectly insensible, so that it is clearly the astronomical operation of finding the difference of latitude that requires the correction. This is further apparent from the results obtained by Colonel Everest on attempting to determine the azimuths of the arc at seven stations *astronomically*.

To show the importance of considering mountain attraction in the delicate problem of the figure of the earth, the author investigates the effect of a small error in the difference of latitude of the extremities of an arc on the deduced value of the earth's ellipticity. As two unknown quantities occur in the determination of the spheroid of revolution which most nearly agrees with the earth, namely  $a$  the equatorial radius and  $\epsilon$  the ellipticity, two arcs are required in order to determine them. The author selects the Russian arc, measured near North Cape, as the most advantageous with which to combine the northern portion of the Indian arc, and shows that an error of  $5'' \cdot 236$  in

defect in the amplitude of the latter would diminish the value of the ellipticity resulting from the two by about the  $\frac{1}{25}$ th part of the whole. If the effect of mountain attraction be as great as the author calculates it to be, ( $15''\cdot885$  in the northern portion of the Indian arc,) the ellipticity would be diminished by  $\frac{1}{8}\epsilon$ , and even by as much as  $\frac{1}{6}\epsilon$  if the whole Indian arc from Kaliana to Damargida were employed.

The author then proceeds, first to develop his method of calculation, and then to reduce his formula to numbers, according to the best data which he was able to collect.

An expression is first investigated for the horizontal attraction of a prism of the earth's crust standing on a given small base, having a small height, and situated at a given angular distance (measured from the centre of the earth) from the station, A, at which the attraction is sought. In the cases to which this expression is employed it reduces itself without sensible error to

$$\frac{M}{a^2} \cos \left( \frac{1}{2}\theta \right),$$

where M is the attracting mass,  $a$  the chord joining its base with A, and  $\theta$  the angle subtended by this chord at the earth's centre.

In applying this expression to the problem in hand, the author divides the earth's surface into lines, by vertical planes passing through at equal angular distances. These lines are further subdivided by small circles having A for their common pole, and in this manner cutting the whole surface into curvilinear quadrilaterals. He then investigates what the law of dissection must be, that is, according to what law the radii of the small circles must be taken to increase, in order that the horizontal attraction of the portion of the crust standing on one of the quadrilaterals may be equal to the product of its average height and density by a constant quantity, independent of the distance of the quadrilateral from A. If  $\alpha$  and  $\alpha + \phi$  be the angular radii of two consecutive small circles, there results

$$\frac{\phi \cos^2 \left( \frac{1}{2}\alpha + \frac{1}{4}\phi \right)}{\sin \left( \frac{1}{2}\alpha + \frac{1}{4}\phi \right)} = \text{a constant quantity} = c.$$

To fix the value of this constant, the author assumes  $\phi = \frac{1}{10}\alpha$  when

$\phi$  and  $\alpha$  are indefinitely small, which gives  $c = \frac{4}{21}$ . The above equation may then be solved numerically with sufficient approximation. In this manner a table is calculated of the radii of the successive small circles.

These distances should be laid down, and the circles drawn, on a map or globe, as well as the lines dividing the surface into lines. Nothing then remains to be done but to ascertain the average heights of the masses standing on the compartments thus drawn.

The author's paper was accompanied by a plate representing an

outline of the continent of Asia. On this was laid down a polygonal figure DEFGHIJKL, (which for convenience the author calls the "enclosed space,") marking the boundary of an irregular mass, which is the only part of the earth's surface that appears to have a sensible effect on the plumb-line in India. The boundary of this space is thus defined:—

DEF is the Himalaya range, having a bend at E from north-west on the left, to east-by-south on the right. FG is a range running to the table-land of Yu-nan in lat.  $25^{\circ}$  and long.  $103^{\circ}$ . GH is the range of the Yun-Ling mountains, in which there are many peaks of perpetual snow. HI is the Inshan range. IJ is the Khing-khan range, very steep on the east side, not so on the west; the passes are said to be 5525 feet above the sea. JK is the Altai range, the highest peak of which is 10,800 feet; the average height is 6000: the range declines towards the east. KL was once thought to be a range of mountains, but is now found to be a line of broken country. LD is the Bolor range, rising to an elevation similar to that of the Hindoo Koosh. There are besides these two ranges of mountains running into the enclosed space, parallel to the Altai and Southern Himalayas, namely the Thian-Schan range, or Celestial Mountains, and the Kuen-Luen range, being a continuation of the Hindoo Koosh, which rises from an altitude of 2558 feet near Herat to about 20,000 where it meets the Bolor range. It is, however, with the elevation of the enclosed space itself that we are principally concerned, since ranges of mountains have not so important an influence, when distant, as table-lands of elevation.

Before describing the country within these limits, the author gives a general sketch of the parts which lie outside, from which it appears that the calculations may be confined to the enclosed space. He then describes in detail the nature of the country within the boundaries of the enclosed space, commencing with the Himalayas, which rise abruptly from the plains of India to 4000 feet and more, and cover an extensive broken space some 100 or 200 miles wide, rising to great heights; perhaps 200 summits exceed 18,000 feet; the highest reaches to more than 28,000. The general base on which these peaks rest rises gradually to 9000 or 10,000 feet, where it abuts on the great plateau north of the range. The character of the country to the south of this plateau is much better known than that to the north. If a circle with a radius  $5^{\circ}046$  (the value of one of the radii employed in the dissection) be drawn around Kaliaua, it will pass over the highest part of this plateau. This circle divides the enclosed space into two portions, of which the southern is called by the author the "Known" and the northern the "Doubtful Region." The effects of the two portions are separated in the calculation by the introduction of an arbitrary factor.

After describing the doubtful region, as far as was possible from the data to which he had access, the author assumes, as the best general representation of the facts, that to the north of a line running through Leh and H'Lassa the doubtful region slopes gradually from 10,000 feet down to 2500 along a parallel line nearly in its centre, and then rises again at the same angle to the north, and that

the portion to the south of the line first mentioned, and not included in the known region, slopes at four times that rate.

The author then proceeds to numerical summations replacing an integration to be extended over the whole of the enclosed space. The breadth of the lines employed in the calculation is taken at  $30^\circ$ , which is shown not to be too large to give good results. The following are the results obtained:—

	Arising from		Total.
	Known region.	Doubtful region.	
Station A, Kaliaua.			
Deflexion of plumb-line in meridian	12 <sup>''</sup> ·972	14 <sup>''</sup> ·881	27 <sup>''</sup> ·853
Correction of same for every 100 feet of change in heights . . . . }	0·312	0·260	
Deflexion of plumb-line in prime vertical . . . . . }	8·136	8·806	16·942
Station B, Kaliaupur.			
Deflexion in meridian . . . . .	3·219	8·749	11·968
Correction for 100 feet . . . . .	0·059	0·158	
Deflexion in prime vertical . . . . .	0·789	3·974	4·763
Station C, Damargida.			
Deflexion in meridian . . . . .	1·336	5·573	6·909
Correction for 100 feet . . . . .	0·022	0·100	
Deflexion in prime vertical . . . . .	0·000	2·723	2·723

whence there results,

Total deflexion at A =  $32^{\circ} \cdot 601$ , and in azimuth  $31^{\circ} 18'$  East.

Total deflexion at B =  $12^{\circ} \cdot 880$ , and in azimuth  $21^{\circ} 42'$  East.

Total deflexion at C =  $7^{\circ} \cdot 426$ , and in azimuth  $21^{\circ} 31'$  East.

Difference of meridian deflexions at A and B =  $15^{\circ} \cdot 885$ .

Difference of meridian deflexions at A and C =  $20^{\circ} \cdot 944$ .

Difference of meridian deflexions at B and C =  $5^{\circ} \cdot 059$ .

The first of these differences is considerably greater than  $5^{\circ} \cdot 236$ , the quantity brought to light by the Indian Survey.

The author then examines these values more minutely, and considers the effect of various hypotheses for reducing them.

In the first place, the density of the attracting mass may have been assumed too large. The density assumed is  $2 \cdot 75$  that of distilled water, the value assumed as the mean density of the mountain Schehallien in the calculations of Maskelyne. This can hardly be too great, but at any rate no remarkable supposition relative to the density can reduce the attraction by more than a small fraction of the whole.

Next, the mass of the doubtful region may have been assumed too great. This hypothesis is then examined by the author, who concludes that even the extravagant supposition of the non-existence of that region will not reduce the difference of meridian deflexions at A and B lower than to  $9^{\circ} \cdot 753$ .

A third means of reduction may be looked for in the known region. A large part of the attraction belonging to this region arises from



the Great Plateau. It would be necessary to cut down this plateau as much as 6000 feet to reduce the deflexions at A and B to 5''·236, even were the whole mass on the doubtful region non-existent; so that it appears to be quite hopeless, by any admissible hypothesis relative to heights, densities, &c., to reduce the calculated deflexion so as to make it tally with the error brought to light by the survey.

After entering into some elaborate calculations confirmatory of the previous results, the author concludes by calculating the form of the Indian arc, that is, by determining what spheroid of revolution,—the axis of revolution being the earth's axis,—would most nearly coincide with that arc without reference to the rest of the earth, the data employed being the lengths and amplitudes of the northern and southern portions of the arc, and of course their sum, and likewise the latitudes, or at least approximate latitudes, of the middle points of the arcs. By using the amplitudes uncorrected for mountain attraction, the author obtains for the value of the ellipticity deduced from the Indian arc alone  $\frac{1}{1963}$ , nearly agreeing with  $\frac{1}{1916}$ , which is Col. Everest's result; but by using the amplitudes corrected for mountain attraction according to the author's calculation, the ellipticity is reduced to  $\frac{1}{4269}$ . He concludes that the arc is more curved than it would be if it had the mean ellipticity of the earth, and regards the supposition of a general deviation of the earth's surface in that region from the mean spheroidal form as the most satisfactory mode of accounting for the discrepancy.

“On the Value of Steam in the Decomposition of neutral Fatty Bodies.” By George Wilson, Esq.

In the course of a long series of experiments conducted on a large scale, the author has observed that the so-called neutral fatty bodies may be resolved, without danger of injurious decomposition, into glycerine and fatty acids, provided the still is maintained at a uniform high temperature, and that a continuous current of steam is admitted into it.

The temperature required to effect the splitting of the fats into their proximate elements varies with the nature of the body itself, but all hitherto tried may be resolved into glycerine and fatty acid at a temperature of 560° Fahr., many at much below that temperature. At a further period it is the author's intention to lay before the Society a detailed account of his experiments, with the confirmatory analyses, but in the mean time he states that palm oil, coconut oil, fish oil, animal tallow, Bornean vegetable tallow, “Japan vegetable wax” (more properly tallow), and several others have yielded satisfactory results, the fatty acid and glycerine distilling over together, but no longer in combination, and separating in the receiving vessel.

Dec. 14, 1854.—The Lord Wrottesley, President, in the Chair.

“On the Structure of some Limestone Nodules enclosed in Seams of Bituminous Coal, with a Description of some Trigonocarpons contained in them.” By J. D. Hooker, M.D., F.R.S., and E. Binney, Esq.

The authors first describe the occurrence of the limestone nodules, which form a continuous bed in the centre of a thin seam of bitumi-

nous coal in the lower part of the Lancashire coal-field. The nodules were of various sizes, some weighing many pounds, and caused the coal to bulge out both above and below them, and they were found to be entirely composed of vegetable tissues converted into carbonate of lime and magnesia. Their formation is supposed by the authors to be due to infiltration of water through the superincumbent shales, which were full of fossil shells supposed to be of marine origin, and the aggregation of the mineral matter round centres of vegetable remains. The chemical constituents of the nodules were found to be carbonates of lime and magnesia, sesquioxide and sulphate of iron, with a little carbonaceous matter.

The probability of these nodules representing an average sample of the vegetable constituents of the surrounding coal is then discussed, and attention is drawn to the very great interest and importance that would attach to them were such a view substantiated, as showing the exact nature of the association of plants which is capable of conversion into bituminous coal.

All the plants contained in the nodules were common in other parts of the coal formation, viz. *Calamodendron*, *Halonina*, *Sigillaria*, *Lepidodendron*, *Stigmara*, *Trigonocarpon*, *Anabothra*, and others; of these the first-named genus occurred in the greatest abundance and as large fragments of fossil wood. Very many of the specimens were sliced, and being reduced to very thin transparent sections, were examined with the view of determining the botanical character of their contents, and the intimate structure of the masses of more or less homogeneous aspect to which they were reduced by decomposition, previous to or during the operation of calcification. The results were very satisfactory, and seemed to indicate that all traces of vegetable structure may be completely obliterated in the substance of highly bituminized coal, which may nevertheless also contain fragments of wood with their tissues preserved.

An account is then given of the examination of the details of structure of *Trigonocarpon*, and this, as well as the comparison of *Trigonocarpon* with the modern genus *Salisburia*, is illustrated by drawings and analyses.

The authors are still engaged with the study of these nodules, with the view of showing the relationship between *Calamodendron*, *Calamites*, *Sigillaria* and *Anabothra*, and the details are preparing for publication.

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### XXXV. Intelligence and Miscellaneous Articles.

#### OBJECT OF SALT IN THE SEA.

AT a recent Meeting of the Canadian Institute, a very interesting paper was read by Professor Chapman, of University College, Toronto, on the "Object of the salt condition of the Sea." Professor Chapman began by assuming that the sea was created salt from the beginning, and for some beneficent purpose; and then proceeded to discuss the views hitherto advanced in elucidation of this object. The suggestion, that the sea is salt in order to preserve it in a state of purity, was considered to be untenable for several

reasons; mainly, however, from the fact, that organic impurities when diffused through a vast body of moving water, whether fresh or salt, become altogether, and very rapidly lost; so much so, indeed, as apparently to have called forth a special agency to arrest the total annihilation of organized matter in its final oscillation between the organic and inorganic worlds. The author alluded to the countless hosts of microscopic creatures which swarm in most waters, and whose principal function has been ably surmised by the great anatomist, Professor Owen, to be that of feeding upon, and thus restoring to the living chain, the almost unorganized matter diffused through their various zones of habitation. These creatures preying upon one another, and being preyed upon by others in their turn, the circulation of organic matter is kept up, and carried through its appointed rounds. If we do not adopt this view, we must at least look upon the Infusoria, the Foraminifera, and many of the higher types, as scavenger agents appointed to prevent an undue accumulation of decaying matter; and thus, as before, the salt condition of the sea does not become a necessity. It was shown also, that, under many circumstances, the amount of saline matter in the sea is not sufficient to arrest decomposition. Other suggestions, to the effect that the sea is salt in order to render it of greater density, and by lowering its freezing-point to preserve it from congelation to within a shorter distance of the poles, were then discussed in their more prominent relations. The value of these suggestions in a secondary point of view was fully admitted, but shown, at the same time, to be scarcely adequate to meet the entire solution of so vast and grand a problem as that which is manifestly involved in the salt condition of the sea. The freezing-point of sea water, for instance, is only  $3\frac{1}{2}^{\circ}$  F. lower than that of fresh water; and hence with the present distribution of land and sea,—and still less, probably, with that which obtained in former geological epochs—no very important effects would have resulted had the ocean been fresh instead of salt. So far as regards the habitable portions of the world, the present difference would be next to nothing. Professor Chapman here submitted to the Institute a suggestion which he believed to be original, in reference to the question under discussion. He considered the salt condition of the sea as mainly intended to regulate evaporation, and to prevent an undue excess of that phenomenon under the influence of any disturbing causes that might from time to time arise. It is well known that under the same atmospheric pressure different liquids have very different boiling-points; and in like manner, saturated solutions evaporate more slowly than weak ones, and these latter more slowly again than pure water. In sea water we have on an average about  $3\frac{1}{2}$  per cent. of solid matter, of which about 2.6 consists of chloride of sodium or common salt. The results of certain experiments by the author in reference to evaporation on weighed quantities of ordinary rain-water, and water holding in solution 2.6 per cent. of salt, were then given. The excess of loss of the rain-water over the water of the salt solution was, for the first twenty-four hours, 0.54 per cent.; at the close of forty-eight hours, 1.04 per cent.; after seventy-two hours, 1.46 per cent.,

and so on, always in an increasing ratio; the experiments in each case being carried on for six days.

Here, then, we have a self-adjusting phenomenon, one of those admirable contrivances in the balance of forces, which an attentive study of nature reveals to us in every direction. If, other conditions being equal, any temporary cause render the amount of saline matter in the sea above its normal value, evaporation goes on the more and more slowly; and, on the other hand, if this value be depreciated by the addition of fresh water in undue excess, the evaporating power is the more and more increased—thus aiding time, in either instance, to restore the balance. In conclusion, the author pointed out that the consideration of this principle might shed some further light on the geographical distribution of fresh and salt water lakes on the present surface of the globe.

ON AN APPEARANCE SEEN IN THE MOON.

BY ROBERT HART, ESQ.

(Letter to the Astronomer Royal.)

“On the night of the 27th December, 1854, between 6 and 7 P.M., the moon was very bright. I had brought my 10-inch reflector to bear upon the moon; on the shaded side of the disc I observed a white spot, where I have marked it on the sketch. As it was of the colour of the light of the moon, and not like starlight, I thought it part of the moon; but as it disappeared in less than a minute after I first observed it, I concluded it was a star eclipsed by the moon\*. I now turned my attention to the light part of the disc, and my eye was at once attracted by an appearance I had never seen before on the surface of the moon, although I have observed her often during these last forty years. She was 8<sup>d</sup> 4<sup>h</sup> old at the time, and just on the edge of the light, where I have marked on the sketch, there were *two luminous spots*, one on either side of a small ridge, *which ridge was in the light*, and of the same colour as the moon; but these spots were of a *yellow flame colour*, while all the rest of the enlightened part was of a snowy white, and the mountain-tops that were coming into the light, and just on the shadow side of these spots, were of the same colour as the moon. The lights of these spots were like the light of the setting sun reflected from a window a mile or two off. I observed it for five hours. I thought them rather less bright than as first seen, *but very little less*; so bright were they, when the instrument was the least thing out of focus, they showed *rays around them as a star would do*.

“As I live about two miles out of Glasgow, I had no scientific friend with me at the time, but I called the attention of three gentlemen, my neighbours, and my own household, and they all described the appearance as I saw it myself, and have given above.

“I would have followed it longer had I been able, but the wind was very cold, 20°·5, and I had no shelter, as I take my telescope

\* This was, no doubt, the occultation of  $\mu$  Piscium, which, according to the Nautical Almanac, was in conjunction with the moon at 6<sup>h</sup> 54<sup>m</sup> 10<sup>s</sup>, on the evening of December 27, 1854.

outside. I left off about 12 P.M.; and from the cloudy weather, I never saw the moon until she was nearly full, then only a few minutes, and again on the 10th January, at 4 A.M.; but the haze was too thick. I thought I might see them in the shade, but I could not: I have never seen her since.

"May I ask the favour of your laying this before the Astronomical Society, that we might learn if any of the members have observed this appearance in the moon, or have heard that it was seen by others. I shall be happy to learn if it has been taken notice of by any other observer, and their opinion of it.

"It appeared to me, from the brightness of the light and the contrast of colour, to be two active volcanos or two mouths of one in action.

"If there was no star eclipsed by the moon at or about 7 P.M. that night, the white spot that was more like one of the mountain-tops tipped with light than a star, may have had some connexion with it."—*Proc. Roy. Astron. Soc.* 1855, p. 89.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1855.

*Chiswick*.—Jan. 1. Cloudy: boisterous. 2. Densely clouded: slight rain: overcast and mild. 3. Very fine. 4. Overcast. 5. Very fine. 6. Overcast. 7. Densely clouded. 8. Overcast. 9. Fine: slight rain. 10. Foggy: very dense fog at night. 11. Foggy. 12. Foggy: hazy clouds. 13. Overcast: fine: cloudy: clear and frosty. After unusually fine dry weather, a most severe period now commences. 14. Fine: cloudy: clear with sharp frost. 15. Hazy: frosty. 16. Slight rain: frosty. 17. Slight snow: clear and frosty. 18. Frosty: clear: snow. 19. Sharp frost: uniformly overcast: clear and frosty. 20. Cloudy: snowing. 21. Sharp frost: frosty haze. 22. Snowing: hazy. 23. Snowing: overcast. 24. Overcast: frosty. 25. Fine: frosty. 26. Hazy. 27. Foggy: fine: frosty. 28. Overcast. 29. Snow showers. 30. Snowing. 31. Overcast: windy: drifting snow.

Mean temperature of the month ..... 33°·45

Mean temperature of Jan. 1854 ..... 37·88

Mean temperature of Jan. for the last twenty-nine years ... 37·06

Average amount of rain in Jan. .... 1·74 inch.

*Boston*.—Jan. 1. Cloudy: stormy. 2. Cloudy. 3. Fine. 4—9. Cloudy. 10. Fine. 11, 12. Cloudy. 13. Cloudy: rain P.M. 14—16. Cloudy. 17. Cloudy: snow A.M. 18—20. Cloudy. 21, 22. Cloudy: snow A.M. and P.M. 23. Cloudy: snow A.M. 24—26. Cloudy. 27. Fine. 28. Cloudy. 29. Fine: snow P.M. 30, 31. Cloudy.

*Sandwick Manse, Orkney*.—Jan. 1. Sleet-showers A.M.: show-showers, frost P.M. 2. Cloudy A.M.: fog P.M. 3. Drizzle A.M.: drizzle, showers P.M. 4. Bright A.M.: rain P.M. 5. Showers A.M.: showers, thunder and lightning P.M. 6. Showers, bright A.M.: showers, thunder and lightning P.M. 7. Drops A.M.: drizzle P.M. 8. Rain A.M. and P.M. 9. Clear A.M. and P.M. 10. Clear, frost A.M.: rain P.M. 11. Drizzle A.M.: cloudy P.M. 12. Cloudy A.M. and P.M. 13. Cloudy, fine A.M.: fog P.M. 14, 15. Cloudy A.M. and P.M. 16. Showers, bright A.M.: cloudy P.M. 17. Clear, frost A.M.: fine, rain P.M. 18. Frost, showers A.M.: damp P.M. 19. Drizzle A.M.: drizzle, showers P.M. 20. Damp, fine A.M.: fine, clear P.M. 21. Cloudy A.M.: fine, cloudy P.M. 22. Showers A.M.: sleet P.M. 23. Bright, frost A.M.: clear, frost P.M. 24. Bright, frost A.M.: fine, frost P.M. 25. Cloudy A.M.: rain P.M. 26. Cloudy A.M.: snow-showers P.M. 27. Bright, frost A.M.: snow-showers P.M. 28. Snow-showers A.M. and P.M. 29—31. Snow, clear A.M.: frost, clear P.M.

Mean temperature of Jan. for twenty-eight previous years ... 38°·38

Mean temperature of this month ..... 38·16

Mean temperature of Jan. 1854 ..... 36·46

Average quantity of rain in Jan. for fourteen previous years . 4·38 inches.

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

Days of Month.	Chiswick.		Barometer.		Orkney, Sandwick.		Thermometer.			Wind.			Rain.		
	Max.	Min.	Boston		9½ a.m.	8½ p.m.	Boston	Chiswick.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
			9½ a.m.	8½ p.m.											
1.	29'951	29'919	29'44	29'31	29'73	36½	34½	46	53	46	W.	W.	W.	W.	W.
2.	30'069	30'028	29'72	29'88	29'88	36	39	44	51	44	NW.	NW.	NW.	NW.	NW.
3.	30'170	30'139	29'74	29'83	29'79	46	46½	44	52	39	NW.	NW.	NW.	NW.	NW.
4.	30'218	30'164	29'72	29'65	29'40	48	48½	37	47	38	W.	W.	W.	W.	W.
5.	30'132	30'062	29'66	29'52	29'35	40½	42	40	51	40	SW.	SW.	SW.	SW.	SW.
6.	30'361	30'240	29'81	29'88	30'01	40½	43	48	50	44	SW.	SW.	SW.	SW.	SW.
7.	30'530	30'428	30'00	29'86	29'99	48	48½	42	49	42	calm	calm	calm	calm	calm
8.	30'464	30'303	29'77	29'78	29'78	48	48½	41	45	45	W.	W.	W.	W.	W.
9.	30'405	30'283	29'83	30'15	30'38	50	28	45	50	28	W.	W.	W.	W.	W.
10.	30'525	30'495	30'20	30'36	30'28	36	39	27	35	27	W.	W.	W.	W.	W.
11.	30'525	30'457	30'16	30'19	30'44	48	41½	29	35	28	W.	W.	W.	W.	W.
12.	30'543	30'519	30'22	30'55	30'58	41	41½	32	41	32	W.	W.	W.	W.	W.
13.	30'536	30'432	30'22	30'54	30'51	42	26	34	43	26	W.	W.	W.	W.	W.
14.	30'468	30'434	30'15	30'47	30'47	43	43	33	43	33	W.	W.	W.	W.	W.
15.	30'456	30'223	30'08	30'34	30'31	35	21	34	44	21	W.	W.	W.	W.	W.
16.	30'081	30'017	29'77	30'28	30'35	37	38	22	35	22	W.	W.	W.	W.	W.
17.	30'077	30'011	29'90	30'28	30'36	32	20	28	35	20	W.	W.	W.	W.	W.
18.	30'118	29'955	29'87	30'23	30'19	34	18	28	39	18	W.	W.	W.	W.	W.
19.	29'984	29'879	29'73	30'15	30'14	28	13	28	40	13	W.	W.	W.	W.	W.
20.	29'839	29'702	29'70	30'17	30'16	31	18	30	38½	18	W.	W.	W.	W.	W.
21.	29'841	29'724	29'65	30'12	30'11	34	17	29	37	17	W.	W.	W.	W.	W.
22.	29'803	29'762	29'50	29'97	29'91	35	18	30	38	18	W.	W.	W.	W.	W.
23.	29'868	29'807	29'64	29'95	30'15	34	24	31	36	24	W.	W.	W.	W.	W.
24.	30'003	30'000	29'77	30'23	30'26	38	23	34	35	23	W.	W.	W.	W.	W.
25.	30'003	29'871	29'66	30'02	30'12	37	17	32	33	17	W.	W.	W.	W.	W.
26.	29'972	29'946	29'70	29'88	29'85	34	15	23	36	15	W.	W.	W.	W.	W.
27.	29'934	29'847	29'66	29'88	29'82	36	26	32	35	26	W.	W.	W.	W.	W.
28.	29'730	29'597	29'45	29'84	29'83	35	17	23	28	17	W.	W.	W.	W.	W.
29.	29'718	29'669	29'54	29'85	29'90	33	25	30	29	25	W.	W.	W.	W.	W.
30.	29'498	29'425	29'40	29'92	29'97	31	23	28	28	23	W.	W.	W.	W.	W.
31.															
Mean.	30'124	30'043	29'79	30'056	30'074	39'87	27'03	34'7	38'08	38'25	0'10	0'43	3'26		

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XXXVI. *Elementary Demonstrations of Propositions in the Theory of Magnetic Force.* By Professor W. THOMSON\*.

*Def. 1.* **T**HE lines of force due to any magnet or electro-magnet, or combination of magnets of any kind, are the lines that would be traced by placing the centre of gravity of a very small steel needle, perfectly free to turn about this point, in any position in their neighbourhood, and then carrying it always in the direction pointed by the magnetic axis of the needle.

*Remark.* Except in the cases of symmetrical magnets, the lines of force will generally be lines of double curvature, and no set of them will lie in one plane.

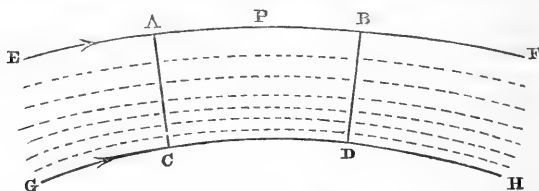
*Def. 2.* The lines of component force in any plane are the lines traced by placing the centre of gravity of a steel needle anywhere in this plane, and carrying it always in this plane in the nearest direction to that pointed by its magnetic axis; that is, the direction of the orthogonal projection of the magnetic axis on the plane; or the direction that the steel needle would point with its magnetic axis if placed with it in the plane, and free to turn about an axis through its centre of gravity perpendicular to the plane.

*Prop. 1.* If the line of component magnetic force through any point in a plane be curved at this point, the force will vary in a line perpendicular to the line of force in its plane, *increasing* in the direction *towards* the centre of curvature.

Let EABF be a line of component force in the plane of the diagram, and let GCDH be another near it, each and all between

\* Communicated by the Author.

them being curved in the same direction, the arrow head on each indicating the way a north pole would be urged. Let AC, BD be lines drawn perpendicular to all the lines of component force between these two. Because of the curvature of these lines, the lines AC and BD (whether straight or curved) must be so inclined to one another that the portion CD cut off from the last shall be less than the portion AB cut off from the first. Let a north pole of an infinitely thin, uniformly and longitudinally magnetized bar, of which the south pole is at a great distance from the magnets, be carried from D to C along the line of component force through these points, from C to A perpendicular to all the lines of force traversed, from A to B again along a line of force, and lastly, from B to D perpendicular to the lines of force. Work must be spent on it in carrying it from C to D, and work is gained in passing it from A to B. Then, because no work is either gained or spent in carrying it from C to A or from B to D, the work gained in moving along AB cannot exceed the work spent in the first part of the motion, or else we should have a perpetual development of energy from no source\*, by simply letting the cycle of motion be repeated over and over



again: and the work spent along DC cannot exceed that gained from A to B, or else we might have a perpetual development of energy from no source, merely by reversing the motion described, and so repeating. The work spent and gained in the motions along DC and AB respectively must therefore be exactly equal. Hence the mean intensity of the force along CD, which is the

\* [Note added March 26, 1855.]—It might be objected, that perhaps the magnet, in the motion carried on as described, would absorb heat, and convert it into mechanical effect, and therefore that there would be no absurdity in admitting the hypothesis of a continued development of energy. This objection, which has occurred to me since the present paper was written, is perfectly valid against the reason assigned in the text for rejecting that hypothesis; but the second law of the dynamical theory of heat (the principle discovered by Carnot, and introduced by Clausius and myself into the dynamical theory, of which, after Joule's law, it completes the foundation) shows the true reason for rejecting it, and establishes the validity of the remainder of the reasoning in the text. In fact, the only absurdity that would be involved in admitting the hypothesis that there is either more or less work spent in one part of the motion than lost in the



shorter of the two paths, must exceed the mean intensity of the force along the other; and therefore the intensity of the force increases from P in the perpendicular direction towards which the concavity of the line through it is turned.

*Prop. II.* The augmentation of the component force in any plane at an infinitely small distance from any point, towards the centre of curvature of the line of the component force through it, bears to the whole intensity at this point the ratio of the infinitely small distance considered, to the radius of curvature.

If, in the diagram for the preceding proposition, we suppose AB and CD to be infinitely near one another, and each infinitely short, they will be infinitely nearly arcs of circles with infinitely nearly equal radii. Hence the difference of their lengths must bear to either of them the ratio of the distance between them to the radius of curvature. But the mean intensities along these lines must, according to the preceding demonstration, be inversely as their lengths, and hence the excess of the mean intensity in CD above the mean intensity in AB must bear to the latter the ratio of the excess of the length of AB above that of CD to the latter length; that is, as has been shown, the ratio of the distance between AB and CD to the radius of curvature.

*Prop. III.* The total intensity does not vary from any point in a magnetic field to a point infinitely near it in a direction perpendicular to the plane of curvature of the line of force through it.

*Prop. IV.* The total intensity increases from any point to a point infinitely near it in a direction towards the centre of curvature of the line of force through it, by an amount which bears to the total intensity itself, the ratio of the distance between these two points to the radius of curvature.

These two propositions follow from the two that precede them by obvious geometrical considerations.

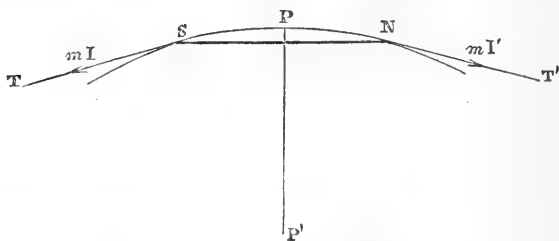
[They are equivalent to asserting, that if X, Y, Z denote the components, parallel to fixed rectangular axes, of the force at any

other, would be the supposition that a thermo-dynamic engine could absorb heat from matter in its neighbourhood, and either convert it wholly into mechanical effect, or convert a part into mechanical effect and emit the remainder into a body at a higher temperature than that from which the supply is drawn. The investigation of a new branch of thermo-dynamics, which I intend shortly to communicate to the Royal Society of Edinburgh, shows that the magnet (if of magnetized steel) does really experience a cooling effect when its pole is carried from A to B, and would experience a heating effect if carried in the reverse direction. But the same investigation also shows that the magnet must absorb just as much heat to keep up its temperature during the motion of its pole *with* the force along AB, as it must emit to keep from rising in temperature when its pole is carried *against* the force, along DC.

point whose coordinates are  $(x, y, z)$ , the expression  $Xdx + Ydy + Zdz$  must be the differential of a function of three independent variables.]

*Examination of the Action experienced by an infinitely thin uniformly and longitudinally magnetized bar, placed in a non-uniform Field of Force, with its length direct along a line of force.*

Let SN be the magnetized bar, and ST, NT' straight lines touching the line of force in which, by hypothesis, its extremities lie, and P a point on it, midway between them. The resultant force on the bar will be the resultant of two forces pulling its ends in the lines ST, NT'. If these two forces were equal (as they would be if the intensity of the field did not vary at all along a line of force, as for instance when the lines of force are concentric circles, as they are when simply due to a current of electricity passing along a straight conductor; or if P were in a situation between two dissimilar poles symmetrically placed on each side of it), the resultant force would clearly bisect the angle between the lines TS, T'N, and would therefore be perpendicular to the bar and to the lines of force in the direction towards which they are curved; that is (Prop. IV.), would be from places of weaker to places of stronger force, perpendicularly across the lines of force. On the other hand, if the line of force through P has no curvature at this point, or no sensible curvature as far from it as N and S, the lines NT and ST' will be in the same straight line, and the resultant force on the bar will be simply the excess of the force on one end above that on the other acting in the direction of the greater; and since in this case (Prop. IV.) there is no variation of the intensity of the



force in the field in a direction perpendicular to the lines of force, the resultant force experienced by the bar is still simply in the direction in which the intensity of the field increases, although this is now a direction coincident with a line of force. Lastly, if the intensity increases most rapidly in an oblique direction in the field, from P in some direction between PS and

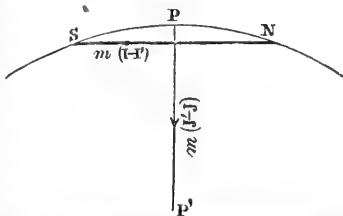
$PP'$ , there must clearly be an augmentation (a "component" augmentation) from  $P$  towards  $P'$ ; and therefore (Prop. IV.) the line through  $P$  must be curved, with its concavity towards  $P'$ , and also a "component" augmentation from  $N$  towards  $S$ , and therefore the end  $S$  must experience a greater force than the end  $N$ . It follows that the magnet will experience a resultant force along some line in the angle  $SNP'$ , that is, on the whole from places of weaker towards places of stronger force, obliquely across the lines of force.

*Prop V. (Mechanical Lemma.)* Two forces infinitely nearly equal to one another, acting tangentially in opposed directions on the extremities of an infinitely small chord of a circle, are equivalent to two forces respectively along the chord and perpendicular to it through its point of bisection, of which the former is equal to the difference between the two given forces and acts on the side of the greater; and the latter, acting towards the centre of the circle, bears to either of the given forces the ratio of the length of the arc to the radius.

The truth of this proposition is so obvious a consequence of "the parallelogram of forces," that it is not necessary to give a formal demonstration of it here.

*Prop. VI.* A very short, infinitely thin, uniformly and longitudinally magnetized needle, placed with its two ends in one line of force in any part of a magnetic field, experiences a force which is the resultant of a longitudinal force equal to the difference of the forces experienced by its ends, and another force perpendicular to it through its middle point equal to the difference between the force actually experienced by either end, and that which it would experience if removed, in the plane of curvature of the line of force, to a distance equal to the length of the needle, on one side or the other of its given position.

$NS$  being the bar as before, let  $I$  denote the intensity of the force in the field at the point occupied by  $N$ ,  $I'$  the intensity at  $S$ ,  $J$  the intensity at  $P$  on the line of force midway between  $S$  and  $N$ , and  $J'$  the intensity at a point  $P'$ , at a distance  $PP'$  equal to the length of the bar, in a direction perpendicular to the line of force. Then if  $m$  denote the strength of magnetism of the bar,  $mI$  and  $mI'$  will be the forces on its two extremities respectively. Hence by the mechanical lemma, the resultant of these forces will be the same as the resultant of a force  $m(I - I')$  acting along the bar in the



direction SN, and a force perpendicular to it towards the centre of curvature, bearing the same ratio to either  $mI$  or  $mI'$ , or to  $mJ$  (which is their mean, and is infinitely nearly equal to each of them), as NS to the radius of curvature, or (by Prop. II.) the ratio of the excess of the intensity at  $P'$  above that at  $P$  to the intensity at either, that is the ratio of  $J' - J$  to  $J$ , and therefore itself equal to  $m(J' - J)$ . The bar therefore experiences a force the same as the resultant of  $m(I - I')$  acting along it from S towards N, and  $m(J' - J)$  perpendicularly across it towards  $P'$ , through its middle point.

*Cor.* The direction of the resultant force on the bar is that in which the total intensity of the field increases most rapidly; or, which is the same, it is perpendicular to the surface of no variation of the total intensity.

*Prop. VII.* The resultant force on an infinitely small magnet of any kind placed in a magnetic field, with its magnetic axis along the lines of force, is in the line of most rapid variation of the total intensity of the field, and is equal to the magnetic moment of the magnet multiplied by the rate of variation of the total intensity per unit of distance; being in the direction in which the force *increases* when the magnetic axis is "direct," (that is, in the position it would rest in if the magnet were free to turn about its centre of gravity).

*Cor. 1.* The resultant force experienced by the magnet will be in the contrary direction, that is, the direction in which the total intensity of the field diminishes most rapidly, when it is held with its magnetic axis reverse along the lines of force of the field.

*Cor. 2.* A ball of soft iron, or of any non-crystalline paramagnetic substance, held anyhow in a non-uniform magnetic field, or a ball or small fragment of any shape, of any kind of paramagnetic substance whether crystalline or not, left free to turn about its centre of gravity, will experience a resultant force in the direction in which the total intensity of the field increases most rapidly, and in magnitude equal to the magnetic moment of the magnetization induced in the mass multiplied by the rate of variation of the total intensity per unit distance in the line of greatest variation in the field. For such a body in such a position is known to be a magnet by induction, with its magnetic axis direct along the lines of force.

*Cor. 3.* A ball of non-crystalline diamagnetic substance held anyhow in a magnetic field, or a small bar or fragment of any shape of any kind of diamagnetic substance, crystalline or non-crystalline, held by its centre of gravity, but left free to turn about this point, experiences the same resultant force as a small steel or other permanent magnet substituted for it, and held with

its magnetic axis reverse along the lines of force. For Faraday has discovered, that a large class of natural substances in the stated conditions experience no other action than a *tendency from places of stronger towards places of weaker force, quite irrespective of the directions the lines of force may have*, and he has called such substances diamagnetics.

*Cor. 4.* A diamagnetic, held by its centre of gravity but free to turn about this point, must react upon other magnets with the same forces as a steel or other magnet substituted in its place, and held with its magnetic axis reverse along the lines of force due to all the magnets in its neighbourhood.

*Cor. 5.* Any one of a row of balls or cubes of diamagnetic substance held in a magnetic field with the line joining their centres along a line of force, is in a locality of less intense force than it would be if the others were removed; but any one ball or cube of the row, if held with the line joining their centres perpendicularly across the line of force, is in a locality of more intense force than it would be if the others were removed.

*Cor. 6.* When a row of balls or cubes, or a bar, of perfectly non-crystalline diamagnetic substance, is held obliquely across the lines of force in a magnetic field, the magnetic axis of each ball or cube, or of every small part of the substance, is nearly in the direction of the lines of force, but slightly inclined from this direction towards the direction perpendicular to the length of the row or bar. Hence, since the magnetic axis of every part differs only a little from being exactly *reverse along the lines of force*, the direction of the resultant of the couples with which the magnets, to which the field is due, act on the parts of the row or bar must be such as to turn its length along the lines of force.

*Cor. 7.* The positions of equilibrium of a row of balls or cubes rigidly connected, or of a bar of perfectly non-crystalline diamagnetic substance, free to move about its centre of gravity in a perfectly uniform field of force, are either with the length along or with the length perpendicularly across the lines of force: positions with the length along the lines of force are stable; positions with the length perpendicularly across the lines of force are unstable.

*Cor. 8.* The mutual influence and its effects, referred to in Cors. 5, 6, 7, is so excessively minute, that it cannot possibly have been sensibly concerned in any phænomena that have yet been observed; and it is probable that it may always remain insensible, even to experiments especially directed to test it. For the influence of the most powerful electro-magnets induces the peculiar magnetic condition of which diamagnetics are capable, to so slight a degree as to give rise to only very feeble, scarcely sensible, mutual force between the diamagnetic and the

magnet; and therefore the magnetizing influence of a neighbouring diamagnetic, which could scarcely, if at all, be observed on a piece of soft iron, must be inappreciably small on another diamagnetic.

*Cor. 9.* All phenomena of motion that have been observed as produced in a diamagnetic body of any form or substance by the action of fixed magnets or electro-magnets, are due to the resultant of forces urging all parts of it, and couples tending to turn them; the force and couple acting on each small part being the same as it would be if all the other parts were removed.

*Cor. 10.* The deflecting power (observed and measured by Weber) with which a bar of non-crystalline bismuth, placed vertically as core in a cylinder electro-magnet (a helix conveying an electric current), urges a magnetized needle on a level with either of its ends, is the reaction of a tendency of all parts of the bar itself from places of stronger towards places of weaker force in its actual field.

The preceding investigation, leading to Props. VI. and VII., is the same (only expressed in non-analytical language) as one which was first published in the Cambridge and Dublin Mathematical Journal, May 1846. The chief conclusions now drawn from it, with particulars not repeated, were stated in a paper entitled 'Remarks on the Forces experienced by inductively magnetized Ferromagnetic or Diamagnetic Substances,' in the Philosophical Magazine for October 1850.

Glasgow College,  
March 15, 1855.

### XXXVII. On *British Pectolites*.

By Dr. M. FORSTER HEDDLE, and R. P. GREG, Esq.\*

**K**OBELL'S mineral *pectolite*, found in the amygdaloid of Monte Baldo in the Southern Tyrol, is introduced into Dr. Thomson's 'Mineralogy' with the original analysis (No. 16) misquoted, the quantity of water being given as 8.89 instead of 3.89. This blunder explains the fact, that in another part of his work, Thomson describes, under the name of *Wollastonite*, a mineral found in greenstone near Kilsyth, which, however, is evidently *pectolite*, the analysis (No. 2) also agreeing.

Dr. Thomson states that the mineral he analysed was also afterwards found by Lord Greenock in the neighbourhood of Edinburgh, and Mr. Rose informs us that the precise locality was the Costorphine Hill. Now Walker has given an analysis (No. 3) of a mineral from the Costorphine Hill which is evi-

\* Communicated by the Authors.

dently also *pectolite*; it is included in mineralogical works under the head of *table spar*, but containing soda, it must be *pectolite*, which differs from *table spar* merely in silicate of soda replacing silicate of lime. Walker's mineral then in all probability is identical with the mineral discovered by Lord Greenock; we are not, however, aware that it has been lately found at this place.

Again, Thomson describes among the zeolites a mineral to which he has given the name of "*stellite*," found in greenstone at Kilsyth; the original specimen analysed by Dr. Thomson we have lately seen, and do not hesitate in saying it is ordinary *pectolite*, as indeed its analysis (made by Dr. Thomson evidently on an impure portion) had before led us to suppose.

Mr. Rose of Edinburgh states, that Lord Greenock obtained at Loch End a weathered specimen of this mineral, but we have no analysis of this specimen.

Then we have the so-called "*Wollastonite*" of the Castle Rock of Edinburgh\*, the analysis of which, according to Kennedy, is given as No. 4 in the Table.

Dr. Thomson sets aside this analysis, and states that the *Wollastonite* of the Edinburgh mineralogists is *prehnite*. White *prehnite* does occur along with the Castle Rock mineral, and as our analysis agrees with the ordinary ones of *pectolite*, the conclusion is that Thomson's specimen contained *prehnite*, but none of the substance in question.

Our analyses, made on 25 grains, afforded,—

	<i>a.</i>	<i>b.</i>
Silica . . . .	55·12	53·06
Alumina . . . .	·30	46
Oxide of iron . . .	·52	...
Lime . . . .	33·36	33·48
Soda . . . .	7·85	9·98
Potash . . . .	·36	·29
Water . . . .	3·13	<i>not det.</i>

The second specimen was evidently the purest.

Next we have to notice the specimens found by Mr. Forrest in greenstone at Ratho, near Edinburgh. Here it occurs of a pale green colour, and in orbicular masses with a fibrous and radiating structure, at a depth of 90 feet, associated with *steatite*, *calcite*, *heavy spar*, *specular iron*; also in pseudomorphs after *analcime*, although *analcime* has not itself yet been found; and is found (though rarely) in distinct crystals. The fibrous variety is extremely tough when first obtained, but soon weathers, becoming asbestos-like on exposure.

\* Occurring in orbicular masses in trap, translucent and highly crystalline, with a radiating structure.

The specific gravity of the fibrous variety is 2·881, and our analysis on 25 grains is given under *c*; that of the crystals taken on 5 grains is given under *d*.

	<i>c.</i>	<i>d.</i>
Silica . . .	52·525	52·580
Alumina . . .	·884	1·456
Lime . . .	32·794	33·750
Magnesia . . .	trace	
Soda . . .	9·752 (with some potash)	9·258
Water . . .	3·040	2·800
	<hr/> 98·995	<hr/> 99·844

About three years ago Mr. M'Gibbon found a mineral at Knockdolian Hill, near Ballantrae in Ayrshire, the matrix of which was limestone.

Its specific gravity is 2·778, and our analysis, made on 25 grains, is given in the Table, No. 8; and this mineral is therefore *pectolite*. At this locality it also occurs in needle crystals.

Some time ago we got this mineral from Talisker in Skye associated with *mesotype* and *stilbite*; the analysis is No. 9. Dr. Scott of Edinburgh has analysed this mineral, with results differing in the soda and the water, the quantity of the former being 7·670, and of the latter 5·058 (see No. 10).

Our last locality is on the shore near Girvan in Ayrshire, where the mineral occurs in quantity, and of considerable beauty, hitherto known to collectors as white *tremolite*; here it varies from highly translucent and fibro-crystalline to opaque, being sometimes, however, as tough as the *okenite* from Faroe.

Our analysis on 25 grains afforded,—

Silica . . . . .	53·480
Alumina . . . . .	·055
Oxide of iron . . . . .	·005
Lime . . . . .	34·384
Magnesia . . . . .	traces
Soda . . . . .	9·877
Potash . . . . .	·355
Water . . . . .	3·262

The mineral is apparently purer at this locality than at any other.

We have *pectolite* occurring in Scotland at the following localities: Kilsyth, Costorphine Hill, Loch End, Edinburgh Castle, Ratho, Knockdolian Hill and Girvan in Ayrshire, and at Talisker in Skye: and we now have analyses of specimens from all these localities, with the single exception of Loch End\*. Specimens

\* According to Prof. R. D. Thomson, *pectolite* has occurred also at Bishoptown, and near Kilpatrick.



from the Morne Mountains, said to be this *mineral*, have by our own analysis proved to be table spar, and consequently the only locality we have as yet of this mineral in the British Islands.

Frankenheim argues that *pectolite* is anhydrous, chiefly because he wishes to prove it to be a *hornblende*. This is quite inadmissible; from not one locality has it been found anhydrous, and the per-centages from the different localities do not vary much, averaging about 3 per cent.; and although the proportion of water does vary *half a per cent.* when the powdered mineral has been dried at 212°, yet when this necessary precaution has been taken, the proportion is in each specimen invariably the same; any previous variation must therefore have been due to hygrometric moisture. In addition to this, we find the mineral to be very slightly absorbent; that from Ratho absorbs .53 per cent. of moisture, that from the Castle Rock .29 per cent., and that from Ayrshire .35 per cent.; again, it parts with the water it does contain only after the most intense heating. The Castle Rock mineral, after about ten minutes' exposure to a red heat, gave only 1.2 per cent. of water; a loss in the analysis led us to redetermine the water, when, after exposing the mineral to a heat approaching to whiteness for thirty minutes, we got 3.13 per cent. Seeing, then, that the mineral in powder is almost *non-absorbent*, and that it retains the water it does contain with such tenacity, it appears to us to be an extraordinary conclusion to arrive at to say that water is unessential; and to exclude a constantly present constituent merely to suit a theory, is absurd.

There is a difference of opinion with regard to the formula of this mineral, and its place in the system. Dr. Scott rejects the formula given by Berzelius, and prefers one of his own, which agrees admirably with his analysis; but as the quantity of water as therein stated is much greater than elsewhere found, his formula of course does not suit the analyses of others.

Frankenheim also rejects Berzelius's formula, and offers instead,—



the calculated per-centages of which are,—

Silica 15 atoms = 52.76

Lime 16 atoms = 34.29

Soda 4 atoms = 9.53

Water 5 atoms = 3.42

This, it must be allowed, agrees well with the analytical results; but the question is, does it agree more closely than Berzelius's; for Frankenheim substituted this formula in order that he might, by writing it more simply  $4(\text{CaO}, \text{NaO}) 3\text{SiO}^3 + 11\text{O}$  (where the

CaO is to the NaO in the proportion of 4 to 1), and rejecting the HO, make the mineral come under the hornblende formula, viz.—



RO standing for CaO, MgO, FeO<sup>3</sup>, Al<sup>2</sup>O<sup>3</sup>, NaO.

We have, in calculating the formula from our analyses, chosen the crystals from Ratho as in all probability the purest:—

	Oxygen.	Atoms.
SiO <sup>3</sup> , 52.580 = 26.209 = 11.03 = 11		11
CaO, 35.206 = 10.016 = 4.25 = 4		12
NaO, 9.258 = 2.376 = 1 = 1		3
HO, 2.800 = 2.489 = 1.05 = 1		3

giving the formula—



the calculated per-centages of which are,—

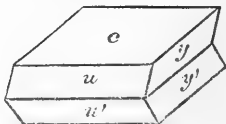
Silica 11 atoms = 508409 = 52.574
Lime 12 atoms = 337956 = 34.947
Soda 3 atoms = 93629 = 9.683
Water 3 atoms = 27039 = 2.796
937043 100.000

This, it will be allowed, is a still closer approximation than the per-centages of the hornblendic formula, and this also is the formula given by Berzelius.

Now this formula points, not to *hornblende*, but to *augite*, for it will be seen that it is the same as that of *table spar*, 3CaO, 2SiO<sup>3</sup>; only neutral silicate of soda replaces silicate of lime, and *table spar* is an *augite*, the formula of which is 3RO, 2SiO<sup>3</sup> (where RO may stand for CaO, MgO, NaO, Fe<sup>2</sup>O<sup>3</sup>, Al<sup>2</sup>O<sup>3</sup>, &c.); for if it is allowable to disregard for the time being the HO, in order to bring the mineral among the *hornblendes*, the same course cannot be objected to, to bring it among the *augites*.

The crystalline form of *pectolite*, as we have ascertained from the Ratho crystals, is the same as *Wollastonite*, with which it is therefore isomorphous. The accompanying figure represents the usual form of these crystals.

$$\begin{aligned} c : u &= 95 \overset{\circ}{2} 3 \overset{l}{3} \\ c : y &= 54 \ 05 \ (125^\circ \ 55') \\ y : u &= 93 \ 30 \end{aligned}$$



Cleavage highly perfect, parallel to *c* and *u*. Twin face *c*. The face *y* coming in the zone *z*, *e*, (*y*), *x* of Brooke and Miller, appears to be hemihedral in the Ratho crystals, and is a plane

not given in their edition of Phillips, though probably identical with the *h*1 of Allan's 'Phillips,' p. 47, where  $P : h1 = 126^\circ$ , and  $M : T = 95^\circ 20'$ .

We have also obtained cleavages of about  $95^\circ 30'$  with highly crystalline specimens of *pectolite* from Monte Baldo in the Tyrol, and the Castle Rock, Edinburgh.

*Pectolite*, which as a characteristic species has not received as much attention as it deserves, is rather an abundant mineral, especially in Scotland, and is identical with the *Wollastonite* and *stellite* of Thomson, with the *stellite* of Sheppard, and with the *osmelite* of Breithaupt.

Table of Analyses.

No.	Locality.	Si.	Ca.	Na.	H.	Al, Fe, Mg, Ka.	Analysér.
1.	Monte Baldo, Tyrol ...	52.34	35.20	9.60	2.80	traces	Kobell, 2nd analysis.
2.	Kilsyth, Scotland ("Wollastonite")	52.74	31.68	9.60	2.00	3.39	
3.	Costorphine Hills .....	54.00	30.79	5.55	5.43	2.59 (Mg)	Walker.
4.	Castle Rock, Edinburgh	51.50	32.00	8.50	5.00	1.00	Kennedy.
5.	Castle Rock, Edinburgh	53.06	33.48	9.98	3.13	0.75	Dr. Heddle.
6.	Ratho ("fibrous") .....	52.525	32.794	9.752	3.040	0.884	Dr. Heddle.
7.	Ratho ("crystallized")	52.580	33.750	9.258	2.800	1.456	Dr. Heddle.
8.	Knockdolian Hill, Ayreshire.....	53.240	32.220	9.570	3.600	1.002	Dr. Heddle.
9.	Talisker, Skye.....	53.820	29.880	9.551	3.760	2.728	Dr. Heddle.
10.	Talisker, Skye.....	52.000	32.850	7.670	5.058	1.820	Dr. Scott.
11.	Girvan, Ayrshire.....	53.48	34.384	9.877	3.262	0.415	Dr. Heddle.
12.	Bishoptown, Renfrewsh.	52.07	32.80	9.60	2.00	4.20	Dr. Thomson.
13.	Bavaria ("osmelite")...	52.91	32.96	Na 6.10 Ka 2.79	4.01	0.86	Adam.
14.	Bergen Hill, New Jersey ("stellite") ...	54.00	32.10		8.89	2.96	1.90
15.	Isle Royale, Lake Superior .....	55.00	32.53	9.72	2.75	1.10	Dickenson.
16.	Monte Baldo, Tyrol ...	51.30	33.77	8.26	3.89	2.47	Kobell, 1st analysis.

XXXVIII. *Magnetic Remarks*.

By PROFESSOR FARADAY, D.C.L., F.R.S. &c.

MY DEAR TYNDALL,

IN relation to your letter of last month\*, I write, not for the purpose of giving what might be taken as an answer, but to say that it seems to me expedient and proper to wait and allow the thoughts that my papers may raise, to be considered and judged of at their leisure by those who are inclined to review and advance the subject. Perhaps, after a respectful interval, I may

\* Phil. Mag. 1855, vol. ix. p. 205.

be induced to put forth such explanations, acknowledgements, or conclusions, as the state of the subject may then seem to render necessary or useful.

In the mean time, the more we can enlarge the number of anomalous facts and consequences the better it will be for the subject; for they can only remain anomalous to us whilst we continue in error. I may say, however, that the idea you suggest presents no difficulty to me; for having on former occasions (Exp. Res. 2501.) had to consider the magnecrystallic phenomena presented by the same body in different media, and having found the magnecrystallic difference unchanged in the media, I have no difficulty in conceiving that a body (as bismuth), which in the amorphous state is of the same magnetic character as the medium around it, shall, when employed as a crystal, be paramagnetic in one direction and diamagnetic in another (3157.). What happens in a medium may, according to my knowledge of the facts, happen in space; and is in full accordance with Thomson's clear paper on the theory of magnetic induction in crystalline bodies\*.

In respect of the effects of pressure, to which you refer in your letter, we cannot easily draw conclusions on either side until we know better what pressure does. I am not aware whether you consider that pressure on bismuth, whilst it makes the metal more diamagnetic in one direction than another, also makes it more diamagnetic as a whole than before; or whether you suppose it *less* diamagnetic in the transverse direction of the pressure than at first. Gmelin says, on the authority of Marchand and Scheerer (vol. iv. p. 428), that the density of bismuth is diminished as pressure upon it is increased, and extraordinary as the fact seems, gives densities of the following degree for increasing pressures, 9.783, 9.779, 9.655, 9.556; a change in texture at the same time occurring. If the statement be true, then the line of pressure in your beautiful experiments may be the line of *least density* or of *least approximation*, though I hardly know how to think so; still it becomes difficult for us to draw reasons from the constitution of a compressed body, until we know what happens during the compression, although no difficulty arises in considering it, after compression in one direction, like to a magnecrystallic substance.

You are aware (and I hope others will remember) that I give the lines of force† only as *representations* of the magnetic power, and do not profess to say to what physical idea they may hereafter point, or into what they will resolve themselves. Ad-

\* Phil. Mag. 1851, vol. i. p. 177.

† It is nearly twenty-four years since I first called attention to these lines; Exp. Res. 114, note.

vancing no principle, I say, that the hypothetical fundamental ideas already advanced, when taken in relation to the body of facts now known, are self-contradictory and inapplicable. The following points, namely,—that the *direction* and *polarity* of lines of magnetic force are always shown truly by the electric current induced in metal moving within their influence;—that the dualities of electricity and magnetism are always respectively and essentially related;—that the dualities of an isolated magnet are not related back in straight lines through the magnet;—are to my mind not hypothetical in character, but easily proveable by experiment:—and they, with the considerations arising from the principle of the conservation of force, seem to me to be left unexplained by, and in opposition to, the usual hypotheses. No difference arises about the laws of magnetic action and their mathematical development; and that, simply because they are as yet applied only partially, and thus far are in accordance with *all* the views taken, including mine. When the attempt is made to apply them so as to include at once *paramagnetic*, *diamagnetic*, and *electro-magnetic* phænomena, and at the same time to deduce them from *one* hypothetical cause, then they may become so large and yet precise as to enable us to distinguish between true and false assumptions. On my part I endeavour not to assume anything, but only to draw such conclusions from the assumptions already made, and the phænomena now discovered, as seem subject to experiment and tangible by facts.

Some persons may feel surprised that I dwell upon points which are perfectly and mathematically explained by the hypothesis of two magnetic fluids, as, for instance, places of little or no action (3341. &c.). My reason is, that being satisfied by the phænomena of diamagnetism, &c. that that hypothesis cannot be true, all these and such like phænomena acquire a new character and a high importance which they had not before, and amongst other philosophical uses, point most emphatically to the essential relation of the dualities and their equivalency in power. They do not contradict the old hypothesis when that is partially applied, but they are not the less strong and striking as evidence in favour of the view of lines of force.

I am, my dear Tyndall,

Yours very faithfully,

M. FARADAY.

Royal Institution,  
March 14, 1855.

XXXIX. *On the Benzole Series.—Determination of Boiling-points.* By ARTHUR H. CHURCH, Esq.\*

SINCE 1825, when Faraday discovered its first member, our knowledge of the hydrocarbons of the benzole series has remarkably extended: it is interesting to see from what various sources these hydrocarbons have been derived; and although doubts still remain as to the relations of these bodies to one another, yet their composition has been ascertained with certainty. Without taking notice of all these sources, it may suffice to mention that the five known members of the series occur in coal-naphtha, and some of them in the oil separated by the addition of water to crude wood-spirit; benzole having also been obtained from oil-gas and from benzoic acid; toluole from resin-gas, balsam of Tolu, dragon's blood and toluylic acid; while cumole has been procured from phorone and from cuminic acid, cymole from camphor and from cumin oil.

In experiments made for the purpose of obtaining a liquid appropriate for the preservation of highly oxidizable metals, I was surprised to find (oxygen being excluded) that on the application of heat, sodium immersed in toluole perfectly free from moisture soon became tarnished and evolved gas. This evolution ceased after a time, and the metallic lustre of pieces of sodium placed in the liquid remained unimpaired.

After many attempts to ascertain the origin of these phenomena, and after having satisfied myself experimentally of the absence of water from the toluole under examination, the observation was made that toluole, which exerted no further action on sodium, tarnishes it after redistillation in the ordinary manner, and it was ultimately found that the occasion of this was a product of oxidation. This oxidation may be prevented, if care be taken that the upper part of the retort do not reach a higher temperature than that at which the liquid boils. Toluole purified by sodium and distilled from a retort immersed in a bath of dilute chloride of calcium solution exerts no further action on sodium, and comes over to the last drop quite colourless and leaving no residue. Toluole, which, in addition to the ordinary methods of purification, had been treated with oil of vitriol and with solution of chromic acid, behaved in the same manner.

I was anxious to ascertain whether the boiling-point of toluole was altered by the treatment with sodium. I employed in the experiments several ounces of toluole from coal-naphtha, which, purified in the ordinary manner, had distilled over between  $108^{\circ}$  and  $109^{\circ}$  C.; I also made use of toluole from toluylic acid. The thermometers employed (three) were finely divided, and had

\* Communicated by the Author.

been accurately tested, the errors being known up to 200° C. Into a small retort of symmetrical shape was introduced a coil of platinum wire, and about 100 grammes of toluole repeatedly purified with sodium. Before a perfectly trustworthy result could be arrived at, there were several precautions to be taken. An allowance had to be ascertained and made for irregularities introduced by the partial immersion of the thermometer-stem in the vapour; and it was found that when the spheroids which form upon the surface of the liquid touch the bulb of the thermometer (immersed in the vapour), they cause variations in the height of the mercury, thus interfering with the constancy of the boiling-point. Remembering these things, and taking care to place the bulb of the retort in a chloride of calcium bath, I observed the boiling-point of purified toluole, the bulb of the thermometer just touching the surface of the liquid. The temperature of the liquid rose gradually to 97° C.; from this point to 103° the rise was rapid. Between 103° and 104° eight-tenths of the toluole distilled over; and on redistillation, the boiling-point remained constant at 103°·6, the barometric pressure being 0<sup>m</sup>·757. With toluole (purified by sodium) prepared from toluylic acid, exactly the same phænomena were observed.

Having now given the more important details of my procedure for the preparation, &c. of pure toluole, I may mention that I subjected benzole (from benzoic acid) to the same treatment: the boiling-point of benzole thus obtained, and that (from coal tar) purified by repeated crystallizations, is, however, identical. Of the xylene (obtained from coal-naphtha and wood-spirit), I collected for experiment that which came over at about 128°. After purification with potash, sulphuric acid, solution of chromic acid, and with caustic baryta and sodium, the boiling-point was but slightly reduced. The cumole (from cuminic acid and coal-naphtha) was purified in the same way, as was also the cymole (from oil of cumin and coal-naphtha).

I give here the (corrected) boiling-points of the liquids thus purified at a uniform barometric pressure of 0<sup>m</sup>·760.

	Formula.	Boiling-point.	Difference.
Benzole . . .	$C^{12}H^6 = C^6 3(C^2 H^2)$	80·8 }	22·9 22·5 22·2 22·3
Toluole . . .	$C^{14}H^8 = C^6 4(C^2 H^2)$	103·7 }	
Xylene . . .	$C^{16}H^{10} = C^6 5(C^2 H^2)$	126·2 }	
Cumole . . .	$C^{18}H^{12} = C^6 6(C^2 H^2)$	148·4 }	
Cymole . . .	$C^{20}H^{14} = C^6 7(C^2 H^2)$	170·7 }	

In the following table will be found the observations of other experimentalists compared with my own.

Benzole.	Toluole.	Xylole.	Cumole.	Cymole.
85°·5 Faraday.	114° Gerhardt.	129° Cahours.	144° 151°·4 Gerhardt and Cahours.	171° Mansfield.
86° Mitscherlich.	113° Mansfield.	126°·2 Church.	148° F. Abel.	171°·5 Noad.
80°·4 Kopp.	108° Deville, Pelletier, and Walter.		148°·4 Church.	175° Gerhardt.
80°·5 Mansfield.				170°·7 Church.
80°·8 Church.	109°·5 Noad.			
	110° Wilson.			
	106° Glénard and Boudault.			
	103°·7 Church.			

My own results are, I believe, strictly comparable with one another, having been obtained by the employment of the same apparatus in similar circumstances. The differences in the results of other observers may arise from very many causes, and in most cases no particular accuracy was aimed at. Kopp found that with benzole from benzoic acid (not purified by distillation from potassium, &c.), the temperature indicated by a thermometer with the bulb immersed in the vapour, differed from that indicated when the bulb was in the liquid by 0°·9 C. ; with perfectly pure benzole, and if care be taken to adopt all the precautions I have mentioned, the difference is greatly diminished.

After obtaining the results which have been given in the present communication, I examined some of the distinguishing reactions of the bodies operated upon, in order to satisfy myself of their identity with those described under their respective names. As the boiling-point of toluole observed by me differed so considerably from those observations previously announced, I made analyses of the purified toluole employed in my experiments. These were perfectly satisfactory, as were also determinations of the sulphate of baryta in sulphotoluolate of baryta,  $C^{14} \left\{ \begin{matrix} H^7 \\ SO^2 \end{matrix} \right\} BaSO^4$ ,



and in nitrosulphotoluolate of baryta,  $C^{14} \left\{ \begin{array}{c} H^6 \\ SO^2 \\ NO^4 \end{array} \right\} Ba SO^4$ : the

details and numerical results of these analyses, and of many others which the present inquiry necessitated, the limits and special object of the present paper do not admit of my giving here. The last-mentioned acid may be obtained most readily by a method which suggested itself to me when working upon the corresponding term of the benzole series; by it I have obtained nitrosulphoxylic and -cumolic acids. As benzole with fuming sulphuric acid yields sulphobenzolic acid, I imagined that nitrobenzole would yield, when similarly treated, nitrosulphobenzolic acid; and this supposition experiment proved to be correct. It is not possible to obtain in this way sulphocymolic acid, for the compound  $C^{20} \left\{ \begin{array}{c} H^{13} \\ NO^4 \end{array} \right\}$  is not yet known; this acid may, however, be obtained easily by dissolving sulphocymolic acid in fuming nitric acid. The salts of these nitrosulpho-acids deflagrate when heated; in the case of the barium compounds, the residual sulphate of baryta assuming the form of long, narrow ribands.

Bases prepared from impure hydrocarbons of the benzole series give, on analysis, results tolerably accordant with theory: this circumstance, if not entirely accounted for by the numerous processes of purification which these bases undergo, is explained when we recollect the recent researches of C. G. Williams\* 'On the Fractional Crystallization of Platinum Salts†.'

Many subjects for further investigation have suggested themselves to me during the progress of the present inquiry; among these the subject of isomers is of great interest. The important observations, too, of Regnault and others as to the alterations in their boiling-points which many liquids undergo when heated under pressure, I have found to refer to benzole, &c.; on this account, in purifying these liquids, I was obliged to desist from digesting them in sealed tubes. A digestion of toluole with sodium in a closed vessel for a fortnight causes the elimination of two bodies, the boiling-point ( $97^\circ$ ) of one being intermediate between that of benzole and toluole, the boiling-point of the second ( $112^\circ$ ) being between those of toluole and xylene. And I have lately found, that among the products of the distillation of eugenic acid with an excess of baryta, a liquid occurs for which my experiments (not made with a perfectly pure substance) indicate a formula  $C^{18}H^{12}$ ; for  $C^{20}H^{12}O^4 - C^2O^4 = C^{18}H^{12}$ .

\* Phil. Mag. Sept. 1854.

† Anderson's observations on the boiling-points of certain bases (Phil. Mag. Feb. 1855) are also exceedingly curious

From this hydrocarbon, nitro-substitution products may be obtained, and I have very little doubt that it is an isomer of cumole. It boils at about  $142^{\circ}$ . It would seem, therefore, that there are three bodies having the formula  $C^{18}H^{12}$ ; cumole boiling at  $148^{\circ}\cdot4$ , mesitilole boiling at  $155^{\circ}$ , and the hydrocarbon from eugenic acid boiling at  $142^{\circ}$ . May not these bodies, and those obtained from toluole, stand in similar relations to one another? The same differences between their boiling-points are observable. I am still investigating this question with reference to the other members of the benzole series, and endeavouring by the differences of specific gravity, vapour density, refractive power, &c. of these liquids, to discover their true relations to one another.

By referring to the table given before, it will be found that, according to my experiments, the increment in the boiling-point in the benzole series for an addition of  $C^2H^2$  varies between  $22^{\circ}\cdot9$  and  $22^{\circ}\cdot2$ . By determining the boiling-points of the consecutive members of several series, in which not only carbon and hydrogen enter, but also oxygen and nitrogen, we shall be able to learn whether such formulæ\* as those of Schröder will prove to any extent generally applicable. The substances in such experiments must of course be absolutely pure. The present contribution, having reference to a simple series containing only carbon and hydrogen, may be considered introductory. Schröder remarks, that between those boiling-points which are theoretically determinable from his formulæ and those experimentally observed, a difference of  $\pm 7^{\circ}\cdot2$ ,  $\pm 14^{\circ}\cdot4$  may be found. Since the remarks upon the isomers of cumole and toluole were written, I have noticed the close approximation of the differences between the boiling-points of these isomers to this difference of  $7^{\circ}\cdot2$ .

February 1855.

*XL. On a New Single Fluid Galvanic Battery, more powerful, and less expensive in construction and use, than any of the Nitric Acid Batteries. By the Rev. N. J. CALLAN, Professor of Natural Philosophy in the Roman Catholic College, Maynooth†.*

**I**N a paper published in the Philosophical Magazine for February 1854, I stated that sheet iron coated with an alloy of lead and tin, in which the quantity of lead is five or six times as great as that of tin, and afterwards platinized, might be substituted with advantage for the platinized silver used in Smee's battery. Being under a necessity of exhibiting occasionally during the academical year, optical experiments which required a very strong light, I was very anxious to have a galvanic bat-

\* Not, however, based on arbitrary numbers.

† Communicated by the Author.

tery in which there would be no need of porous cells or nitric acid, and which, with a moderate number of plates, would produce a brilliant coke light or lime light. From experiments made with our large galvanometer on the voltaic power of a pair of zinc and platinized coated iron plates excited by dilute sulphuric acid, I inferred that a battery containing a hundred, or one hundred and fifty such plates, would produce the light which I required. Having more than twenty Wedgwood troughs, each containing ten cells, I saw that the cost of such a battery would be very moderate: I therefore resolved to make one of 200 pairs of plates. Whilst the battery was being made, I occasionally tried the power of one trough or ten cells. I first used a solution of sulphuric acid consisting of one part of strong acid and about seven of water. I then tried one part of the same acid mixed with about six times its bulk of a pretty strong solution of common salt. The addition of the salt produced a considerable increase of voltaic power. I next employed an exciting mixture consisting of one part of nitric, six of sulphuric acid, and about fifty of water. With this mixture, ten pairs of plates, each about 6 inches square, ignited a pair of coke-points, which gave a small but brilliant star of light. But the nitric acid gradually dissolved the alloy of lead and tin with which the iron plates were coated, and the platina powder fell off. I then tried one part of nitric acid, twelve of sulphuric, and about a hundred of water. This mixture also acted on the coating of the iron plates. I was therefore obliged to give it up, and to return to the solution of sulphuric acid and common salt.

Before the entire battery was finished, I tried the power of 100 cells in igniting a pair of coke points. I was greatly disappointed by the feeble illuminating power of the coke light, and began to examine whether any of the cheap negative metals could be advantageously used with an exciting fluid which would contain a small portion of nitric acid. One of the first which occurred to me was cast iron, which acted so well as the negative element of the nitric acid battery. I prepared a cast-iron cell of the nitric acid battery, and a zinc plate, which I separated from the iron by pieces of wood. The cell was filled with one part of nitric, six of sulphuric acid, and about fifty of water. The galvanic action was very powerful, but the zinc plate was soon covered with a deposit of nitrate of iron. It was evident, then, that cast iron and zinc could not be excited with any fluid containing nitric acid unless the two metals were separated by a porous cell. I then tried the same cast-iron cell and zinc plate excited with one part of sulphuric acid and seven of water. The galvanometer showed that the voltaic current was very feeble. I therefore gave up all hope of making a battery such as I

wanted, by exciting zinc and cast iron with dilute sulphuric acid.

During the evening of the day on which I tried the cast iron cell excited with dilute sulphuric acid, it struck me that, on having increased the strength of the acid solution by pouring a little acid into the cell, a strong impulse was given to the needle of the galvanometer, and that perhaps, by increasing very much the strength of the solution, I might get a very powerful galvanic current. I then resolved to try concentrated sulphuric acid and dilute acid of the various degrees of strength between concentrated acid and one part of acid mixed with eight of water.

The sulphuric acid which I used was very strong and pure, being made, not from pyrites, but from sulphur. In comparing the galvanic powers of the various batteries which I tried, I always used our large galvanometer, and sent the voltaic current only through the outermost coil, the diameter of which is about 25 inches. The coil is made of copper wire  $\frac{3}{8}$ ths of an inch thick. The outside coil is nearly 7 feet long. The galvanometer may be used as a sine or tangent galvanometer.

I began by exciting the cast-iron cell and zinc plate with concentrated sulphuric acid: the galvanic current produced was exceedingly feeble. I then mixed a small quantity of water with the acid: the deflection of the needle was greatly increased. I continued to pour water gradually into the cast-iron cell, and found that the deflection of the needle increased until the acid was diluted to a certain degree, and that when it was diluted still further, the deflection was diminished, and became constantly less as the quantity of water was increased. After five or six hours spent in frequently charging the cell with dilute sulphuric acid of various degrees of strength, I found that the acid which I used, diluted with twice its bulk of water, produced the most powerful galvanic current; and that when the acid was diluted with more or less than twice its bulk of water, the voltaic power of the cell was diminished. I afterwards found that acid which was still stronger than the very strong acid which I first used, and which was also made from sulphur, and not from pyrites, required to be diluted with three times its bulk of water in order to produce the maximum of galvanic power, or to produce a current equal in power to that which was produced by one part of the first acid diluted with twice its bulk of water. Either of the two diluted acids which I have described act very little on amalgamated zinc. I am inclined to think that the common sulphuric acid, which is generally made from pyrites, and is not so pure as that which is made from sulphur, nor so strong as that which I first used, should not be diluted with more than about once and a half its own bulk of water.

I subsequently tried the effect of sulphuric acid mixed with a solution of common salt, of dilute sulphuric and muriatic acid together, of undiluted muriatic acid, and muriatic acid diluted with a small quantity of water, and found by the galvanometer that a galvanic current of the same power was produced by the same cast-iron cell and zinc plate, whether they were excited by one part of the sulphuric acid which I first used, diluted with two of water, or with one part of the strongest sulphuric acid diluted with three of water, or with the first sulphuric acid mixed with three times its bulk of a strong solution of common salt, or with the strongest acid mixed with  $3\frac{3}{4}$  times its bulk of the same solution, or with sulphuric and muriatic acid together diluted with a volume of water about twice as great as that of the sulphuric acid, or with undiluted muriatic acid, or muriatic acid diluted with a small quantity of water. Cast iron and zinc appear to be equally excited by the seven fluids just described.

Seeing, by the indications of the galvanometer, that the galvanic current produced by a cast-iron cell excited by any of the preceding fluids was exceedingly powerful, I resolved to compare it with the current produced by a nitric acid cell of the same size, and found that nearly the same deflection of the needle was produced by the two currents. I used a cell of the cast-iron nitric acid battery rather than one of Grove's or Bunsen's, because I found it more convenient than either, and not inferior in power.

The two cast-iron cells which I used were of the same size and shape; both were of a rectangular form. They were made for holding porous cells which contained zinc plates 4 inches square; their inside width was about  $\frac{7}{8}$ ths of an inch. For the nitric acid battery their width could not be less: were it less, the porous cells would not fit in the iron cells. The thickness of the zinc plate was about  $\frac{1}{8}$ th of an inch. Therefore the distance between each side of the zinc and cast iron was  $\frac{5}{8}$ ths of an inch. This distance cannot be diminished in the cells of the nitric acid battery; but in the battery excited with any of the above-mentioned fluids, it may be diminished without limit, and thus the galvanic power of the battery may be greatly increased.

When the distance between the zinc and cast iron of a cell excited by one part of sulphuric acid and two of water, or by any of the fluids which have been described, does not exceed the  $\frac{1}{10}$ th of an inch, the voltaic power of the cell is considerably superior to that of a cell of equal size of the nitric acid battery. I prepared a 4-inch cell and zinc plate of the nitric acid battery, and a zinc plate and cast-iron cell of the same size, in which the distance between the zinc and cast iron was about the  $\frac{1}{10}$ th of an inch. The opposite ends of these two cells were connected

with the opposite ends of the coil of the galvanometer, in such a way, that, as soon as the two cells should be filled, the galvanic currents from them would flow simultaneously through the coil in opposite directions. The cell of the nitric acid battery was first filled: it was in the best working order, and produced a steady deflection of  $82^{\circ}$ . The other cell was then filled with one part of sulphuric acid and two of water, and the voltaic currents from the two cells flowed simultaneously through the coil in opposite directions. The current from the cell excited by dilute sulphuric acid destroyed the entire deflection produced by the nitric acid cell, and produced an opposite and steady deflection of  $72^{\circ}$ . On another occasion I made a similar experiment with a nitric acid cell and one of the same size, in which the distance between the zinc and cast iron was about the  $\frac{1}{30}$ th of an inch, and which was excited with one of the fluids already described. In this experiment the sine galvanometer was used, and the compass-box containing the magnetic needle was slid  $21\frac{1}{2}$  inches from the centre of the coil in the direction of its axis. The voltaic current was first sent through the coil from the nitric acid cell, and a deviation of  $38^{\circ}$  was produced. The single fluid cell was then filled, and the galvanic currents flowed simultaneously from the two cells in opposite directions through the coil. The current from the single fluid cell destroyed the entire deviation of  $38^{\circ}$ , and produced a deviation of  $42^{\circ}$  on the opposite side of the magnetic meridian, thus showing that its power was more than twice as great as that of the current produced by the nitric acid cell. The porous cell employed in this experiment was of the best quality; it was perfectly saturated on the outside with nitro-sulphuric acid, and on the inside with dilute sulphuric acid, consisting of one part of very strong and pure sulphuric acid and five of water.

From the experiments and results which have been described, it is evident that a single fluid battery more powerful than any nitric acid battery of the same size, may be made by exciting a suitable arrangement of cast-iron cells or plates and amalgamated zinc with any of the following fluids. First, undiluted muriatic acid, or muriatic acid diluted with a small quantity of water; secondly, muriatic and sulphuric acid together diluted with a quantity of water a little more than twice as great by measure as that of the sulphuric acid; thirdly, sulphuric acid diluted with about twice its bulk of water, or the strongest sulphuric acid made from sulphur diluted with three times its bulk of water; fourthly, sulphuric acid mixed with three times its bulk of a strong solution of common salt, or the strongest sulphuric acid made from sulphur mixed with about  $3\frac{3}{4}$  times its bulk of the same solution. The solution may be made

by dissolving nearly two pounds of salt in each gallon of water.

It will be very useful to dissolve some carbonate or sulphate of soda in each of these exciting fluids, except the last. I recommend the last in preference to any of the others: first, because it is the cheapest of all; secondly, because I think the amalgamated surface of the zinc is better preserved, and kept cleaner by the last than by any of the others; thirdly, because it appears to act less on amalgamated zinc than any of the others.

A very powerful battery may be made by exciting amalgamated zinc, and iron, copper, platina, or a platinized metal with any of these fluids. But they produce the greatest galvanic power when cast iron is the negative element.

Muriatic acid preserves the amalgam on the zinc, but it soon covers it with a dark scum, which I fear would, after some time, diminish the action of the battery. Besides, it acts more than dilute sulphuric acid on amalgamated zinc. One would expect that sulphuric acid mixed with a solution of common salt would have the same effect as muriatic acid on the zinc; but such is not the case.

On account of its powerful galvanic action, a cast-iron battery charged with any of the above-mentioned fluids generates a large quantity of hydrogen, which produces considerable effervescence at the top of the cells. To obviate this inconvenience, I mixed with the exciting fluids various metallic and alkaline salts, hoping that the oxygen set free might combine with the liberated hydrogen, as it does in Daniell's and in the nitric acid batteries; and that some of them which contained the same proportion of oxygen as the nitrates do, might, like them when dissolved in sulphuric acid, give an increase of voltaic power. But my hopes were disappointed. The salts which I used principally were sulphate of iron and copper, acetate of lead, phosphate of soda, chlorate, arseniate and permanganate of potash. The effervescence appeared to be somewhat diminished by acetate of lead and phosphate of soda. Sulphate of copper and arseniate of potash produced a slight increase of voltaic power; but when sulphate of copper was used, copper was soon deposited on the zinc plate. Chlorate of potash, as might be expected, caused a series of explosions in the cell. There was no beneficial result from the use of permanganate of potash or peroxide of lead. On the whole, the effect produced by these substances would not be worth their cost.

In making a cast-iron battery to be excited by any of the fluids which I have described, care must be taken first to have the cast iron and zinc very near to each other; and secondly, to protect the inactive part of the cast iron against the action of the

exciting fluid, and when the battery is intended not for constant work, but for experiments which may be occasionally interrupted, to make provision for taking the metallic plates out of the exciting fluid whenever it may be necessary to suspend the experiments.

First, the greater the proximity of the metallic elements, the more powerful will be the galvanic action, provided the hydrogen escapes with sufficient freedom to allow the exciting fluid to be in continual contact with the metals. If the hydrogen prevents the continual contact of the exciting fluid and metals, there will be a series of voltaic currents instead of one constant uniform current. I have found that when the zinc plate is not more than 4 inches square, the hydrogen does not interfere with the constant action of the battery, even though the distance between the zinc and cast iron should not exceed the  $\frac{1}{30}$ th of an inch. Even in that case a perfectly steady deflection of the magnetic needle is produced by the battery, and therefore the voltaic current must be constant and steady. I have not yet tried plates larger than 4 inches square, but I am inclined to think that plates a foot square or more need not be more than  $\frac{1}{16}$ th of an inch asunder.

Secondly, the part of the cast iron which has little or no effect in producing the voltaic current, must be protected against the action of the exciting fluid. Each of the exciting fluids that have been described acts a good deal on cast iron; and were the inactive part of the iron unprotected, there would be a considerable and useless consumption of cast iron, and of the strength of the exciting fluid. The iron may be sufficiently protected by covering it with vulcanized india-rubber, wood, or any other substance on which the exciting fluid acts but little or not at all.

The cast iron may have the form of cells which hold the exciting fluid, or of plates, each pair being connected together at the top, but separated from each other elsewhere so as to admit a zinc plate between them. The distance between each cast-iron plate and the one connected with it at the top may be a quarter or a fifth of an inch, so that a zinc plate an eighth of an inch thick may be put between the two, and that each side of the zinc will be a sixteenth or twentieth of an inch from the cast iron. The contact between the zinc and cast iron may be prevented by a thin wedge of wood at each corner of the plate. If cast-iron cells be used, they must be an inch or an inch and a half higher than the zinc plates. The width of the part of the cell which contains the zinc plate should not exceed one-fourth of an inch, otherwise the distance between the zinc and cast iron will be too great, and there will be a loss of galvanic power. The part of the cast-iron cell which is near the top or above the zinc plate should be about  $1\frac{1}{2}$  inch wide, in order to hold a sufficient



quantity of the exciting fluid. The fluid contained in the narrow part of the cell would be so small that its exciting power must be exhausted in a few minutes. The annexed figure represents a vertical section of the best form of cast-iron cells. The form is rectangular. The zinc plate is contained in the narrow part of it; the wide part, if it be  $1\frac{1}{2}$  inch high and  $1\frac{1}{2}$  inch wide, will contain as much of the exciting fluid as will be required for a lecture of an hour or two.



If cast-iron plates be used, earthenware or other cells must be employed for holding the exciting fluid, and then the outside of each cast-iron plate is inactive; it is exposed to the action of the fluid and must be protected. Ten or twelve pairs of cast-iron and zinc plates may, like the copper and zinc plates of a Wollaston battery, be fastened to a bar of wood; they may then be let down at once into a Wedgwood trough containing the exciting fluid, and may be taken up whenever it is found necessary or convenient to suspend the action of the battery.

The great galvanic power of cast iron and zinc excited by one part of sulphuric acid and two of water, was first discovered in the beginning of last June. In a few days after the discovery, I arranged in one series 72 cast-iron cells of our nitric acid battery, each containing a 4-inch zinc plate separated from the iron cells by pieces of wood. They were charged with one measure of sulphuric acid and two of water. The galvanic current was sent through a pair of very thick coke points, each about an inch long. The greater part of the coke was raised to a white heat; the result was a most brilliant light, which appeared to me to be equal in illuminating power to any coke light I have ever seen produced by an equal or much larger number of cells of the nitric acid battery. There was a remarkable difference between the appearance of the coke points when ignited by this battery and the nitric acid battery. On this occasion the negative and positive points appeared to be equally ignited and illuminated. With the nitric acid battery, the positive point is much more intensely ignited, and gives out a far greater quantity of light than the negative one.

At least one-third of the power of the battery just described was lost: first, because the distance between the zinc and cast iron was about three-eighths of an inch; secondly, because the cast-iron cells had been used several times in the nitric acid battery, and were coated with nitrate of iron, which greatly diminishes the galvanic power when the cast iron is excited by dilute sulphuric acid. I have found by means of the galvanometer, that the power of a galvanic current produced by a cast-

iron cell excited by one part of sulphuric acid and two of water, whilst it is coated with nitrate of iron, is considerably less than one-half that of the current produced by the same cell excited by the same fluid after the nitrate of iron has been removed. It is necessary to clean the cast-iron cells or plates before they are used, by leaving them for some time in dilute sulphuric or muriatic acid.

In the beginning of September I showed in the College a battery of 182 cast-iron cells excited by one part of sulphuric acid and three of a strong solution of common salt. The experiments made with this battery consisted in the ignition of coke-points and of various metals. A scientific friend who came from Dublin to be present at the experiments, who had great experience of the working of nitric acid batteries, and witnessed in the College the action of 275 4-inch cells of the nitric acid battery on one occasion, and of 200 cells on another, stated that the 182 cells of the single fluid battery appeared to him to be superior in power to the 275 or the 200 nitric acid cells. On this occasion I requested several persons to compare the degree of ignition and illumination of the negative and positive coke-points. In order to be able to look steadily at the ignited points, they used deeply-coloured glasses. They all stated that they could not perceive any difference between the degree of ignition of one point and the other. When the connexion with the battery was broken, both points appeared equally heated, and equally or nearly equally consumed.

It has occurred to me, that the superior heat and light of the positive coke-point ignited by a nitric acid battery arise principally from the action of the exciting fluid on the positive metal; and that, because in the cast-iron battery excited by any of the fluids I have used, the exciting fluid acts a good deal on the negative as well as on the positive metal, the negative coke-point should be ignited and illuminated nearly as much as the positive one. Perhaps also the galvanic power of the single fluid cast-iron battery is proportional, not to the action on the zinc alone, but to the sum of the actions on the cast iron and zinc. If this be so, the expense arising from the consumption of metal in producing a given galvanic power will be less in the single fluid battery than in the nitric acid battery; for zinc is more than twice as dear as cast iron.

In using for the electric light a cast-iron battery of 60 cells, excited by about one part of sulphuric acid, one of muriatic, and two of water, I made an experiment which proves the great intensity of this battery. After the battery had been at work for about three-quarters of an hour, I emptied two of the cells at one end. Whilst they were empty, the ends of the battery

were connected with a pair of coke-points. The voltaic current passed through the two empty cells, ignited the coke-points, and produced a brilliant light. The voltaic current had no other means of passing to the coke than through the damp pieces of wood which separated the zinc plates from the cast-iron cells, or through the wooden frames in which the two empty cells were placed.

The Rev. Dr. Robinson and Mr. Bargin, to whom I mentioned the various fluids which I found to excite cast iron and zinc so powerfully, have lately tried a cast-iron battery of 48 cells, charged with one part of strong sulphuric acid and three of a pretty strong solution of common salt. The distance between the zinc and iron was nearly  $\frac{1}{8}$ th of an inch. The quantity of fluid used in filling the battery was, I think, a gallon and a half, in which there were about three pints, or about seven or eight pounds of sulphuric acid. With this battery they had a brilliant coke light sufficiently steady to enable them to make observations on the light with the prism and polariscope; also lights produced by the ignition of various metallic points, on which lights similar observations were made. Various other experiments were made; they commenced at one o'clock, and were not given up till nine. There were of course several interruptions, during each of which the fluid was, by a very ingenious contrivance, poured off the metallic plates. During the eight hours the experiments lasted, the battery was in constant action at least three and a half or four hours. At the end of the experiments the two metals were quite clean, and there was no sensible diminution of voltaic power. Such is the substance of the account which I received of the action of this battery. The results of this trial of the battery show that a cast-iron battery, excited by one part of sulphuric acid and three of a solution of common salt, is very powerful, extremely constant in its action, and most oeconomic in use. The sulphuric acid employed was made, not from pyrites, but from sulphur. I purchase, at the vitriol works of Messrs. Boyd, Belfast, sulphuric acid made in the same way, and of the same strength, at the rate of 8s. 6d. per cwt., or for less than one penny per pound. Hence at the price at which sulphuric acid is sold by Messrs. Boyd, the cost of the exciting fluid for eight hours' experiments scarcely exceeded eight pence. I have also got from Messrs. Boyd for 3s. 6d. per cwt., weak but pure muriatic acid of the specific gravity 1.130, which I have found to answer for the cast-iron battery better without being diluted, than strong muriatic acid diluted with a small quantity of water.

The cast-iron battery excited by any of the fluids which I have mentioned, is superior in power to any of the nitric acid

batteries; it is less expensive in construction, since it does not require porous cells; it is far more economical in use; it is free from the trouble, annoyance, and all the very serious disadvantages which arise from the use of porous cells and nitric acid; it is extremely constant in its action, and may by a simple contrivance be made to act with almost perfect constancy till the zinc plates are worn out. The metals will remain clean for any length of time the battery may be in action: no hydrogen adheres to the cast iron. The only cause, then, of the diminution of galvanic power is the exhaustion of the exciting fluid. By siphon tubes, one of which reaches near the bottom of each cell, passes through a hole in the side of the cell a little above the upper edge of the zinc plate, and projects about an inch from the outside of the cell, and dropping tubes connected with the vessel which contains the exciting fluid, one of the tubes being set over each cell, a supply of fresh exciting fluid may be given to each cell, and the exhausted fluid may be made to pass out through the siphon tube. The dropping tubes should project from the bottom of the vessel which holds the fluid, and should be at such distances from each other that one will be over each cell of the battery. The vessel should be air-tight, and a stopcock fitted to the top of it, so that the communication between the inside of the vessel and the external air may be cut off by closing the stopcock, and may be renewed by opening the stopcock. When the communication between the inside of the vessel and the external air is cut off, the fluid ceases to flow into the cells from the dropping tubes: as soon as the communication is renewed, the fluid begins again to flow into the cells from the dropping tubes, and the exhausted fluid, as well as the sulphate of iron and zinc, at the bottom begins to flow out through the siphon tubes. By suitable clockwork, the stopcock may be opened and closed as may be required, in order to give the necessary supply, and no more, of the exciting fluid to each cell.

I have not yet tried a large series in which the zinc and cast iron were very near to each other. I am preparing for the College a battery of about 300 pairs of 4-inch cast-iron and zinc plates separated from each other about  $\frac{1}{16}$ th of an inch, and 20 pairs of plates, each about a foot square. The former are intended for effects which require considerable intensity, such as heat and light; and the latter for the decomposition of water and the lime light.

Having found that the galvanic power of cast iron and zinc excited by one part of sulphuric acid and two of water, was increased by dissolving sulphate of copper in the acid solution, it occurred to me to try the effect of cast iron instead of the copper used in Daniell's battery. I put into a saturated solu-

tion of sulphate of copper, a copper plate 4 inches square, and a zinc plate into a porous cell filled with dilute acid. When the porous cell was put into the solution of sulphate of copper, and the two metals connected with the opposite ends of the coil of the galvanometer, a steady deflection of  $25^\circ$  was produced. The copper plate was removed and a cast-iron one of the same size was put in its place. The cast iron and zinc plate produced a deflection of  $30^\circ$ . Hence by substituting cast iron for the copper used in Daniell's battery, a more powerful constant battery may be made. The cast iron may be in the form of cells, which will act as the negative metal, and will at the same time hold the solution of sulphate of copper. Thus the expense of glazed cells, which are so easily broken, may be saved.

I have also tried cast iron excited with a solution of sulphate of iron and zinc excited with dilute acid, both being separated by a porous cell. A 4-inch cast-iron plate gave a deflection of about  $21^\circ$ , whilst a Daniell's of the same size produced a deflection of  $25^\circ$ . Hence a battery nearly equal in power to Daniell's may be made by exciting cast iron with sulphate of iron and zinc with dilute acid. The cost of sulphate of iron is only about the eighth of that of sulphate of copper.

Maynooth College,  
March 1, 1855.

P.S. I have lately tried a battery of 70 4-inch cells, in which the zinc and cast iron were very near to each other. The battery was charged with one part of sulphuric acid and three of a solution of common salt. The experiments consisted in the ignition of metals and coke-points. A brass wire  $\frac{1}{4}$  of an inch diameter, and a piece of zinc about  $\frac{3}{8}$  by  $\frac{1}{4}$  of an inch thick, were burnt away so rapidly that the lecture room, which is about 40 feet square and 18 high, was soon filled with the smoke produced by the combustion of the metals.

For the coke light, I used Deleuil's apparatus; the light was sufficiently intense to enable a person to read the smallest print at the most distant part of the room whilst the window-shutters were closed. During the entire time of the experiments, which lasted nearly an hour and a half, the action of the battery was not suspended for more than about five minutes. At the end of the experiments, the light appeared to be as brilliant as at the commencement. Had there been time to continue the experiments, the power of the battery, would, I think, have been sufficient to produce a strong coke light for another hour or half-hour. The cost of the acid used in charging the 70 cells was about 9*d.* or 10*d.*

The voltaic current by which the metals and coke points were

ignited was constantly passing through the coil of the galvanometer. I forgot to observe the deflection produced by the voltaic current during the combustion of the metals; but during the ignition of the coke-points, the deflection was frequently observed: the average deflection did not exceed  $40^\circ$ . I have since found that the current from a single pair of plates, each an inch and a half square, produces a deflection of  $58^\circ$ , which requires a current about twice as powerful as one which would cause a deflection of  $40^\circ$ . Hence not more than about the  $\frac{1}{16}$ th of the power of the 70 4-inch cells was employed in producing the coke light. Fifteen-sixteenths of their power was inactive, whilst the exciting fluid was acting on the metals, and wasting its own strength. Hence it strikes me, that for the electric light which is now used for practical purposes, a battery consisting of 80 or 90 cells, each about an inch and a half or two inches square, would be the most æconomical. I purpose to get a battery of 100 pairs of plates, each two inches or an inch and a half square, in order to try its power of producing the electric light.

The greatest length of the flame between the coke-points appeared to me to be less than it would be with a nitric acid battery of 70 4-inch cells. Hence I infer that the intensity of the single fluid battery is less than that of the nitric acid battery, although the quantity of electricity is much greater. The state of some of the cells after the experiments, excited an apprehension that the distance of  $\frac{1}{16}$ th of an inch between the zinc and iron may not be sufficient for the free descent of the sulphate of zinc, though it may permit the hydrogen to escape with perfect freedom. However, I am inclined to ascribe the condition of the cells, which were new, to the sand which remained attached to them after they were taken out of the mould.

Maynooth College,  
March 15, 1855.

XLI. *Notes on Mineralogy.*—No. I. *On the Chemical Composition and Optical Properties of the Mica of the Dublin, Wicklow, and Carlow Granites.* By the Rev. SAMUEL HAUGHTON, M.A., Professor of Geology in the University of Dublin\*.

THE minerals included in the mica family may be divided into various groups, founded on their chemical composition and optical properties.

Chemically considered, they are divided by Rammelsberg and others into three families:—

\* Communicated by the Author.

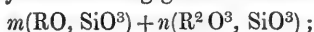
1. Potash mica.
2. Lithia mica.
3. Magnesia mica.

Optically considered, they are divided into three groups also :—

1. Biaxial micas, the angle between the optic axes being from 44° to 75°.
2. Biaxial micas, with angle between optic axes from 5° to 20°.
3. Biaxial and uniaxial micas, with angle between optic axes from 5° to 0°.

The first optical group includes the potash and lithia micas of the chemical division, while the third chemical group, or magnesia mica, is divided between the second and third optical groups, the second group being usually designated phlogopite, and the third Biotite.

The potash and lithia micas are considered by Rammelsberg as represented by the following general formula,—



in which formula, in the potash micas,—

$$m=1,$$

$$n=2, 3, \text{ or } 4;$$

the last two cases,  $n=3$  and  $n=4$ , being the muscovite of mineralogists; and the first,  $n=2$ , or



being known as margarodite.

The micas of the Dublin, Wicklow, and Carlow granites, analysed by me, belong to the margarodite genus, and contain two atoms of water, corresponding to  $k=2$ .

The following are the analyses of three micas selected from three distant localities of the granite chain of the south-east of Ireland.

No. 1. Mica from the Three Rock Mountain, county Dublin; gray, transparent, containing specks or flakes of a bronze-coloured or black mica.

	Per cent.	Atomic quotients.		
Silica . . .	43·47	0·959	2·863	3
Alumina . . .	31·42	0·611	} 0·670	2·000
Peroxide of iron	4·79	0·059		
Lime . . .	1·38	0·049	} 0·379	1·131
Magnesia . . .	1·13	0·056		
Potash . . .	10·71	0·228		
Soda . . .	1·44	0·046		
Loss by ignition	5·43	0·603	1·800	2
	<u>99·77</u>			

No. 2. Mica from Glendalough Valley, county Wicklow; gray, silvery, transparent, with specific gravity = 2.793.

	Per cent.	Atomic quotients.		
Silica . . .	44.71	0.987	2.973	3
Alumina . . .	31.13	0.606	} 0.664	2.000
Peroxide of iron	4.69	0.058		
Lime . . .	1.09	0.035	} 0.332	1.000
Magnesia . . .	0.90	0.045		
Potash . . .	9.91	0.211		
Soda . . .	1.27	0.041	} 2.083	2
Loss by ignition	6.22	0.691		
	<u>99.92</u>			

No. 3. Mica from Mount Leinster, county Carlow; a gray, silvery, transparent mica.

	Per cent.	Atomic quotients.		
Silica . . .	44.64	0.970	2.913	3
Alumina . . .	30.18	0.587	} 0.666	2.000
Peroxide of iron	6.35	0.079		
Lime . . .	0.00	0.000	} 0.298	0.900
Magnesia . . .	0.72	0.036		
Potash . . .	12.40	0.262		
Soda . . .	trace		} 1.774	2
Loss by ignition	5.32	0.591		
	<u>99.61</u>			

If we take the mean of these analyses, we find,—

*Average Mica of Dublin, Wicklow, and Carlow Granite.*

	Per cent.	Atomic quotients.		
Silica . . .	44.27	0.962		3
Alumina . . .	30.91	0.601	} 0.667	2
Peroxide of iron	5.27	0.066		
Lime . . .	0.82	0.029	} 0.338	1
Magnesia . . .	0.92	0.046		
Potash . . .	11.01	0.234		
Soda . . .	0.90	0.029	} 2	2
Loss by ignition	5.66	0.629		

There can be little doubt, from the foregoing analyses, that the transparent, gray mica of the district under consideration is a margarodite, having the rational formula



The mica which deviates most from this formula is the mica



of the Three Rock Mountain, and this deviation may possibly be due to the presence of grains of black mica, which also occurs in the mass of the granite, and of which I was unable to obtain a sufficient quantity for chemical or optical examination. The quantity of protoxides in the Three Rock mica is somewhat in excess of that required by the formula.

The angles between the optic axes of these micas were carefully determined, and found to be as follows:—

*Angles between Optic Axes of Micas.*

1.	Three Rock mica . . . .	53	8
2.	Glendalough mica . . . .	70	4
3.	Mount Leinster mica . . . .	72	18
4.	Lough Dan mica . . . .	70	0
5.	Glenmalure mica . . . .	67	11

I have added to the determination of angles of the micas analysed, the optic angles of two other specimens of gray, transparent mica from Lough Dan and Glenmalure, county Wicklow.

The four micas which were free from any intermixture of black mica have a high angle, while the angle between the optic axes of the mica from the Three Rock Mountain, which contained flakes of black mica, is nearly 20° less than that of the pure transparent gray micas.

The uniformity of the preceding analyses is sufficient to show that margarodite is entitled to be considered as a distinct species of hydrated mica, and that it is not merely an altered form of muscovite, an opinion advocated by M. Dana in the last edition of his 'System of Mineralogy.'

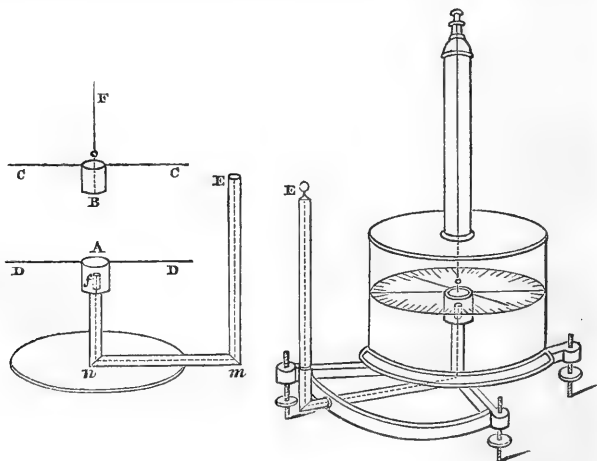
Crystallographically considered, the micas just described are trimetric, and occur in flat, right rhombic prisms, with angles of 6° and 120°; or in hexagonal tabular prisms, formed by the replacement of the acute angles of the rhombic prisms, all the angles of the hexagon being 120°.

The plane of the optic axes is perpendicular to the flat plane of cleavage, and bisects the acute angles of the rhomb, or intersects perpendicularly two of the sides of the hexagon. I have not found among the micas of the granite of the south-east of Ireland, any instance of the optic axes lying in the shorter diagonal of the rhomb.

XLII. *On a new Electroscope.* By M. MELLONI\*.

WHEN a conductor in a natural state is brought near an electrified conductor, it renders imperceptible a portion of this electrical state, and by communicating its positive tension to the masked fluid in proportion as the perceptible fluid quits it by dispersion, prolongs the duration of the electric charge. On the other hand, it is well known that this effect is derived from the opposite electricity, developed by induction, in the nearest part of the induced body, and that the electricity, homologous with that of the inductive body, appears in the most distant portions, where it is distributed in proportions increasing with the diminution of the radius of curvature.

A combination of these three data, led me to conceive the possibility of constructing an electroscope of extreme sensibility, and capable of remaining electrified in either direction for a much longer time than any known apparatus of this description. The result has completely answered my expectations, and as I am convinced that this new instrument will become exceedingly useful in electrical researches of several kinds, I shall endeavour to describe it with all necessary details.



A is a little metallic cup, furnished with two long wire appendages, D, which are soldered to opposite points of its upper margin; it communicates, by a conductor passing through a

\* From the *Comptes Rendus*, Dec. 11, 1854, p. 1113.

glass tube, with a ball or disc of metal E. B is a second reversed metallic cup, a little smaller and much lighter than A; it is attached below a wire or very thin metallic lever CC, which is suspended by its middle with a silk thread F. The axes of the two cups are brought to the same vertical line, and the suspending thread, placed at such a level that the second cup is entirely contained within the first, where it must be able to turn freely round its point of suspension, without touching the walls of the fixed cup A\*.

When the apparatus is thus arranged, it is clear that if the conductor E receives an electrical charge, it will be propagated by transmission to the outer cup A, and that when there it will act by induction upon the inner cup B. If we suppose, for example, that the electricity communicated is positive, this electrical force distributed in A will disturb the equilibrium of the natural fluid of B; will repel the positive fluid and attract the negative, which will react in its turn upon the free fluid of A, mask a certain quantity of it, and abandon the rest to the known laws of electrical distribution on insulated conductors; so that the intensity of the action will depend on the curvature of the surfaces, and will be weaker on the walls of the cup than upon its appendages. The outer cup A of the charged apparatus will consequently contain a certain proportion of masked positive electricity (that is to say accumulated without tension and without mobility), and its appendages DD will possess a free electricity of the same kind, which will increase in energy in proportion as we approach the extremities.

As regards the inner mass B and its lever CC, there will be masked negative electricity on its central portion opposite the cup A, and free positive electricity on the remainder of the moveable system, that is to say, on the flat top of the reversed cup and on the lever above it. Now the latter description of electricity will evidently be far more energetic at the extremities of the lever than at its middle portion or on the top of the cup,—1st, because these extremities constitute the most distant points from the inductive action; and 2nd, because the curvature here is greater than anywhere else.

Thus the lever CC, possessing the same kind of electricity as the appendages DD, and being from its concentric position subject to the mutual action of their repulsive forces, will be energetically repelled, unless it be exactly in the same azimuth with them; after a few oscillations it will stop at a certain angle

\* In the electroscope which has been constructed, there is a peculiarity not mentioned in the description,—a small metallic cylinder *f* rises from the middle of the fixed cup A; when the moveable cup is well hung, this cylinder occupies its interior without touching it.

of deviation. The electrical charge, communicated to the fixed system EADD, will then begin to diminish.

This diminution, however, takes place much more slowly than in the ordinary electroscopes, in consequence of the masked electricity which is gradually disengaged from the central part, and which replaces on the cup A its appendages DD, the wire of communication and the disc E, a portion of the free electricity lost by dispersion. The double or inductive electrization of the moveable system BCC will exactly follow the successive phases of the simple electrization of the fixed system, its two principles will be re-compounded gradually in proportion to the loss of the charge, and after a certain time the whole will return to the natural condition. All this is independent of the method employed for charging the conductor E, and will consequently apply equally to cases of direct charge by contact, and to cases of indirect or opposite charge obtained by means of induction.

To resume, the moveable part of the instrument is always electrified by induction and never by communication; the difference in the form of the centres and extremities of the fixed and moveable portions renders the distribution of the motive forces the most advantageous that can possibly be for the rotation of the index, and the inductive action of the central surfaces masking a portion of electricity which is afterwards set free in proportion to the loss undergone, prolongs the duration of the charge.

The small size of the pieces constituting the essential part of the apparatus accelerating the loss of electricity by diffusion in the surrounding medium, it is necessary that they should be enclosed in a case, in which the air must be kept very dry by means of some substance which possesses a great attraction for moisture. A dry state of the internal air is especially necessary to prevent variation in the torsion of the silk thread which supports the reversed cup, so as to enable the index CC to return constantly to the same azimuth, when the appendages DD have lost their electrical charge.

The case must be of a convenient form, and as the observations to be made require the knowledge of the angles of deviation formed by two rods superposed without contact, and kept at a certain distance from a dial plate placed below them, the most favourable arrangement is evidently to suspend the free extremity of the silk thread from the interior of the apex of a vertical tube, opening in the centre of a horizontal glass disc, the circumference of which rests upon a cylindrical metallic receiver, but little wider than the moveable lever and the subjacent appendages of the fixed cup. The upper edge of this receiver must be flattened and lined with leather, to intercept the communication between

the air within and the air outside the apparatus, when the glass is fixed by the pressure of little screws upon the ring of metal which keeps it in its place.

The divided circle which measures the angles formed by the repulsion of the index, is to be perforated in the centre to give a free passage to the fixed cup A. The conducting wire is contained in a tube of varnished glass, filled with mastic; it is bent twice at a right angle in the same vertical plane, so that it resumes its original direction, and thus terminates at the outer piece of metal intended for the introduction of the electrical charge.

Into the space below the dial plate one or two reservoirs filled with chloride of calcium are passed, by means of openings in the bottom of the metal receiver, which are afterwards closed by screws. The receiver is supported on a tripod furnished with screws, which serve to place the suspended portion of the apparatus in equilibrium.

Lastly, the necessity of moving the apparatus from place to place, and of giving a certain initial angle of deviation to the moveable lever, renders it necessary that the upper extremity of the tube in which the silk thread is enclosed should be capable of motion in two directions. The first is a simple vertical movement, to allow the inner cup to rest on the flat bottom of the outer cup, and to raise it again to the proper height; the second a horizontal rotatory movement, to cause a slight deviation of the index at the commencement of each series of experiments. The rotatory movement will be communicated to the moveable system by means of the torsion of the silk.

As the same force is the cause of the resistance which stops the electrized lever at a greater or less angular distance, its strength must be proportioned to the weight of the moving mass. On this account, instead of a single thread from the cocoon, it is useful to employ several of them united, not by twisting, but simply glued together by the action of their own gummy nature and hot water.

If the force of the torsion of the thread be found too weak, and it be desired to abridge the time required for the observations, it is only necessary to place parallel to the index on the moveable cup a little magnetized needle, as is done with the indicator of Peltier's electroscope, and to arrange the appendages of the fixed cup in a direction forming an angle of 4 or 5 degrees with the magnetic meridian. We must not forget, however, that in this case we lose in exactitude what we gain in rapidity of observation. The assistance of the magnetized needle may nevertheless be useful under some circumstances, and especially when the great humidity of the air rapidly removes the electricity to the exterior of the instrument.

**XLIII.** *On some Extensions of Quaternions.* By Sir WILLIAM ROWAN HAMILTON, LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the French Institute, Hon. or Corr. Member of several other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from p. 51.]

Section V.

[30.] **I**N applying to associative *quines* the general theory of the Third Section\*, we may (as has been seen) omit each of the signs  $\Sigma^h$  as unnecessary, the index  $h$  receiving only one value in the sum thereby indicated; and may suppress each sum  $\Sigma^h$  as vanishing. In this manner the type IV., or the formula (151), becomes,

$$\text{IV.} \quad (egf)(eff + egg) = geh . ehf; \dots (191)$$

while the equation (127), already derived as a sub-type from II., gives, by interchanging  $e$  and  $f$ ,

$$(egf)(fee + fgg) = (geh)(hee + hgg). \dots (192)$$

Multiplying the latter of these two equations by  $eff + egg$ , and the former by  $fee + fgg$ , and subtracting, we eliminate the symbol  $(egf)$ , and find that

$$(geh) \{ (ehf)(fee + fgg) - (eff + egg)(hee + hgg) \} = 0; \dots (193)$$

and a similar elimination of  $(geh)$  gives the equation,

$$(egf) \{ (ehf)(fee + fgg) - (eff + egg)(hee + hgg) \} = 0. \dots (194)$$

And because  $(geh) = -(egh)$ , by (34), we may make any separate or combined interchanges, of  $e$  with  $g$ , and of  $f$  with  $h$ , and so vary the expression within the  $\{ \}$ , without introducing any new factor, distinct from  $(egf)$  and  $(egh)$ , outside them. If, then, for any particular arrangement of the four unequal indices,  $e, f, g, h$ , as chosen from among the four numbers 1, 2, 3, 4, the *two* following conditions are not *both* satisfied,

$$(egf) = 0, \quad (egh) = 0, \dots (195)$$

we must have, for that arrangement of the indices, a system of *four* other equations, whereof one is

$$\text{VI.} \quad (ehf)(fee + fgg) = (eff + egg)(hee + hgg), \dots (196)$$

while the three others are formed from it, by the interchanges just now mentioned. And if we further suppose that the two sums,  $fee + fgg$  and  $hee + hgg$ , are for the same arrangement dif-

\* Comprising paragraphs [17.] to [25.], and published in the Phil. Mag. for October, 1854.

ferent from zero, and write for abridgment, as a definition,

$$(ehf)_0 = (fee + fgg)^{-1}(eff + egg)(hee + hgg), \quad (197)$$

the four equations furnished by the formula (196), which may be regarded as a *sixth type for quines*, may be concisely expressed as follows :

$$\left. \begin{aligned} (ehf) &= (ehf)_0, & (ghf) &= (ghf)_0; \\ (efh) &= (efh)_0, & (gfh) &= (gfh)_0. \end{aligned} \right\} \dots (198)$$

With the notations  $l_1 \dots u_3$ , for the symbols  $(efg)$ ,  $(eff)$ , we find thus that unless  $l_1$  and  $p_1$  both vanish, we must have the four equations,

$$\left. \begin{aligned} t_2(n_1 - m_1) &= (m_2 - n_2)(r_2 + r_3); & s_3(n_1 - m_1) &= (m_3 - n_3)(r_2 + r_3); \\ p_3(r_2 + r_3) &= (u_2 + n_2)(n_1 - m_1); & p_2(r_2 + r_3) &= (m_3 - u_3)(n_1 - m_1); \end{aligned} \right\} (199)$$

and that unless  $s_1 = 0$ ,  $t_1 = 0$ , then

$$\left. \begin{aligned} -l_2(u_2 - m_2) &= (n_1 + u_1)(n_3 + u_3); & t_3(u_2 - m_2) &= (r_1 + r_2)(n_3 + u_3); \\ l_3(n_3 + u_3) &= (u_1 - m_1)(u_2 - m_2); & s_2(n_3 + u_3) &= (r_1 + r_3)(u_2 - m_2). \end{aligned} \right\} (200)$$

[31.] Supposing then that no one of the twelve symbols  $(efg)$  vanishes, and that each of the twelve sums  $eff + egg$  is also different from zero, the various arrangements of the four indices  $efgh$  give us a system of twenty-four equations, included in the new type VI., or in any one of the four formulæ (198); which equations may, by (34), be arranged in twelve pairs, as follows :

$$(ehf) = (ehf)_0 = -(hef)_0 \dots (201)$$

It might seem that *twelve* equations between the twelve symbols of the form  $(eff)$  should thus arise, by the comparison of two expressions for each of the twelve symbols of the form  $(efg)$ ; but if we write for abridgement

$$[g] = (fee + fgg)(fhh + fgg)\{(ehf)_0 + (hef)_0\}, \quad (202)$$

and observe that by the definition (197) of the symbol  $(ehf)_0$ , we have then

$$\begin{aligned} [g] &= (eff + egg)(fhh + fgg)(hee + hgg) \\ &+ (hff + hgg)(fee + fgg)(ehh + egg), \quad (203) \end{aligned}$$

we shall see that this quantity  $[g]$  is independent of the arrangement of the three indices  $e, f, h$ ; and therefore that the twelve equations between the twelve symbols  $(eff)$ , obtained through (201), reduce themselves to the *four* following relations,

$$[e] = 0, \quad [f] = 0, \quad [g] = 0, \quad [h] = 0; \quad (204)$$

which are not even all distinct among themselves, since any *three* of them include the fourth. An easy combination of the two first or of the two last of these four relations (204) conducts

to this other formula, which is equivalent to three distinct equations :

$$\begin{aligned} & (eff + ehh)(fee + fgg)(ghh + gee)(hgg + hff) = \\ & (eff + egg)(fee + fhh)(ghh + gff)(hgg + hee); \end{aligned} \quad (205)$$

and which may also be thus written,

$$(hef)_0(egf)_0 = (ehf)_0(gef)_0. \quad (206)$$

With the notations  $l_1 \dots u_3$ , the twenty-four equations (201) are sufficiently represented by the formulæ (199) and (200), if cyclical permutation of the indices be employed; the four equations (204) take the forms,

$$\left. \begin{aligned} (r_1 + r_2)(n_3 + u_3)(m_2 - n_2) &= (r_1 + r_3)(m_2 - u_2)(m_3 - n_3), \\ (n_1 + u_1)(n_2 + u_2)(n_3 + u_3) &= (m_1 - u_1)(m_2 - u_2)(m_3 - u_3); \end{aligned} \right\} (207)$$

whereof the equation on the second line may be obtained from the product of the three represented by the first line: and the three equations (205) or (206) are included in the following, which is evidently a consequence of (207),

$$(r_1 + r_3)(n_1 + u_1)(n_2 + u_2)(n_3 - m_3) = (r_1 + r_2)(u_1 - m_1)(n_2 - m_2)(u_3 - m_3). \quad (208)$$

[32.] As regards the quotients and products of the symbols ( $efg$ ), which we shall continue to write occasionally without parentheses, we have by type VI., or by (197) (198),

$$\frac{ehf}{ghf} = \frac{eff + egg}{gff + gee}; \quad (209)$$

$$ehf \cdot efh = (eff + egg)(ehh + egg); \quad (210)$$

$$ehf \cdot gfh = (eff + egg)(ghh + gee); \quad (211)$$

eliminating ( $ehf$ ) between the two last of which three equations, we obtain a relation of the same form as the first. Interchanging  $g$  and  $h$  in (210), and subtracting, we find that

$$I. \quad ehf \cdot efh - egf \cdot efg = (egg)^2 - (chh)^2; \quad (212)$$

but this is precisely what the type I., or the formula (113), becomes for quines, when we cyclically advance the four indices in the order  $fegh$ ; the conditions (117) (118) of that *first type* will therefore be satisfied, if we satisfy those of the *sixth*. Had we divided instead of subtracting, we should have found

$$\frac{ehf \cdot efh}{egf \cdot efg} = \frac{eff + egg}{eff + ehh}. \quad (213)$$

To interchange  $f, g$ , and divide, would only lead by (210) to another equation of this last form; but the same operations performed on (211) conduct to the equation

$$\frac{ehf}{heg} = \frac{ghh + gee}{fhh + fee}; \quad (214)$$



which, when we interchange  $g$  and  $h$ , reproduces the formula (192); and shows thereby that the sub-type (127), included under type II., is satisfied by our new type VI., which indeed it had assisted to discover. The same equation (192) may also be derived from the formula (205), by dividing each member of that formula by  $(fee + fhh)(hff + hgg)$ , and attending to the expressions given by type VI., for  $(egf)$  and  $(geh)$  respectively. To interchange  $e, h$ , in (211), and divide, would only conduct to another equation of the same form as (214). Permuting cyclically the three indices  $e, f, g$  in (209), and multiplying together the two equations so obtained therefrom, the product gives

$$\frac{fhg \cdot ghe}{ehg \cdot fhe} = \frac{gff + gee}{eff + egg}; \dots \dots (215)$$

and if we multiply this equation by (209) itself, we find that

$$ehf \cdot fhg \cdot ghe = ghf \cdot ehg \cdot fhe. \dots \dots (216)$$

In fact if we operate thus on the expression (197) for  $(ehf)_0$ , or for its equal  $(ehf)$ , or on the formula (196), we are led to this new equation,

$$ehf \cdot fhg \cdot ghe = (hee + hgg)(hff + hee)(hgg + hff), \dots (217)$$

of which the second member does not alter, when we interchange any two of the three indices  $e, f, g$ . Another multiplication of three equations of the form (209), with the cycle  $egh$ , conducts to the equation  $[f] = 0$  of (204). Interchanging  $e, h$  in (210), and substituting the value so obtained for the product of the two extreme factors of the second member of (217), we find this other expression,

$$ehf \cdot fhg \cdot ghe = hef \cdot hfe \cdot (hee + hff); \dots (218)$$

which is still seen to remain unaltered, by an interchange of  $e$  and  $f$ . Interchanging  $f, g$ , and dividing, we obtain by (216) an equation of the same form as (213); and if we divide each member of (218) by  $(hef)$ , we are conducted to the formula

$$IV. \dots fhg \cdot hge = hfe \cdot (hee + hff), \dots \dots (219)$$

which is of the same form as the equation (191), or as the type IV., and may be changed thereto by cyclical permutation of the four indices  $efgh$ . The same relation (219) may also be derived more directly from type VI., by substitutions of the values (198); for it will be found that the definition (197) gives this identity,

$$(fhg)_0(ghe)_0 = (hee + hff)(fhe)_0. \dots \dots (220)$$

The conditions of type IV., like those of type I., and of the sub-type (127) of II., are therefore all included in those of the new type VI.; which gives also in various ways this other formula

respecting products of four symbols of the form (*efg*),

$$egh . flg . gfe . hef = ehg . fgh . gef . hfe : \dots (221)$$

indeed it will be found that the members of this last equation, taken in their order, are respectively equal by (196) to the members of the equation (205).

With the notations  $l_1 \dots u_3$ , supposing that none of the twelve constants *lpst* vanish, and that the twelve combinations of the forms  $n_1 - m_1, n_1 + u_1, u_1 - m_1, r_1 + r_2$ , are in like manner different from zero, we find thus, or from the equations (199), (200), combined with their consequences (207), the following among other relations, in which cyclical permutation of the indices is still allowed :

$$\left. \begin{aligned} \frac{l_1}{t_2} &= -\frac{n_3 + u_3}{r_1 + r_3}, & \frac{t_2}{s_3} &= \frac{m_2 - n_2}{m_3 - n_3}, & \frac{s_3}{l_1} &= \frac{r_1 + r_2}{u_2 - m_2}, \\ \frac{p_1}{p_2} &= \frac{m_2 - u_2}{n_1 + u_1}, & \frac{l_1}{p_1} &= \frac{r_2 + r_3}{n_1 - m_1}, & \frac{s_1}{t_1} &= \frac{n_3 + u_3}{m_2 - u_2} \end{aligned} \right\} (222)$$

$$\left. \begin{aligned} l_2 l_3 &= (n_1 + u_1)(m_1 - u_1), & s_2 t_3 &= (r_1 + r_2)(r_1 + r_3), \\ p_1 s_2 &= (m_2 - n_2)(m_2 - u_2), & p_1 t_3 &= (m_3 - n_3)(n_3 + u_3), \\ -l_2 s_2 &= (n_1 + u_1)(r_1 + r_3), & l_3 t_3 &= (u_1 - m_1)(r_1 + r_2), \\ p_1 s_1 &= (m_1 - n_1)(n_3 + u_3), & p_1 t_1 &= (m_1 - n_1)(m_2 - u_2). \end{aligned} \right\} (223)$$

The conditions (152), (153), of the *fourth type*, are satisfied; and we have these other products, of which some have occurred already, in (176), (177), in connexion with the particular systems (A) and (B) of quines :

$$\left. \begin{aligned} s_1 s_2 s_3 &= t_1 t_2 t_3 = -(r_1 + r_2)(r_2 + r_3)(r_3 + r_1); \\ s_1 p_2 l_3 &= t_1 l_2 p_3 = (n_1 + u_1)(m_1 - u_1)(m_1 - n_1); \\ s_2 s_3 l_2 p_3 &= t_2 t_3 p_2 l_3; \end{aligned} \right\} \dots (224)$$

where the two members of the equation on the last line are easily proved by (223) to be respectively equal to those of (208).

[33.] As yet we have only partially satisfied the conditions of type II., or of the formula (123), which may for quines be written thus :

$$\text{II.} \dots fgh . hff = fee . gee - fgg . gff + feh . ghe. \dots (225)$$

Substituting for the last product in this formula its value given by (211), namely

$$feh . ghe = (fgg + fhh)(gee + gff), \dots (226)$$

and writing for abridgement

$$v_f = fee + fgg + fhh, \dots (227)$$

we are in this way led to establish the following *seventh type*\* for quines :

$$\text{VII.} \quad fgh \cdot hff = v_f \cdot gee + fhh \cdot gff. \quad \dots \quad (228)$$

Or since, by the sixth type, we have already

$$\text{VI.} \quad fgh \cdot (hee + hff) = (fee + fhh)(gee + gff), \quad (229)$$

it is only necessary, for the purpose of satisfying the conditions of type II., or the equations (143) (145), to suppose besides that

$$\text{III.} \quad fgh \cdot hee = fee \cdot gff - fgg \cdot gee; \quad \dots \quad (230)$$

such being the expression which remains, when we subtract (228) from (229). But this last equation (230) is precisely what the type III., or the formula (147), becomes for quines; it reproduces therefore the equations (148), with a correction elsewhere noticed (namely the substitution of  $s_2n_3$  for  $s_2r_3$ ): and conversely, if we retain that old type III., it will not be *necessary*, although it may be convenient, to introduce the new type VII., in combination with type VI. And if in (230) we substitute for the symbol ( $fgh$ ) its value given by (229), and so combine types III. and VI., we obtain the equation

$$\frac{fee + fhh}{hee + hff} = \frac{gff \cdot (fee + fgg) - fgg \cdot (gee + gff)}{hee \cdot (gee + gff)}; \quad \dots \quad (231)$$

that is, by (209),

$$\frac{fge}{hge} + \frac{fgg}{hee} = \frac{gff}{hee} \cdot \frac{fhe}{ghe}; \quad \dots \quad (232)$$

or finally,

$$\text{V.} \quad hee \cdot fge = fgg \cdot ghe - gff \cdot fhe. \quad \dots \quad (233)$$

But this is exactly what the type V., or the formula (154), becomes for quines, when we suppress the sum  $\Sigma''$ , change  $k$  to  $h$ , and advance cyclically the three indices  $efh$ ; it includes therefore the equations (155), which were the only remaining conditions of association to be fulfilled. If then we satisfy the *two types*, III. and VI., we shall satisfy *all the conditions of association for quines*: since we shall thereby have satisfied *also* the *four other earlier types*, namely those numbered as I. II. IV. V. It only remains then to consider *what new restrictions* on the constants ( $eff$ ) are introduced by the comparison of the values which type III. gives for the other constants ( $efg$ ), as expressed in terms of them, with the values furnished by type VI.; or to discuss the consequences of the following general formula, ob-

\* It will be shown that this *single type* (the seventh) *includes all the others*, or is sufficient to express *all the general conditions* of association, between the 24 symbols of the forms ( $efg$ ) and ( $eff$ ). But the eliminations required for this deduction cannot be conveniently described at this stage.

tained by eliminating the symbol  $(fgh)$  between (229) (230), and not essentially distinct from the recent equation (231):

VIII...  $hee.(fee + fhh)(gee + gff) = (hee + hff)(fee.gff - gee.fgg)$ ; (234)  
 which contains all the old and new relations, subsisting between the twelve constants of the form  $(eff)$ , and may be regarded as an *eighth type* for quines.

[34.] Denoting the first minus the second member of (234) by the symbol  $[efgh]$ , we easily see that

$$\begin{aligned} [efgh] &= gee.(v_f.hee + fgg.hff) + gff.(fhh.hee - hff.fee) \\ &= hee.(v_f.gee + fhh.gff) + hff.(fgg.gee - gff.fee); \end{aligned} \quad (235)$$

and therefore that we have, *identically*,

$$[efgh] = [efhg]; \quad . \quad . \quad . \quad . \quad (236)$$

this last or eighth type (234) contains therefore, at most, only a system of twelve equations. Interchanging  $f$  and  $g$ , and attending to the notation (202), we see, by (203) (234), that of the three equations

$$[efgh] = 0, \quad [egfh] = 0, \quad [e] = 0, \quad . \quad . \quad (237)$$

any two include the third; if then we only seek what *new* conditions, additional to those marked (204), are to be satisfied by the symbols  $(eff)$ , or rather by the eight following ratios of those symbols,

$$eff : egg : ehg : fee : fgg : fhh : gee : gff : ghg : hee : hff : hgg, \quad (238)$$

we need only retain at most four new equations, suitably selected from among those furnished by type VIII., such as the four following, which differ among themselves by the initial letters within the brackets, and so belong to different groups,

$$[efgh] = 0, \quad [fghe] = 0, \quad [ghef] = 0, \quad [hefg] = 0; \quad (239)$$

and then to combine these with any three of the four former relations (204), for example with the three first, namely

$$[e] = 0, \quad [f] = 0, \quad [g] = 0; \quad . \quad . \quad . \quad (240)$$

from which the fourth equation  $[h] = 0$  would follow, by means of the *identity*,

$$\begin{aligned} (ehf)_o(fge)_o(gfh)_o(heg)_o &= (ehg)_o(fgh)_o(gfe)_o(hef)_o \\ &= (eff + egg)(fee + fhh)(gee + ghg)(hff + hgg). \quad . \quad . \quad (241) \end{aligned}$$

It might seem however that the seven equations (239) and (240), thus remaining, should suffice to determine seven of the eight ratios (238): whereas I have found that it is allowed to *assume two pairs of ratios* arbitrarily, out of the four pairs (238), and then to deduce the two other pairs from them. For I find that it is sufficient to retain, instead of the *twelve* equations included under type VIII., or the *seven* equations (239) (240), a system

of only four equations of the type just mentioned; namely two pairs, selected from any two of the four groups, which have (for each group, and also for each pair) a common initial letter within the brackets; for instance, these two pairs of equations:

$$[efgh]=0, [egfh]=0; [fegh]=0, [fgeh]=0; \dots (242)$$

which leave as many as eight arbitrary constants (for example these eight, *eff, egg, ehh, fhh, ghh, hee, hff, hgg*, from which all the rest can be determined) in the resulting system of associative quines. An outline of the investigation by which this important reduction is effected, may be presented in the following way.

[35.] The two first equations (242) connect the three last pairs of ratios (238), in such a manner that when any two of those three pairs are assumed, or known, the third can be determined. Hence, with the interpretation (197) of the symbol  $(ghf)_0$ , we easily find that those two equations (242) give,

$$(e) \dots \left\{ \begin{array}{l} gee \cdot hgg - ghh \cdot hee = (ghf)_0 \cdot fee; \\ v_g \cdot hee + gff \cdot hgg = (ghf)_0 \cdot fgg; \\ -(v_h \cdot gee + hff \cdot ghh) = (ghf)_0 \cdot fhh; \end{array} \right\} \dots (243)$$

because we find that *fee, fgg, fhh* are proportional to the left-hand members of these last equations (243); and that the sum of the two first of those left-hand members is identically equal to the product  $(gee + gff)(hee + hgg)$ . For the same reason, the two first of these three equations (243) express really only one relation, namely that which is contained in the second equation (242), although they do so under different forms, both of which it is useful to know; and it is convenient to have ready also this other combination, obtained by adding the three equations (243) together,

$$(e) \dots v_h \cdot gff - v_g \cdot hff = v_f \cdot (ghf)_0; \dots (244)$$

which, like those equations (243) themselves, we shall consider as belonging to the group (e), because they are all derived from two of the three equations of that group, included under type VIII., which in the recent notation  $[efgh]$  have *e* for their initial letter; and because the third equation of that group, included under the same type, namely

$$(e) \dots [ehfg]=0, \dots (245)$$

may be derived from them, by the elimination of the symbol  $(ghf)_0$  between the first and third of the equations (243). In like manner the two last equations (242) include a third of the same type VIII., and belonging to the same group (f), namely

$$(f) \dots [fheg]=0; \dots (246)$$

because they conduct to the following system of expressions,

which may be formed from (243) (244) by cyclical permutation of  $efg$ :

$$(f) \dots \left\{ \begin{array}{l} eff \cdot hee - eh h \cdot hff = gff \cdot (ehg)_0; \\ v_e \cdot hff + egg \cdot hee = gee \cdot (ehg)_0; \\ -(v_h \cdot eff + hgg \cdot eh h) = gh h \cdot (ehg)_0; \\ v_h \cdot egg - v_e \cdot hgg = v_g \cdot (ehg)_0. \end{array} \right\} \quad (247)$$

Multiplying then the equations (243) and (244) by  $(ehg)_0$ , and observing that the identity (220) gives

$$(ehg)_0 \cdot (ghf)_0 = (hee + hff)(ehf)_0, \quad (248)$$

we find, on substitution of the first for the second members of (247), that the results are divisible by  $hee + hff$ ; and that thus the elimination of the third pair of ratios (238), between (243) (244) (247), or between the four equations (242), conducts to expressions of the recent forms, namely,

$$(g) \dots \left\{ \begin{array}{l} hee \cdot egg - eh h \cdot hgg = fgg \cdot (ehf)_0; \\ v_e \cdot hgg + eff \cdot hee = fee \cdot (ehf)_0; \\ -(v_h \cdot egg + hff \cdot eh h) = fh h \cdot (ehf)_0; \\ v_h \cdot eff - v_e \cdot hff = v_f \cdot (ehf)_0. \end{array} \right\} \quad (249)$$

A similar analysis may be applied to effect the elimination of the fourth pair of ratios (238), with results entirely analogous. On the whole then it is found, that the four equations (242) express such connexions between the four pairs of ratios (238), as to satisfy not only the two remaining equations, (245) and (246), of their *own* groups, ( $e$ ) and ( $f$ ), but also the six other equations of the two *other* groups, ( $g$ ) and ( $h$ ), included under type VIII.; namely

$$\left. \begin{array}{l} (g) \dots [gef h] = 0, \quad [gfeh] = 0, \quad [gh ef] = 0; \\ (h) \dots [hefg] = 0, \quad [hfeg] = 0, \quad [hgef] = 0; \end{array} \right\} \quad (250)$$

for the first line is satisfied by the ratios (249), and the second line by the analogous ratios, which are found in a similar way. Thus all the *twelve* equations of type VIII. are satisfied, if we satisfy only *four* suitably selected equations of that type; for example, the equations (242): which was what we proposed to demonstrate.

[36.] The *eighty* equations of association, assigned in the Third Section, between the *twenty-four* constants  $l_1 \dots u_3$ , or ( $efg$ ), ( $eff$ ), have therefore, by the recent analysis, been ultimately *reduced to sixteen*; namely the *four* equations which thus remain from the last type VIII.; and the *twelve* others which were contained in the type III., established in that earlier Section: and which (as was lately remarked) leave still no fewer than **EIGHT**

CONSTANTS ARBITRARY in this theory of ASSOCIATIVE QUINES. We may indeed vary in many ways, consistently with the same general theory, and by the assistance of the other recent types VI. and VII., the system of the sixteen equations of condition which are to be satisfied, and the choice of the eight constants which are to be regarded as still remaining arbitrary and undetermined: and it may not be useless, nor uninteresting, to make some remarks hereafter, upon the subject of such selections. But in the mean time it appears to be important to observe, that if some of the recent results, especially the formulæ (210), (228), be combined with some of those previously obtained, and more particularly with the equations (112), (121), of Section III., the following very simple expressions are found, for the ten remaining *constants of multiplication*, the discussion of which had been reserved:

$$(f) = v_f^2; (fg) = v_f \cdot v_g; \dots \dots \dots (251)$$

or, with the notations *abc*, and with the usual cyclical permutation of the indices 1, 2, 3,

$$a_1 = v_1^2, \quad a_4 = v_4^2, \quad b_1 = v_2 v_3, \quad c_1 = v_1 v_4. \dots \dots (252)$$

If then we write for abridgement,

$$\left. \begin{aligned} v &= v_1 x_1 + v_2 x_2 + v_3 x_3 + v_4 x_4, \\ v' &= v_1 x'_1 + v_2 x'_2 + v_3 x'_3 + v_4 x'_4, \end{aligned} \right\} \dots \dots (253)$$

the square of any quadrinomial vector  $\omega$ , and the scalar of the product of any two such vectors, will take these remarkably simple forms:

$$\omega^2 = v^2; \quad S\omega\omega' = v \cdot v'; \dots \dots \dots (254)$$

this latter scalar thus decomposing itself into a product\* of two linear functions of the constituents, namely those here denoted by *v* and *v'*. And because it is easy to prove, from what has been already shown, compare (244), that in the present theory the constants *v<sub>e</sub>* are connected by relations of the form

$$-v_e \cdot efe = v_e \cdot fee = v_f \cdot eff + v_g \cdot efg + v_h \cdot efh, \dots (255)$$

we find, by multiplying this equation by *v<sub>g</sub>*, and attending to (251), the following theorems for those *general associative quines* which have been in this section considered:

\* A similar decomposition into linear factors takes place for the *quadrinomes* (A) of par. [13.], but at the expense of one of the six arbitrary constants *l<sub>1</sub> l<sub>2</sub> l<sub>3</sub> m<sub>1</sub> m<sub>2</sub> m<sub>3</sub>*, when we establish between those symbols the relation,

$$l_1 m_1^2 + l_2 m_2^2 + l_3 m_3^2 = l_1 l_2 l_3 + 2m_1 m_2 m_3.$$

In general, I find that it is possible to satisfy all the conditions of association for *polynomes*, and at the same time to secure a decomposition of *Sωω'* into linear factors, while yet preserving so many as *3n - 4* constants of multiplication arbitrary. (For quadrinomes, *3n - 4 = 9 - 4 = 5*; for quines, *3n - 4 = 12 - 4 = 8*.)

$$\left. \begin{aligned} 0 &= S t_g V t_e t_f = S t_g t_e t_f; \\ 0 &= S \omega \omega' \omega''; \quad 0 = (V \omega \omega')^2; \end{aligned} \right\} \dots (256)$$

results which may be compared with some of those obtained in Section IV., for the two particular quinc-systems, (A) and (B).

Observatory of Trinity College, Dublin,  
February 14, 1855.

[To be continued.]

**XLIV.** *Observations on the "Magnetic Medium," and on the Effects of Compression.* By Professor W. THOMSON.

**T**HE following letter was received a few days ago. It was not written for publication, but the subject to which it refers being of general interest at present, I ventured to suggest to Professor Thomson the desirableness of having the letter printed. This he at once agreed to. With the exception of a paragraph relating to matters of a purely private nature, the letter appears as I received it.

JOHN TYNDALL.

March 24, 1855.

2 College, Glasgow,  
March 12, 1855.

MY DEAR SIR,

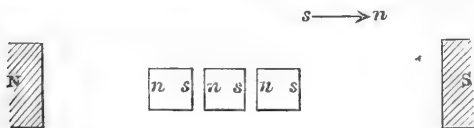
Allow me to thank you for the abstract of your lecture on magnetism, and the copy of your letter to Mr. Faraday, which I have recently received from you, and have read with much interest. I am still strongly disposed to believe in the magnetic character of the medium occupying space, and I am not sure but that your last argument in favour of the reverse bodily polarity of diamagnetics may be turned to support the theory of universally direct polarity. There is no doubt but that the medium occupying interplanetary space, and the best approximations to vacuum which we can make, have perfectly decided mechanical qualities, and among others, that of being able to transmit mechanical energy in enormous quantities (a platinum wire, for instance, kept incandescent by a galvanic current in the receiver of an air-pump, emits to the glass and external bodies the whole mechanical value of the energy of current spent in overcoming its galvanic resistance). Some of these properties differ but little from those of air or oxygen at an ordinary barometric pressure. Why not, then, the magnetic property? (of which we know so little that we have no right to pronounce a negative). Displace the interplanetary medium by oxygen, and you have a slight increase of magnetic polarity in the locality with a drawing in of the lines of force. Displace it with a piece of bismuth or a piece of wood, and a slight decrease of magnetic polarity through the locality takes place, accompanied by a pushing out of the lines of force. A state of strain by compression



may enhance, in the direction of the strain, that quality of the substance by which it lessens the magnetizability of the space from which it displaces air or "æther;" just as a similar state may enhance, in the direction of compression, the augmenting power of a paramagnetic substance.

By the bye, a long time ago (rather more than a year after the Edinburgh meeting of the British Association) I repeated with much pleasure some of your compression experiments, and found a piece of fresh bread instantly affected by pressure, so as always to turn the compressed line perpendicular to the lines of force, to whatever form the fragment was reduced. A very slight squeeze between the fingers was quite enough to produce this property, or again to alter it so as to make a new line of compression set equatorially. I repeated it a few days ago with the same results, and got a ball of bismuth, too, to act similarly. I remember formerly finding the bread *attracted* as a whole, instead of being repelled, as I expected from your results. I suppose, however, this must have resulted from some ferruginous impurities, which it may readily have got either in the course of the experiments with it, or in the baking. I mean to try this again\*.

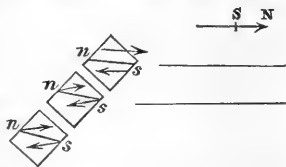
I do not quite admit the argument you draw from your compression experiments regarding the effect of contiguity of particles, because in fact we know nothing of the actual state of the molecules of a strained solid. You have made out a most interesting fact regarding their magnetic bearings; but experiments are neither wanted, nor can be made, to show any sensible effect whatever of the mutual influence of a row of small pieces of bismuth placed near one another, or touching one another. It is perfectly easy to demonstrate that it *must* be such as to impair the "diamagnetization" of each piece when the line of the row is parallel to the lines of force, and to enhance it when that line is perpendicular to the lines of force, but in each case to so infinitesimally minute a degree, as to be wholly inappreciable to the most refined tests that have ever been applied. For let the lines of force be parallel to the line shown in the figure,



and act on a steel needle in the manner there represented. Then, whatever hypothesis be true for diamagnetism, there is not a

\* Prof. Thomson's supposition is correct; pure bread is *repelled* by a magnetic pole. I may remark that I am at present engaged in the further examination of the influence of compression, and have already obtained numerous instructive results.—J. T.

doubt but that each piece is acted on, and consequently reacts, precisely as a piece of steel very feebly magnetized, with its magnetic axis reverse to that of a steel needle free to turn, substituted for it, would do. Each piece of bismuth therefore acts as a little magnet, having its polarity as marked in the diagram, would do. Hence the magnetizing force by which the middle fragment is influenced is less than if the two others were away (this being such a force as would be produced by a north pole on the right-hand side of the diagram, and a south pole on the left). It is easily seen, similarly, that if the line joining the centres be perpendicular to the lines of force, the magnetizing force on the space occupied by the middle fragment is increased. Corresponding assertions are true for the terminal fragments, although the disturbing effect will be less on them in each case than in the middle one. Hence the diamagnetization of each will be enfeebled in the former case and enhanced in the latter, by the presence of the others. It follows, according to the principle of superposition of magnetizations, that if the line of the row be placed



obliquely across the lines of force, the magnetic axis of each particle, instead of being exactly parallel to the lines of force, will be a little inclined to them, in the angle between their direction and the direction transverse to the bar. The magnets causing the force of the field must act on the little diamagnets, each with its axis thus rendered somewhat oblique, so as to produce on it a statical couple (as shown by the arrow heads), and the resultant of the couples thus acting on the fragments will, when all these are placed on a frame, or rigidly connected, tend to turn the whole mass in such a direction as to place the length of the bar along the lines of force. Still, I repeat, this action, although demonstrated with as much certainty as the parallelogram of forces, is so excessively feeble as to be absolutely inappreciable. A fragment of bismuth, of any shape whatever, held in any position whatever in any kind of magnetic field, uniform or varying most intensely, only exhibits the resultant action of couples on all its small parts if crystalline, and of forces acting always according to Faraday's law on them if the field in which it is placed be non-uniform. Some phænomena that have been observed are to be explained by the resultant of forces from places of stronger to places of weaker intensity in the field, others by the resultant of couples depending on crystalline structure, and others by the resultant of such forces and couples coexisting; and none observed depend at all on any other cause.

I gave a very brief summary of these views (which I had ex-

plained somewhat fully and illustrated by experiments on paramagnetics of sufficient inductive capacity to manifest the effects of mutual influence, at the meeting at Belfast) as an abstract of my communication, for publication in the Report of the Belfast meeting of the British Association, where you may see them stated, I hope intelligibly. The experiments on the paramagnetics are very easy, and certainly exhibit some very curious phænomena, illustrative of the resultant effects due to the attractions experienced by the parts in virtue of a variation of the intensity of the field, and to the couples they experience when their axes are diverted from parallelism to the lines of force by mutual influence of the magnetized parts.

I had no intention of entering on this long disquisition when I commenced, but merely wished to try and briefly point out, that the assertions I have made regarding mutual influence are demonstrable in every case without special experiment, are confirmed amply by experiment for paramagnetics, and are absolutely incontrovertible, as well as incapable of verification, by experiment or observation on diamagnetics.

Believe me, yours very truly,

WILLIAM THOMSON.

Prof. Tyndall.

#### XLV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 236.]

Dec. 14, 1854.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—"The Physical Theory of Muscular Contraction." By Charles Bland Radcliffe, M.D.

The theory set forth in this paper is, that muscle is *prevented from contracting* by the several vital and physical agencies which act as stimuli upon muscle,—volition, nervous influence, blood, electricity, light, heat, and the rest,—and that *contraction happens on the cessation of stimulation*, by virtue of the operation of that universal principle of attraction which belongs to muscle in common with all matter, and, so happening, that it is a *physical* phænomenon of the same nature as that contraction which takes place in a bar of metal on the abstraction of heat.

This theory is supported by various arguments, some of which are now stated for the first time. It is argued:—

(a.) That *nervous influence* cannot cause muscular contraction, (1) because the degree of innervation, as measured by the supply of nerves, is inversely related to the tendency to contraction; (2) because contraction does not take place so long as the nerve gives evidences of electricity (Du Bois Reymond); (3) because, in some instances at least, contraction does not happen so long as the nerve

gives evidences of "irritability"—for contraction is not caused by heat, by acids and alkalis, and by several other chemical and medicinal substances, until the possibility of provoking contraction by the touch of a needle has been destroyed by the action of the agent—until, that is to say, the "irritability" of the nerve has been destroyed by this action (Eckardt); and (4) because the influence of the nervous centre in causing contraction is to suspend the natural electricity of the nerve and muscle. This last conclusion is evident in the fact, that the signs of electricity, which are absent during tetanus, immediately reappear in the muscle and in the portion of nerve connected with it, when the influence of the nervous centre is cut off and the tetanus resolved by dividing the nerve.

(b.) That *blood* cannot cause contraction, (1) because the tendency to contraction is inversely related to the supply of blood; thus, this tendency is greater in the voluntary muscles of fishes and reptiles than of mammals and birds—greater in involuntary than in voluntary muscles—greater in the muscles of any given animal during the state of hibernation than during the period of summer life; and (2) because the state of *rigor mortis* may be relaxed more than once, and the lost "irritability" restored to the muscle by the injection of living blood into the vessels (Brown-Séguard).

(c.) That *electricity* cannot cause contraction, (1) because there is a constant current of electricity in a muscle during rest, *but not during contraction* (Du Bois Reymond),—because, that is to say, contraction is absent when muscle is in a state of electrical or *polar action*, and present when this state is absent, so that contraction appears to be antagonized by this state of polar action; and (2) because contraction is never coincident with the passage of a current of artificial electricity; for, not only is it true that a muscle does not contract during the time that a current of artificial electricity is passing through it, but *contraction is invariably relaxed if contraction pre-existed* (Eckardt). There is, indeed, momentary contraction at the opening or at the closing of the circuit, but this contraction can be shown to be coincident with neutralization of electrical action, which neutralization is consequent upon the momentary opposition of the natural current of the muscle and the artificial current.

(d.) That *mechanical agents* cannot stimulate contraction, (1) because the electrical phenomena of muscle are opposed to such an idea; thus muscle affords evidences of electricity during rest, but not during contraction, and hence the probability is that electricity has been discharged when a muscle contracts on being touched by a needle,—a probability which is supported by the analogy which exists between the structure of muscle and the structure of the electrical organ of the Torpedo, and between the circumstances producing contraction on the one hand and discharge on the other (Owen, Faraday, and others); and (2) because the movements of the stomach, or uterus, or any other viscus are not to be accounted for on the supposition that the contractions are stimulated by the contents of the viscus; thus the food accumulates and the stomach expands until the appetite is satisfied, and contraction does not happen until

the preliminary processes of digestion are at an end, and thus also the child grows and the uterus expands, and labour pains do not begin until the growth of the child is completed, and the *stimulus* of that growth suspended.

(e.) That *heat and cold* do not *stimulate* contraction, because contraction does not happen until the *natural polar action* of the muscle is suspended,—an event which happens equally under either extreme of temperature,—and thus the muscle would seem to contract because the heat or cold extinguishes that polar action of the muscle which antagonizes contraction.

(f.) That *light* cannot cause contraction, (1) because it exercises a directly opposite influence upon the irritable cushions of the sensitive plant; and (2) because it is as easy to agree with Bichât, and suppose that light expands the curtain of the iris, as that it causes contraction in sphincter-fibres surrounding the pupil, which fibres have no existence.

(g.) That *chemical and mechanical agencies* do not stimulate contraction, because contraction does not happen until the agent has destroyed that polar action of the muscle which antagonizes contraction (Eckardt).

It is argued, also, that the action of the *will* upon muscle is not necessarily that of a *stimulus*, for the will *may act* by withdrawing something from the muscle as well as by communicating something to the muscle, and, if so, then the previous considerations enhance the probability that it acts by withdrawing something.

In the course of the argument it is further shown that this conclusion is borne out by the history of the muscular movements which are manifested in the coats of vessels and in the heart, while at the same time this view is found to give the clue to the physical interpretation of “capillary action,” and of rhythm, whether this be in the heart or elsewhere.

It is shown, also, that the same conclusion is borne out by the pathology of tremor, convulsion, and spasm,—of those diseases, that is to say, in which muscular contraction is in excess. Thus, (to mention one argument out of many,) the state of circulation which is invariably associated with tremor, convulsion, and spasm, is one which necessarily implies the diminution of all accustomed stimulation in the muscle, for it is a state which borders closely upon syncope or asphyxia.

And, lastly, it is shown that there is nothing in the phenomenon of muscular contraction which need prevent it from being referred to the operation of that common principle of attraction which belongs to muscle *in common with all matter*, and thus the general conclusion is that another barrier between the organic and inorganic world is broken down, and that muscular contraction is an effect of the universal law of gravitation.

There are, however, sundry grave objections to this theory, and one main object of the paper under consideration is to remove them. Thus, for example, if muscle contracts when nervous influence is withdrawn, how is it that it relaxes when the nerve is divided or

otherwise paralysed? and if a muscle contracts for want of blood, how is it that it relaxes in syncope, asphyxia, and death? These objections are grave, but not unsurmountable, as the following hints at explanation will serve to show.

It must be understood, then, that that state of polar action which is present in a muscle during rest and absent during contraction, *is re-established immediately after contraction*; it must also be understood that this state of polar action in the muscle is suspended during ordinary muscular contraction by certain changes which take place in the nervous centre, and that it has *died out* when contraction happens after death, as in *rigor mortis*; and the rest is sufficiently simple.

It is quite in accordance with the theory, then, that a muscle should contract when nervous influence is withdrawn, and that it should relax after the nerve is divided or otherwise paralysed. At the moment when the continuity of the nerve is broken the muscle contracts, because the influence of the nervous centre is cut off; but this contraction cannot continue, because that state of polar action which antagonizes contraction is immediately re-established in the muscle, and in the portion of nerve connected with it. This relaxation, moreover, must continue, if the paralysed muscle be left to itself, so long as the muscle continues to be the seat of this polar action. And, on the other hand, this contraction must return when this action is suspended, or diminished, or extinguished, as indeed it does; thus the muscle contracts when the polar action is suddenly suspended by galvanism or by the touch of a needle; thus it contracts after the paralysis has continued for some time, and when the failure in the nutrition of the muscle has entailed a corresponding failure in its polar action; and thus it contracts in *rigor mortis*, when all polar action is finally extinguished.

It is also in accordance with theory that tremor, convulsion, and spasm should be caused by want of blood, and that they should cease when the circulation fails, as it fails in syncope, asphyxia, or death. During tremor, convulsion, or spasm, the muscles are insufficiently supplied with nervous influence, because the deficient supply of blood to the nervous centres involves a corresponding deficiency in the degree of innervation; but once let the circulation fail below a certain point, and the whole case is altered. During tremor, convulsion, and spasm, the supply of blood to the nervous centres is insufficient to keep up the normal degree of innervation, *but it is sufficient to prevent the nerves from being paralysed*, and hence the contractions in the muscles, for the nerves being conductors, the failure in the action of the nervous centres is propagated along them to the muscles, and of this failure the contractions are the consequence. But if the circulation fails below a certain point, *the nerves are paralysed for want of blood*, and being paralysed, the failure of innervation in the nervous centres, even though this be now complete, does not entail a corresponding failure in the polar action of the muscle, because the nerves are no longer conductors; and not doing this, the polar action of the muscle, which is much more

vigorous than that of the nervous centre and nerve, and far less dependent upon the supply of blood, is immediately re-established, and being re-established, the muscle relaxes (just as it does in the case where paralysis is caused by division of the nerve), and tremor, convulsion and spasm are at an end. Nor is there any doubt that the nerves are paralysed when the circulation fails to the point which is here supposed. Thus, if the circulation in the hand be depressed by immersion in cold water, the sense of touch and the power of movement are partially or wholly destroyed; or if the principal vessel of a limb be tied, the nerves are similarly paralysed until the collateral circulation be established; and in each case, also, the power of provoking "reflex movements" is diminished or destroyed. In either case the nerves are more or less paralysed for want of blood, and, if so, it surely follows that the nerves must be paralysed, and still more effectually, when the circulation fails as it fails in syncope, asphyxia, or death, and when the movement of the blood is almost or altogether at an end. Hence it is quite intelligible that tremor, convulsion or spasm should be caused by want of blood, as is stated in the argument, and that they should cease in syncope, asphyxia, and death; and thus this objection falls to the ground, and with it all objections of the same kind.

Such is an imperfect sketch of the evidence upon which the physical theory of muscular contraction is founded.

Dec. 21, 1854.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"Remarks on the Anatomy of the *Macgillivrayia pelagica* and *Cheletropis Huxleyi* (Forbes); suggesting the establishment of a new genus of Gasteropoda." By John D. Macdonald, R.N., Assistant-Surgeon H.M.S. Herald.

Having examined the anatomy of the *Macgillivrayia pelagica* and several smaller species of pelagic Gasteropoda, not exhibiting the least similarity in the character of their shells, the author found that they all presented a very close relationship to each other in the type of their respiratory organs, and in other points of structure of less importance.

The gills in every instance seemed to be fixed to the body of the animal immediately behind the head, and did not appear to be appended to the mantle, as in the Pectinibranchiata properly so called. They were also invariably four in number, and arranged in a cruciform manner round a central point. They were free in the rest of their extent, elongated and flattened in form, with a pointed extremity, and fringed with long flowing cilia, set in a frilled border. They were, moreover, furnished with muscular fibres, both transverse and longitudinal, and exhibited great mobility when protruded, but lay side by side in the last whorl of the shell when retracted.

The auditory capsules, each containing a spherical otolithe, were closely applied to the inner and posterior part of the larger or anterior ganglion of the subœsophageal mass.

There were two tentacula, each bearing at the outer side of

its base an eye consisting of a globular lens with optic nerve and retinal expansion. The foot was large and very mobile, but a vesicular float has been observed only in *Macgillivrayia*.

The respiratory siphon was either a simple fold of the mantle forming a temporary tube (*Cheletropis*), or a fold whose borders were united through their whole length, leaving an aperture at the end, as in *Macgillivrayia*.

A lingual ribbon with well-marked rachis and pleuræ occurs in all the species. Very perfect labial plates with closely-set dental points arm the mouth in some instances, and probably in all.

The little animals possessing in common the characters here described, nevertheless fabricate shells so very different as to admit of their division into well-marked genera.

The author conceives that the obvious difference between the pectinibranchiate type of respiratory organs and that observed in the group of Gasteropoda now under consideration, affords sufficient grounds for placing the latter in a distinct order by themselves; and as illustrations of it he proceeds to give some details of the structure of the two species mentioned in the title of the paper, whose shells have been already described by the late Prof. E. Forbes, and figured in Mr. Macgillivray's 'Narrative of the Voyage of H.M.S. Rattlesnake.'

In *Macgillivrayia* the disc of the foot is broad and connected by a narrow attachment to the body just beneath the neck; it carries an operculum behind, and is cleft by a notch in front. A raphe observable in the median line, as well indeed as the whole character of this part of the organ, seems to shadow forth the transformation of the single foot of the Gasteropod into the wing-like expansion of the Pteropod.

After describing the labial plates and lingual strap, the eyes and the branchiæ, the author observes that the tubular siphon protrudes from the shell on the left side and seems to indicate the coexistence of a respiratory chamber with naked branchiæ.

The vesicular float, like that of *Ianthina*, noticed by Mr. Macgillivray, consists of an aggregation of vesicles varying both in number and size in different cases. It is exceedingly delicate, and could not be found in the specimens first obtained, having probably been destroyed or detached from the foot by the force of the water running through the meshes of the net with which they were captured. Its coexistence with an operculum shows that it is not a modification of the latter.

Of the *Cheletropis Huxleyi*, numerous specimens were found in Bass's Straits and in the South Pacific, between Sydney and Lord Howe's Island.

After giving some details respecting the shell and the foot, the author observes that the latter organ was destitute of float, at least in the specimens he obtained, but was furnished with an operculum, which, probably from its extreme thinness and smallness, had escaped the notice of Professor Forbes. He then points out the peculiarities of the respiratory apparatus.



The portion of the mantle which forms the respiratory siphon, is short, and its opposite edges are merely in apposition, without organic union. The branchiæ are of two kinds, covered and naked. The covered gill is single but of considerable length. It is beautifully pectinated, and fringed with long cilia, and, doubtless, represents the respiratory organ of the pectinibranchiate Gasteropoda. The basis of this part is a long and narrow strip of a tough and fibrous material, folded upon itself into a series of loops invested with a coating of epithelium, and richly ciliated along the free border. The naked gills are four in number, similar both in situation and character to those of *Macgillivrayia*. Each gill is of an oval or elongated form, presenting a thin, frilled and corrugated border, beset with long whip-like cilia. In the central parts muscular fibres are distinctly discernible, some disposed lengthwise and others transversely.

The lingual strap is next described, as well as two file-like triturating plates with which the mouth is furnished.

The two tentacula of each side appear as it were enclosed in one envelope, so as to form a single tactile instrument, which bears a large dark eye on its outer side near the base. To this latter organ the tegumentary covering forms a kind of cornea, beneath which is a spherical lens resting on a mass of black pigment, both being enclosed in a little sac; and the optic nerve, emerging from the sub-œsophageal ganglion, joins the miniature globe and expands into a retina. The author was unable to trace an opening through the pigment for the passage of light, but thinks it probable that, as in the ocelli of insects, such an aperture exists in the central part. The auditory capsules are situated at some distance behind the eyes, and may be distinctly seen with the microscope when the surrounding parts are carefully removed with fine needles. They are of a rounded or oval form, and each contains a beautifully transparent and highly refracting otolithe, much larger than the lens of the eye.

January 11, 1855.—Thomas Bell, Esq., V.P., in the Chair.

The following communications were read:—

“On the Development of Muscular Fibre in Mammalia.” By William S. Savory, M.D.

The author's observations were made chiefly upon foetal pigs, but they have been confirmed by repeated examinations of the embryos of many other animals, and of the human foetus.

If a portion of tissue immediately beneath the surface from the dorsal region of a foetal pig, from one to two inches in length, be examined microscopically, there will be seen, besides blood-corpuscles in various stages of development, nucleated cells and free nuclei or cytoblasts scattered through a clear and structureless blastema in great abundance. These cytoblasts vary in shape and size; the smaller ones, which are by far the most numerous, being generally round, and the larger ones more or less oval. Their outline is distinct and well defined, and one or two nucleoli may be seen in their interior as small, bright, highly refracting spots. The rest of their substance is either uniformly nebulous or faintly granular.

The first stage in the development of striated muscular fibre consists in the aggregation and adhesion of the cytoblasts, and their investment by blastema so as to form elongated masses. In these clusters the nuclei have, at first, no regular arrangement. Almost, if not quite as soon as the cytoblasts are thus aggregated, they become invested by the blastema, and this substance at the same time appears to be much condensed, so that many of the nuclei become obscured.

These nuclei, thus aggregated and invested, next assume a much more regular position. They fall into a single row with remarkable uniformity, and the surrounding substance at the same time grows clear and more transparent, and is arranged in the form of two bands bordering the fibre and bounding the extremities of the nuclei, so that now they become distinctly visible. They are oval, and form a single row in the centre of the fibre, closely packed together side by side, their long axes lying transversely, and their extremities bounded on either side by a thin clear pellucid border of apparently homogeneous substance.

It is to be observed how closely the muscular fibres of mammalia at this period of their development resemble their permanent form in many insects.

The fibres next increase in length and the nuclei separate. Small intervals appear between them. The spaces rapidly widen, until at last the nuclei lie at a very considerable distance apart. At the same time the fibre strikingly decreases in diameter; for as the nuclei separate, the lateral bands fall in and ultimately coalesce.

This lengthening of the fibre and consequent separation of the nuclei is due to an increase of material, and not to a stretching of the fibre.

Soon after the nuclei have separated some of them begin to decay. They increase in size; their outline becomes indistinct; a bright border appears immediately within their margin; their contents become decidedly granular; their outline is broken and interrupted; and presently an irregular cluster of granules is all that remains, and these soon disappear.

It sometimes happens that the nuclei perish while in contact, before the fibre elongates; but the subsequent changes are the same.

The striæ generally first become visible at this period, immediately within the margin of the fibre.

The fibre is subsequently increased in size, and its development is continued by means of the surrounding cytoblasts. These attach themselves to its exterior, and then become invested by a layer of the surrounding blastema. Thus, as it were, nodes are formed at intervals on the surface of the fibre. These invested nuclei are at first readily detached, but they soon become intimately connected and indefinitely blended with the exterior of the fibre. All its characters are soon acquired, the nuclei at the same time gradually sink into its substance, and an ill-defined elevation, which soon disappears, is all that remains.

Lastly, the substance of the fibre becomes contracted and condensed. The diameter of a fibre towards, or at the close of intra-uterine life, is considerably less than at a much earlier period.

At the period of birth muscular fibres vary much in size.

The several stages in the development of muscular fibre, above mentioned, do not succeed each other as a simple consecutive series; on the contrary, two, or more, are generally progressing at the same time. Nor does each commence at the same period in all cases.

“On the General Integrals of the Equations of the Internal Equilibrium of an Elastic Solid.” By W. J. Macquorn Rankine, F.R.S.S.L. & E.

The *First Section* of this paper is introductory, containing a summary of principles already known respecting the elasticity of solids. Those principles are treated as the consequences of the following DEFINITION OF ELASTICITY, without introducing any hypothesis as to the molecular structure of bodies.

“Elasticity is the property which bodies possess of preserving determinate volumes and figures under given pressures and temperatures, and which in a homogeneous body manifests itself equally in every part of appreciable magnitude.”

The investigations are limited by the following conditions:—

1. The temperature of the elastic body is supposed to be constant and uniform.

2. The variations of the volumes and figures of its particles are supposed to be so small, that the elastic pressures may be considered as sensibly linear functions of those variations.

3. It is assumed, that the only force, besides elastic pressures, acting on the particles of the body, is that of terrestrial gravitation.

All possible small variations of volume and figure of an originally rectangular molecule, when referred to three orthogonal axes, may be resolved into six, viz. three linear dilatations or compressions, and three distortions.

In like manner the elastic pressures exerted on and by such a molecule may be resolved into six, viz. three normal pressures, and three tangential pressures.

Those six pressures are connected with each other and with the attractive force acting on the molecule, by three well-known differential equations of the first order.

They are also connected at every element of the surface of the body, by three well-known linear equations, with the components of the external force acting on that element.

The general problem to be solved is, to find the integrals of the first three equations, subject to conditions fixed by the last three.

The six variations of volume and figure of a rectangular molecule are expressed by six small fractions called “coefficients of displacement.”

If the differential of each of these fractions be multiplied by the pressure which directly tends to vary it, the sum of the products is the complete differential of a function called the *Potential Energy of Elastic Forces* for the molecule in question, which is sensibly a homogeneous quadratic function of the six fractions. It has twenty-one terms, and twenty-one constant coefficients, which constitute the *Coefficients of Elasticity* of the body, for the system of orthogonal axes chosen.

Twenty-one equations express the relations between the systems of coefficients of elasticity in a given body for any two different systems of orthogonal axes.

When a body possesses a system of orthogonal *axes of elasticity*, its coefficients of elasticity, when referred to these axes, are reduced to *nine*.

A body *isotropic* with respect to elasticity has but *three* coefficients of elasticity, which are the same for all sets of orthogonal axes, and are connected with each other by an equation.

If the Potential Energy of Elastic Forces be expressed as a homogeneous quadratic function of the *six elastic pressures*, its coefficients constitute the *coefficients of compressibility* and *extensibility*, and of *pliability*. They have relations to the coefficients of elasticity which are consequences of the properties of determinants.

The *Second Section* of the paper relates to the problem of the general integration of the equations of the internal equilibrium of an Elastic Solid, especially when it is *not isotropic*. The method of solution consists of the following eight processes:—

I. The centre of gravity of the body being (in general) taken for the origin of co-ordinates, the forces applied to the surface of the body are subdivided into nine systems of “*REDUCED EXTERNAL PRESSURES*,” which are of such a nature, that for any integration of the external forces as originally expressed over a portion of the surface of the body, may be substituted the sum of three integrations of certain of the reduced external pressures over the three projections of that portion of the surface upon the co-ordinate planes.

By such integrations, extended to the whole of the body, are found the mean values of the nine reduced external pressures, which are connected by simple equations with the mean values, or *constant terms*, of the six internal elastic pressures.

The deviations of the reduced external pressures above and below their mean values, constitute nine systems of *variable parts* of those pressures.

II. The *eighteen* coefficients of the three co-ordinates in the *linear terms* of the six internal elastic pressures are determined by means of eighteen equations; viz. three equations of internal equilibrium between certain of these coefficients and the force of gravity, and fifteen equations formed by means of the conditions of equilibrium of portions of the body cut off by the co-ordinate planes, and planes parallel to them.

III. The six constant terms, and the eighteen linear terms, of the three dilatations or compressions and the three distortions, are computed from the corresponding terms of the internal pressures by elimination, or by means of the coefficients of extensibility and compressibility, and of pliability. The coefficients of the co-ordinates in those twenty-four terms bear linear relations to the coefficients in the linear and quadratic terms of the three projections of the molecular displacement.

IV. The parts of the nine reduced external pressures corresponding to the constant and linear terms of the internal pressures having been determined for each element of the body's surface and sub-

tracted from the nine actual reduced external pressures, there remain nine *residual* reduced external pressures for each such element, which form three systems, each suitable for development in series of trigonometrical functions of a different pair of independent co-ordinates.

V. The parts of the three projections of the molecular displacement, which correspond to each system of residues of the reduced external pressures, are to be expressed by infinite series in terms of the sines and cosines of linear functions of the proper pair of independent co-ordinates, each order of terms containing (except in some special cases) four kinds of trigonometric functions, multiplied by six exponential functions of the third co-ordinate, whose parameters are the roots of an equation of the sixth order, and by twenty-four arbitrary constants.

From the expressions thus formed are to be computed symbolical expressions for the values of the system of residues or transcendental parts of the reduced external pressures, for each pair of independent co-ordinates, which, by the aid of the equation of the form of the surface of the body, are to be transformed into series containing terms in trigonometric functions of the independent co-ordinates only, multiplied by linear functions of the arbitrary constants, which are (in general) twenty-four times as numerous as the orders of terms.

VI. By equating the constant factor of each term of the symbolical developments thus formed, to the constant factor of the corresponding term of the arithmetical developments found by the process IV., there are formed as many linear equations between the arbitrary constants and known quantities as there are constants to be determined, from which equations those constants are found.

VII. Cases in which one ordinate intersects the surface of the body in two or more pairs of points are to be treated by a special method.

VIII. The results of the previous processes are to be combined, and the solution of the problem completed by determining and adding to them the displacements and rotations of the body as a whole.

The *Third Section* relates to the internal equilibrium of a rectangular prismatic body.

Processes I., II. and III. The determination of the constant and linear terms of the internal pressures, and the corresponding terms of the molecular displacements, consists in the special application of the methods of the preceding section. The axes of figure are taken for axes of co-ordinates.

IV. The means and differences of the transcendental residues of the reduced external pressures on each pair of faces of the prism are developed in series of trigonometric functions of the pairs of independent co-ordinates of the respective faces to which they are applied; the series employed being of such a nature, that for the edges of the body all their terms vanish.

V. and VI. An order having been fixed for the consideration of the forces acting on the three pairs of faces, let  $yz$ ,  $zx$ ,  $xy$  be that order.

Series of functions trigonometric in  $y$  and  $z$ , exponential in  $x$ , and satisfying the equations of internal equilibrium, with arbitrary constant coefficients, are taken to represent the molecular displacements produced by the residual pressures on the faces normal to  $x$ . From those series are computed series representing symbolically those residual pressures, which series being equated to the series numerically representing those pressures, the arbitrary constants are found by elimination.

The formulæ thus obtained are employed to compute ideal systems of external pressures on the faces normal to  $y$  and  $z$ , called "*Provisional pressures*," which are developed in trigonometric functions of the independent co-ordinates of the faces to which they are conceived to be applied. Should the provisional pressures agree with the actual residual pressures on those faces, the process is complete; should they not so agree, the provisional pressures are to be subtracted from the actual residual pressures, leaving systems of remainders called "*Secondary pressures*."

The series representing the molecular displacements corresponding to the *Secondary Pressures* on the faces normal to  $y$  are to be found in the manner already referred to. The formulæ thus obtained are to be employed to compute an ideal system of "*Provisional Secondary Pressures*" on the faces normal to  $z$ , which are to be developed in trigonometric functions of  $x$  and  $y$ .

Should the provisional secondary pressures thus found agree with the actual secondary pressures on the faces normal to  $z$ , the process is complete. Should they not so agree, the provisional are to be subtracted from the actual secondary pressures, leaving a system of remainders called "*Tertiary Pressures*" on the faces normal to  $z$ , whose effects are to be computed in the usual manner.

Process VII. is not required.

Process VIII. consists in combining the terms of the molecular displacements due to the constant and linear terms of the internal pressures, the residual pressures on the faces normal to  $x$ , the secondary pressures on the faces normal to  $y$ , and the tertiary pressures on the faces normal to  $z$ , and finally determining and adding to the other terms, those depending on the displacements and rotations of the prism as a whole.

The *Fourth Section* relates to the integrals of the equations of the internal equilibrium of an isotropic elastic solid.

The constant and linear terms of the internal pressures are to be determined by the methods described in the previous sections, for all solids, whether isotropic or not.

The transcendental terms of the internal pressures and molecular displacements in an isotropic elastic solid, require a special method for their determination.

The three projections of the molecular displacement, with all their functions, in an isotropic solid, are deducible from one primitive function or series of primitive functions of the co-ordinates, by certain processes of derivation, distributive, but not necessarily commutative.

Each primitive function must satisfy the condition

$$\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}\right)\Phi = 0,$$

and may belong to one or other of *eight* classes, according as it is even or odd with respect to  $x$ ,  $y$  and  $z$ .

The processes of derivation applicable to the primitive functions contain *three* arbitrary constants for each primitive function. Hence when there is a series of primitive functions of different orders, there are *twenty-four* arbitrary constants for each order of terms.

In the developments of the residual external pressures, there are also *twenty-four* constant coefficients for each order of terms, of which the arbitrary constants are linear functions.

The notation of M. Lamé's work on the Mathematical Theory of the Elasticity of Solid Bodies, so far as it relates to isotropic substances, is compared with that of this paper.

Reference being made to the known method of representing the elastic pressures at a given particle of a solid, in magnitude and direction, by the radii of an ellipsoid, and the positions of the surfaces to which those pressures are applied by those of the tangent planes to an ellipsoid or hyperboloid, the difference (not generally attended to) between the *cone of tangential pressures*, and the *cone of sliding*, is pointed out. This difference is important in the theory of the strength of materials.

January 18, 1855.—Sir Benjamin Brodie, Bart., in the Chair.

A paper was in part read, entitled "Ocular Spectres and Structures as Mutual Exponents." By James Jago, A.B. Cantab., M.B. Oxon.

The paper opens by stating that for want of a methodical elimination of ocular spectres from one another—a want which its aim is to meet—physiological optics remain to this day without any real foundation; and even when we have followed the rays of light through all the refracting media of the eye, we cannot safely assert what sensations belong to them until we have detected everything connected with the *percipient membrane* which may obstruct the action of light on it, or which may originate sensations *as of light* through other sorts of impulses. Our eyes in many important respects provide us with an opportunity for microscopical research that no optical instrument employed on the dead eye can rival. We may thus gather a variety of information, physical and physiological, solve points of ocular structure that escape other means of investigation, and bring a profusion of ingenious speculations to a termination, by showing that the phenomena (and this is especially true of the retinal phenomena) which have occasioned them are simply exponential of anatomical facts; and important physiological laws may be arrived at by like means.

The first step in the author's task is to determine the conditions which render objects existing upon or within the eye visible by their shadows, and to obtain optical principles by which we may examine

the interior of our own eye with facility, so as to recognize in what lenticular structure, and what part of it, the cause of any shadow or "diffractive image" resides. He shows that we may make every measurement of interest, may decide all the points just alluded to, at the instant, as it were, by mere inspection; and he illustrates his optical principles by appropriate experiments.

The paper then commences its actual elimination of ocular spectres from one another, starting from the appendages of the eye and going on through the ocular tissues in succession to the retina, under several heads, as—

#### *Optical Effects of the Eye-lashes, Eye-lids, and Conjunctival Fluids.*

These produce phenomena of reflexion, refraction, and inflection. They may multiply the images of objects which are without or which are within the eye, and occasion us to see the latter. The conjunctival fluids render apt illustrations of "recondite" diffractive shadows.

#### *Optical Structure of the Crystalline Lens.*

It is shown that the stelliform figure of our crystalline lens is distinctly visible in divergent light. The lens contains numerous bodies displaying a series of diffractive fringes. The fringes of the border of the iris are likewise conspicuous. Whenever light radiates into the eye from a near point, all these things happen. Hence when a line of radiants (an edge of any body) is before the eye, a mosaic fringe of these coloured shadows will be formed; and there is an *ocular* fringe, as well as the fringe on the edge of a body by light inflected at the body. The ocular colours mentioned seem to have been the cause of the belief that it can be *proved* experimentally that the eye contains no provision for the correction of chromatic dispersion; whereas the colours spoken of should only be compared with those that are produced by *flaws* in the glasses of optical instruments.

#### *The Structure of the Vitreous Body derived from Optical Phenomena.*

On this head the author arrives at the following conclusions.

In the vitreous body are innumerable vesicular globules, ranging in size from 0.0008 to 0.005 of a line, which are arranged in unbroken series, in tubes more or less transparent. These tubes precisely resemble veins and arteries in their mode of ramification; they frequently anastomose and are united to one another by capillary plexuses, and they are of less specific gravity than the vitreous fluid. The trunks of this peculiar system of vessels probably arise in the region of, or at the base of the optic nerve, and ramify in the vitreous humour; the larger branches passing circumferentially within a limited distance of the hyaloid membrane, and yielding again many branches, which, after repeated subdivisions, end in a capillary network exceedingly subtle and close. Many of the terminal loops of the capillaries are attached to the hyaloid membrane, so as to confine the majority of the branches in a lax manner to its vicinity. A true



idea of this system may be gained by conceiving that the veins and arteries here existing in the fœtal eye have in after life been developed according to the growth of the body, but also metamorphosed into these light, peculiar, globule-holding, transparent vessels, and deprived of all foreign support except at their roots and a part of their capillary loops. The intricate ramifications of these vessels have the mechanical effect of in a great degree restraining the relative motion of the humour which fills the hyaloid capsule, and compelling it to concur in the various movements of the eye-ball, so as to obviate the risk of concussion from eddies of the fluid in rapid movements of the eye, and consequent disturbing effects on the lens, the retina and its vessels.

The paper goes on to take this subject up in detail; supplies the dynamical laws which must be kept in view in the application of previously obtained optical methods to the required examination; shows that it is the system of ramifications described which has given rise to the peculiar appearances simulating concentric lamellæ in the vitreous humour previously subjected to chromic acid, so differently interpreted by microscopists. Here too the hitherto vagrant *muscæ volitantes* are, for the first time, invested with form, disposition and office. They are now shown to be the essential element in the structure of the vitreous body; and certain radical misconceptions, as to the nature of these appearances and the constitution of the vitreous body, are pointed out.

#### *The Optical Anatomy of the Retina.*

The existence of the *vasa centralia retinæ* in the substance of the retina, and the movements of the blood therein, occasion diversified phænomena. We may examine these vessels in our own eyes, in their minutest distributions, by means of a pin-hole, lens, &c., *in movement across the eye's axis*, in virtue of a physiological law hereafter determined. Currents of blood in these vessels, by pressure upon the nervous matter *at their sides*, produce remarkable phænomena, differing for the superficial and deep vessels (that is, according to the place of the vessel in the five layers of the retina lately discovered by microscopists). These phænomena may all be distinguished from one another, and assigned *with precision* each to its cause. The phænomena of this kind are *always* before us by daylight and night. In every use of the organs of sight these effects may be observed. In twilight, and *into* night, the pressure of the blood-currents on the retina first equals and then *excels* the impression made by the *fading* external light; and the whole circulatory system may be seen, with proper attention, definitely figuring itself in white or golden colour. A great concentration of light appears at the middle of the retina, which requires a bountiful supply of blood, and owing to the pointing of vessels towards the *foramen centrale*, there is an apparent gyration of *light* currents round a darker pivot. The whole conduct of the retinal circulation may be traced by the *blood-light*. And the manner in which the blood flows through the retina may be equable, or irregular and fitful; it may be very slow,

and it may roll with great rapidity. A rhythmical or recurrent circulation of the retinal blood is very frequent, and produces very singular phenomena. We may produce the uncommon states of the retinal circulation at pleasure, by artifices described; and it is shown that it is the retinal circulation which is the cause of all the phenomena which have been taken to prove spontaneous, vibratory, &c. sensations of light.

From these elementary facts being overlooked, fundamental errors as to the conduct of the retina proper have prevailed on all hands. When external light is so faint that the retinal light from blood-pressure exceeds it, *the middle of the retina is so occupied with retinal light* as to be, comparatively with other parts of the retina, unavailable for the usual purposes, and we do not see *anything* with direct nearly so well as with oblique vision; and this inefficiency of the centre of the retina is not limited to the case of "stars of the last degree of faintness" (Herschel and South), but all small objects that are quite visible by "lateral" inspection appear to be "suddenly blotted out" by the eyes being turned directly upon them.

The rhythmical waves of light, or rhythmical progression of the retinal blood (and the mode of movement of the retinal blood as rendered by optical phenomena can be observed by other means), may occur, in a certain sense, spontaneously, or may be produced at will. The retinal circulation may be excited to show astonishing luminous effects.

Among other ways of causing a rhythmical or recurrent movement of this blood, is that by simple fatigue of the retina by overstraining the sight, when the retina, more or less suddenly (or after a few oscillations), becomes flooded with blood, and complete obliteration of all objects having less than a certain luminosity ensues. This circumstance has misled Brewster and Purkinje, separately, into the belief that they had discovered that a sensation excited in one portion of a retina may be "extended" or "irradiated" to an adjacent portion. Other cases which are imagined by J. Müller and Brewster to support this view are subjected to examination; the real cause of each of the phenomena mentioned being pointed out. Some peculiar effects of retinal light are given; and it is determined that *the rigid correspondence of the limits of sensation with those of the painted image*, is a physiological law literally *absolute*.

Unsuspected difficulties of a solitary eye, and certain well-known phenomena are explained upon the foregoing principles.

*May sensation be excited in the trunk of the optic nerve, or centrifugally?*

The arguments which have been presumed to prove the affirmative are shown, one by one, to be fallacious, while there is presumption of a negative sort. Observations are offered as to the correct explanation of various physiological points which have been otherwise interpreted, and reputed physiological contrasts of colour are considered.

*Images of external objects are painted on the limitary membrane, and perceived by the radial fibres.*

This head commences with the quotation of a passage from Sir David Brewster's 'Optics' which he offered towards an explanation of the difficulty of seeing a very faint star by direct vision; and it is shown that the retina is not liable, as Brewster imagines, to be thrown into a state of "undulatory" perception by our looking through the teeth of a "fine comb" or through a single "narrow aperture." The paper points out that the effect observed in these circumstances is produced by our looking near the edge of any body whatever, provided, *and only then*, that the object *move*, be it never so little, *across the eye's axis*. It shows that the same effect is produced by light radiating from a point, by a flame, by lenses, curved reflectors, *whilst they are in the act of moving across* the eye's axis; or by the movement of the eye itself, merely in relation to the light entering it,—even the naked eye along the sky. The effect produced is shown to be simply owing to this; that the retina, under such action, ceases to perceive in the spaces corresponding to its blood-vessels and capillaries, so that they completely display themselves in the semblance of black bodies (or lines); and the law is arrived at, that the images of external points which are painted on the vessels and capillaries are not perceived when the retina *loses light from one point of space and receives light from another point of space within a certain interval of time*, or that the percipient points lying in front of the vessels *require a certain time to perceive*. A physiological hypothesis is suggested to account for this phenomenon, on the presumption that the "radial fibres," which project from the layer of rods and cones and end in the limentary membrane, are the ultimate percipients of light.

It is pointed out how wonderfully close we may find the correspondence between the microscopical and optical anatomy of the retina. Each pair of identical fibres of the two optic nerves must be regarded as one nerve. Another supposed anomaly to the simplicity of nervous action being explained on anatomical principles, a statement of ordinary optical nervous action is made, and a summary evinces how the anomalies in visual experience are due to the complex additions to a simple organ of sight.

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#### ROYAL INSTITUTION OF GREAT BRITAIN.

Feb. 2, 1855.—"On the Pendulum-experiments lately made in the Harton Colliery, for ascertaining the mean Density of the Earth." By G. B. Airy, Esq., F.R.S., Astronomer Royal.

The Lecturer commenced with remarking that the bearing of the experiments, of which he was about to give a notice, was not limited to their ostensible object, but that it applied to all the bodies of the solar system. The professed object of the experiments was to obtain a measure of the density of the earth, and therefore of the mass of the earth (its dimensions being known); but the ordinary data of astronomy, taken in conjunction with the laws of gravitation, give the relative proportions of the mass of the earth to the masses of the sun and the principal planets; and thus the determination of the

absolute mass of the earth would at once give determinations of the absolute masses of the sun and planets. To show how this proportion is ascertained, it is only necessary to remark, that a planet, if no force acted on it, would move in a straight line; that, therefore, if we compute geometrically how far the planet moves in a short time, as an hour, and then compute the distance between the point which the planet has reached in its curved orbit, and the straight line which it has left, we have found the displacement which is produced by the sun's attraction, and which is therefore a measure of the sun's attraction. In like manner, if we apply a similar calculation to the motion of a satellite during one hour, we have a measure of the attraction of its primary. The comparison of these two gives the proportion of the attraction of the sun, as acting upon a body, at one known distance, to the attraction of a planet, as acting upon a body at another known distance. It is then necessary to apply one of the theorems of the laws of gravitation, namely, that the attraction of every attracting body is inversely as the square of the distance of the attracted body, and thus we obtain the proportion of the attractions of the sun and a planet, when the bodies upon which they are respectively acting are at the same distance from both: and finally, it is necessary to apply another theorem of the law of gravitation, namely, that the attractions thus found, corresponding to equal distances of the attracted bodies, are in the same proportion as the masses of the attracting bodies (a theorem which applies to gravitation, but does not apply to magnetic and other forces). Into the evidence of these portions of the law of gravitation, the Lecturer did not attempt to enter: he remarked only that they rest upon very complicated chains of reasoning, but of the most certain kind. His only object was to show that the proportion of the masses of all bodies, which have planets or satellites revolving round them, can easily be found (the proportion for those which have no satellites is found by a very indirect process, and with far less accuracy); and that if the absolute mass of the earth be known, the absolute mass of each of the others can be found. As their dimensions are known, their densities can then be found. Thus it rests upon such inquiries as those on which this lecture is to treat, to determine (for instance) whether the planet Jupiter is composed of materials as light as water or as light as cork.

The obvious importance of these determinations had induced philosophers long since to attempt determinations of the earth's density: and two classes of experiments had been devised for it.

The first class (of which there was only one instance) is the attraction of a mountain, in the noble Schhallien experiment. It rests, in the first place, upon the use of the zenith sector; and, in the next place, upon our very approximate knowledge of the dimensions of the earth. [The construction of the zenith sector was illustrated by a model: and it was shown, that if the same star were observed at two places, the telescope would necessarily be pointed in the same direction at the two places, and the difference of direction of the plumb-line, as shown by the different points of the

graduated arc which it crossed at the two places, would show how much the direction of gravity at one place is inclined to the direction of gravity at the other place.] Now from our knowledge of the form and dimensions of the earth, we know that the direction of gravity changes very nearly one second of angle for every 100 feet of horizontal distance. Suppose then that two stations were taken on Schehallien, one on the north side and the other on the south side, and suppose that their distance was 4000 feet; then, if the direction of gravity had not been influenced by the mountain, the inclination of the directions of gravity at these two places would have been about 40 seconds. But suppose, on applying the zenith sector in the way just described, the inclination was found to be really 52 seconds. The difference, or 12 seconds, could only be explained by the attraction of the mountain, which, combined with what may be called the natural direction of gravity, produced directions inclined to these natural directions. In order to infer from this the density of the earth, a calculation was made (founded upon a very accurate measure of the mountain) of what would have been the disturbing effect of the mountain if the mountain had been as dense as the interior of the earth. It was found that the disturbance would have been about 27 seconds. But the disturbance was really found to be only 12 seconds. Consequently the proportion of the density of the mountain to the earth's density was that of 12 to 27, or 4 to 9 nearly. And from this, and the ascertained density of the mountain, it followed that the mean specific gravity of the earth would be about five times that of water. The only objection to this admirable experiment is, that the form of the country near the mountain is very irregular, and it is difficult to say how much of the 12 seconds is or is not really due to Schehallien.

The second class is what may be called a cabinet experiment, possessing the advantage of being extremely manageable, and the disadvantage of being exceedingly delicate, and liable to derangement by forces so trifling that they could with difficulty be avoided. Two small balls upon a light horizontal rod were suspended by a wire, or two wires, forming a torsion balance, and two large leaden balls were brought near to attract the small balls from the quiescent position. We could make a calculation of how far the great balls would attract the little ones, if they were as dense as the general mass of the earth; and comparing this with the distance to which the leaden balls really do attract them, we find the proportion of the density of the earth to the density of lead. The peculiar difficulty and doubt of the results in this experiment depend on the liability to disturbances from other causes than the attraction of the leaden balls, especially the currents of air produced by the approach of bodies of a different temperature; and after all the cautions of Cavendish, Reich and Baily, in their successive attempts, it seems not impossible that the phænomena observed may have been produced in part by the temperature of the great balls as well as their attraction.

These considerations induced the Lecturer, in 1826, to contem-

plate a third class of experiments, namely, the determination of the difference of gravity at the top and the bottom of a deep mine, by pendulum experiments. Supposing the difference of gravity found, its application to the determination of density (in the simplest case) was thus explained. Conceive a spheroid, concentric with the external spheroid of the earth, to pass through the lower station in the mine. It is easily shown that the attraction of the shell included between these produces no effect whatever at the lower station, but produces the same effect at the upper station as if all its matter were collected at the earth's centre. Therefore, at the lower station we have the attraction of the interior mass only: at the upper station we have the attraction of the interior mass (though at a greater distance from the attracted pendulum) and also the attraction of the shell. It is plain that by making the proportion of these theoretical attractions equal to the proportion actually observed by means of the pendulum, we have the requisite elements for finding the proportion of the shell's attraction to the internal mass's attraction, and therefore the proportion of the matter in the shell to the matter in the internal mass; from which the proportion of density is at once found. Moreover, it appeared probable, upon estimating the errors to which observations are liable, that the resulting error in the density, in this form of experiment, would be less than in the others.

Accordingly, in 1826, the Lecturer, with the assistance of his friend Mr. Whewell (now Dr. Whewell), undertook a series of experiments at the depth of nearly 1200 feet, in the Dolcoath mine, near Camborne, in Cornwall. The comparison of the upper and lower rocks (to which further allusion will be made) was soon found to be the most serious difficulty. The personal labour was also very great. They had, however, made a certain progress when, on raising a part of the instruments, the straw packing took fire (the origin of the fire is still unknown), and partly by burning and partly by falling, the instruments were nearly destroyed.

In 1828 the same party, with the assistance of Mr. Sheepshanks and other friends, repeated the experiment in the same place. After mastering several difficulties, they were stopped by a slip of the solid rock of the mine, which deranged the pumps and finally flooded the lower station.

The matter rested for nearly twenty-six years, the principal progress in the subjects related to it being the correction to the computation of "buoyancy" of the pendulum, determined by Colonel Sabine's experiments. But in the spring of 1854, the manipulation of galvanic signals had become familiar to the Astronomer Royal and the Assistants of the Greenwich Observatory, and it soon occurred to him that one of the most annoying difficulties in the former experiment might be considered as being practically overcome, inasmuch as the upper and lower clocks could be compared by simultaneous galvanic signals. Inquiries, made in the summer, induced him to fix on the Harton Colliery near South Shields, where a reputed depth of 1260 feet could be obtained; and as soon as this

selection was known, every possible facility and assistance were given by the owners of the mine. Arrangements were made for preparing an expedition on a scale sufficient to overcome all anticipated difficulties. A considerable part of the expense was met by a grant from the Board of Admiralty. The Electric Telegraph Company, with great liberality, contributed (unsolicited) the skill and labour required in the galvanic mountings. The principal instruments were lent by the Royal Society. Two observers were furnished by the Royal Observatory, one by the Durham Observatory, one by the Oxford Observatory, one by the Cambridge Observatory, and one by the private observatory of Red Hill (Mr. Carrington's). Mr. Dunkin, of the Royal Observatory, had the immediate superintendence of the observations.

The two stations selected were exactly in the same vertical, excellently walled, floored, and ceiled; the lower station, in particular, was a most comfortable room, or rather suite of rooms. Every care was taken for solidity of foundation and steadiness of temperature. In each (the upper and the lower) was mounted an invariable brass pendulum, vibrating by means of a steel knife edge upon plates of agate, carried by a very firm iron stand. Close behind it, upon an independent stand, was a clock, carrying upon the bob of its pendulum an illuminated disc, of diameter nearly equal to the breadth of the tail of the invariable pendulum; and between the two pendulums was a chink or opening of two plates of metal, which admitted of adjustment, and was opened very nearly to the same breadth as the disc. To view these, a telescope was fixed in a wall, and the observer was seated in another room. When the invariable pendulum and the clock pendulum pass the central points of vibration at the same instant, the invariable pendulum hides the illuminated disc as it passes the chink, and it is not seen at all. At other times it is seen in passing the chink. The observation, then, of this disappearance determines a coincidence with great precision. Suppose the next coincidence occurs after 400 seconds. Then the invariable pendulum (swinging more slowly) has lost exactly two swings upon the clock pendulum, or the proportion of its swings to those of the clock pendulum is 398 : 400. If an error of a second has been committed, the proportion is only altered to 397 : 399, which differs by an almost insignificant quantity. Thus the observation, in itself extremely rude, gives results of very great accuracy. As the proportion of invariable-pendulum-swings to clock-pendulum-swings is thus found, and as the clock-pendulum-swings in any required time are counted by the clock dial, the corresponding number of invariable-pendulum-swings is at once found. Corrections are then required for the expansion of the metal (depending on the thermometer-reading), for the arc of vibration, and for the buoyancy in air (depending on the barometer-reading).

But when the corrected proportion of upper-invariable-pendulum-swings to upper-clock-pendulum-swings is found, and the proportion of lower-invariable-pendulum-swings to lower-clock-pendulum-

swings is found, there is yet another thing required:—namely, the proportion of upper-clock-pendulum-swings to lower-clock-pendulum-swings in the same time; or, in other words, the proportion of the clock rates. It was for this that the galvanic signals were required. A galvanometer was attached to each clock, and an apparatus was provided in a small auxiliary clock, which completed a circuit at every 15 seconds nearly. The wire of this circuit, passing from a small battery through the auxiliary clock, then went through the upper galvanometer, then passed down the shaft of the mine to the lower galvanometer, and then returned to the battery. At each galvanometer there was a small apparatus for breaking circuit. At times previously arranged, the circuit was completed by this apparatus at both stations, and then it was the duty of the observers at both stations to note the clock times of the same signals; and these evidently give comparisons of the clocks, and therefore give the means of comparing their rates. Thus (by steps previously explained) the number of swings-made by the upper pendulum is compared with the number of swings made in the same time by the lower pendulum.

Still the result is not complete, because it may be influenced by the peculiarities of each pendulum. In order to overcome these, after pendulum A had been used above and pendulum B below, they were reversed; pendulum B being observed above, and A below; and this, theoretically, completes the operation. But in order to ensure that the pendulum received no injury in the interchange, it is desirable again to repeat the experiments with A above and B below, and again with B above and A below.

In this manner the pendulums were observed with 104 hours of incessant observations, simultaneous at both stations, A above and B below; then with 104 hours, B above and A below; then with 60 hours, A above and B below; then with 60 hours, B above and A below. And 2454 effective signals were observed at each station.

The result is, that the pendulums suffered no injury in their changes; and that the acceleration of the pendulum on being carried down 1260 feet is  $2\frac{1}{4}$  seconds per day, or that gravity is increased by  $-\frac{1}{19190}$  part.

It does not appear likely that this determination can be sensibly in error. The circumstances of experiment were, in all respects, extremely favourable; the only element of constant error seems to be that (in consequence of the advanced season of the year) the upper station was cooler by  $7^\circ$  than the lower station, and the temperature-reductions are therefore liable to any uncertainty which may remain on the correction for  $7^\circ$ . The reductions employed were those deduced by Sabine from direct experiment, and their uncertainty must be very small.

If a calculation of the earth's mean density were based upon the determination just given, using the simple theory to which allusion is made above, it would be found to be between six times and seven times the density of water. But it is necessary yet to take into



account the deficiency of matter in the valley of the Tyne, in the hollow of Jarrow Slake, and on the sea-coast. It is also necessary to obtain more precise determinations of the specific gravities of the rocks about Harton Colliery than have yet been procured. Measures are in progress for supplying all these deficiencies. It seems probable that the resulting number for the earth's density will probably be diminished by these more accurate estimations.

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## XLVI. *Intelligence and Miscellaneous Articles.*

ON THE ARTIFICIAL PRODUCTION OF MINERALS BELONGING TO THE FAMILY OF SILICATES AND ALUMINATES BY THE REACTION OF VAPOURS UPON ROCKS. BY M. DAUBRÉE.

MODERN geology admits as proved, that the rocks in contact or in the neighbourhood of the massive crystalline strata have undergone some modification; to this theoretical idea they owe their name of *metamorphic rocks*.

Ever since the remarkable experiments of James Hall, an elevated temperature has been admitted as the principal agent of metamorphism. Nevertheless the intervention of heat alone cannot explain all the details of the extraordinary modifications which rocks have undergone in different countries; complex chemical actions have also evidently contributed to the alteration of the primitive type.

In some previous researches presented to the Academy, I paid attention principally to the reproduction of the minerals peculiar to the stanniferous deposits and the reciprocal action of the vapours. The new experiments, of which the present memoir contains the results, start from the same theoretical idea, of which they extend the application to a series of crystalline rocks.

When chloride of silicium in a state of vapour and at a red heat acts upon the bases which enter into the composition of rocks, it is decomposed, forming chloride of calcium and silicic acid. Sometimes this acid remains free, sometimes it combines with the excess of the base and forms simple or complex silicates. The most remarkable point connected with this reaction in a chemical, and especially in a geological point of view, is that the silicic acid thus formed, and the silicates produced from it, have an extreme tendency to crystallization. The crystals are small, but generally very distinct. It must be observed, moreover, that the crystallization of these compounds takes place at a temperature far below their point of fusion.

With lime, magnesia, alumina and glucina, *crystallized quartz* is obtained in the ordinary form of the pyramidal hexagonal prism, and a portion of the base passes into the state of silicate. In this manner the silicate of lime called *Wollastonite* is obtained in the form of rhombic tables, with two broad bevelled faces replacing the obtuse angles, which is the usual form of the native crystals. These tables

are often grouped perpendicularly to each other, like the prisms of *staurolite*. In the same way, with magnesia, *peridote* is obtained in rectangular prisms,

Alumina gives a silicate in elongated prisms, with oblique bases, which are not attacked by acids, are infusible, and possess all the characters of *disthene*. It is very interesting in this case to see chloride of aluminium formed at the expense of the silicium.

To form a double or multiple silicate, it is necessary not only to mix the bases in suitable proportions, but also to furnish the oxygen necessary for the formation of the silicic acid, by the addition of an excess of one of the bases or of lime.

A mixture of lime and magnesia furnishes crystals of *diopside* perfectly colourless and transparent; they present the broad truncation and the bevel usual with *augite*. Seven equivs. of potash or soda and one equiv. of alumina, or one equiv. of alkali, one equiv. of alumina, and six equivs. of lime, produce by the action of chloride of silicium, crystals in oblique prisms with obtuse bevelments, which are scarcely acted upon by acids, and fuse before the blowpipe, presenting, in fact, all the characters of the *feldspaths*.

By the same process, varying the proportions and the nature of the bases submitted to the chloride of silicium, I obtained silicates presenting the crystallographic characters of *willemite*, *idocrase*, *garnet*, *phenakite*, *emerald*, *euclase* and *zircon*.

By mixing the elements corresponding with the compositions recently given by Rammelsberg for the magnesian and ferromagnesian *tourmalines*, adding an excess of magnesia or lime to furnish oxygen to the silicium, I obtained in the midst of crystals of quartz some very distinct hexagonal prisms, presenting in other respects all the external and chemical characters of *tourmaline*.

Chloride of aluminium may be used in the same manner as chloride of silicium. When passed over lime at a red heat, it produces chloride of calcium and aluminium, in crystals belonging to two types proper to *corundum*—the prism, and the acute double pyramid.

The same reaction takes place with magnesia; and besides, in this latter case, a portion of the regenerated alumina may combine with the excess of magnesia, so as to produce *spinel*, recognizable by its crystals in regular octahedra truncated at the edges. Nevertheless it is preferable, in order to obtain *spinel*, to submit a mixture of chlorides of aluminium and magnesium in contact with lime to a red heat. With chlorides of zinc and aluminium, we obtain zinciferous *spinel* or *gahnite*.

Chloride of titanium conveyed over lime, furnishes, along with some other crystals which will be studied hereafter, oxide of titanium in the form of *brookite*. Oxide of tin, obtained in a similar manner, is in crystals of the same form as that which I had previously produced by the action of aqueous vapour. Thus the rectangular prismatic form is persistent for the acids of titanium and tin, produced by the decomposition of the chlorides of these metals, at temperatures included between 572° and 1652° F.

By the reaction of perchloride of iron upon lime I have obtained hæmatite, either in very distinct specular crystals like those of Saint Gothard, or in transparent hexagonal laminae, presenting by refraction a ruby-red colour. Perchloride of iron mixed with chloride of zinc, furnishes, under the same conditions, a crystalline compound analogous to *franklinite*.

Lastly, crystallized magnesia, or the *periclase* of La Somma, may easily be obtained by the reaction of lime upon chloride of magnesium, which is found amongst the abundant chlorinated vapours of the fumaroles of Vesuvius. The same chloride, decomposed by aqueous vapour, also furnishes periclase, and chloride of zinc gives crystalline oxide of zinc.

The results just described lead to geological consequences to which I can only refer very briefly in this place. I do not pretend to say that all the silicates composing the mass of the crystalline rocks are formed by vapours. But even in the midst of the fused rocks of Vesuvius, a certain number of minerals, to which M. Scacchi has recently directed attention, are met with which appear to be products of sublimation.

Amongst the minerals of the oldest formation, there are many also which could not by fusion have lined the fissures in which they are now found so well isolated; such are the diopside pyroxene, with garnet of Piedmont and the Ural, the adular feldspaths and the periclase of the Alps, the epidotes and axinites of l'Oisans, and many others.

The great richness of the crystalline limestones in minerals often foreign to the neighbouring rocks, cannot result entirely from the fact that the lime, by reacting upon the silica, has served for the formation of peculiar silicates. Whatever might have been the original impurities of the limestones, corundum, spinel, periclase, or chondrodite could not have been produced in them without the subsequent introduction of foreign chemical agents.

All these various products of transport,—silicates, aluminates, oxides, and other combinations, formed either in fissures or in the midst of rocks now become very compact, are explained, it appears to me, in the most satisfactory manner by the intervention of chlorinated or fluorated emanations. Besides, in the case of such volatile and penetrating compounds, there is nothing to oppose the idea that their action may have extended itself over spaces of considerable thickness from their centre of disengagement, as in the crystalline schistose rocks of the Alps or of Brazil. Sometimes the substitution of the silicates thus formed has only been partial, as in many of the crystalline limestones, which remain as perpetual witnesses of the ancient exhalations which have escaped from the neighbouring eruptive rocks. Sometimes the attack has been more complete, and the primitive mass has even disappeared in the state of soluble chloride, just like the water which has formed the specular iron ore of the volcanos.

If we turn to the crystalline limestones and dolomites most largely

furnished with minerals, such as those of Saint Gothard, Sweden, Finland, or the United States, we find that the introduction of chlorides, mixed with fluorides and sometimes with sulphuretted compounds, accounts for the formation of their most characteristic minerals. In this explanation we must include the rich deposits of red oxide of zinc with franklinite in New Jersey, as well as different masses of specular iron, and protoxide of iron, which have also been produced in the limestone.

We find magnesian compounds, such as spinel, chondrodite, mica, pyroxene, amphibole, warwickite, and serpentine accumulated, sometimes in large quantities, in limestones which contain no magnesia. This fact, hitherto unexplained, would only be a consequence of the different chemical affinities of lime and magnesia, for throughout our experiments we see chloride of magnesium precipitated by lime; and when these bases are both in the presence of chloride of silicium or aluminium, the lime yields its oxygen, and the magnesia remaining in the state of oxide enters by preference into an oxidized combination with the regenerated silica or alumina. The same principle explains the presence of magnesia, with exclusion of lime, in the protoxidized irons. Are we to attribute to the same cause the preponderance of magnesia over lime in the elements of granite and in serpentine?

The mode in which quartz and the silicates are connected principally with the granite rocks, has long been a difficulty in all the hypotheses upon the formation of the strata called primitive. Now we find in our experiments that quartz crystallizes at the same time, or even later than the silicates, at a temperature scarcely exceeding a cherry-red heat, and consequently infinitely below its point of fusion.

Is not this also the cause which appears sometimes to protect quartz from the influence of the basic silicates or of the aluminates, as in granite when it envelopes crystals of cymophane instead of forming a double silicate, like emerald or euclase?

If mica still gives off, by heat, fluorides of silicium, boron or lithium, shall we venture to affirm that the granitic pastes have not also originally contained chlorides of silicium, boron or aluminium? These are deficient, it is true, amongst the vapours collected in the present day in the neighbourhood of the apertures of volcanos, where they are decomposed and precipitated by aqueous vapour in contact with the atmosphere, but where they very probably contribute to the formation of the silicates, already considered by the best observers as products of volatilization. Do we not also find chlorine in considerable quantity in certain masses, such as the zirconian syenite of Norway, and the rock of Ilmen (miascite), where this body is principally combined in *elaolite*, and where it appears to have carried with it zirconium, tantalum, and all that series of rare elements which belong to these rocks?

It has never been shown that the presence of a certain quantity of water is an obstacle to such reactions at a high temperature, since we find silica and alumina separate, in an anhydrous state, from a watery

solution at a temperature of 572° to 752° F. And if hitherto the experiments have principally borne upon the *extreme conditions* of the different modes of formation, by the dry and humid way, a similar effect produced in these extreme states, such as quartz and corundum, may perhaps authorize us in concluding that it would also take place in the intermediate conditions.—*Comptes Rendus*, July 17, 1854, p. 135.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1855.

*Chiswick*.—February 1. Clear: overcast. 2. Drifting snow. 3. Hazy. 4. Dense fog: foggy throughout. 5. Overcast: drizzly. 6. Snow-flakes: slight rain. 7. Cloudy: clear and frosty. 8. Snowing: drifting snow: boisterous at night. 9. Overcast: sharp frost. 10. Clear and frosty: severe frost at night. 11. Thermometer within 1 degree of zero: clear. 12. Overcast: clear and cold: partially overcast. 13. Snowing: cloudy: clear: intense frost: thermometer at zero. 14. Clear and cold. 15. Snow-flakes: bright sun: frosty haze. 16. Snow-showers: overcast. 17. Snowing, with cold wind: clear: severe frost. 18. Frosty: hazy: severe frost at night. 19. Uniform haze: clear and frosty. 20. Clear and frosty. 21. Overcast. 22. Hazy. 23. Overcast: snowing. 24. Fine. 25. Overcast. 26. Rain. 27. Foggy: rain. 28. Drizzly: large halo round the moon in the evening.

Mean temperature of the month .....	28°·01
Mean temperature of Feb. 1854 .....	37°·67
Mean temperature of Feb. for the last twenty-nine years ...	39°·07
Average amount of rain in Feb. ....	1·54 inch.

*Boston*.—Feb. 1. Cloudy. 2. Fine. 3. Cloudy: rain and hail P.M. 4. Foggy: rain P.M. 5. Foggy: rain A.M. 6, 7. Cloudy: snow P.M. 8. Cloudy: stormy P.M. 9—11. Cloudy. 12—14. Fine. 15. Fine: snow A.M. 16. Fine: thermometer early A.M. 6°·5. 17—22. Fine. 23. Cloudy. 24. Fine: snow A.M. 25. Cloudy. 26. Cloudy: rain P.M. 27. Cloudy: snow A.M. 28. Cloudy.

*Sandwich Manse, Orkney*.—Feb. 1. Damp A.M.: sleet-showers P.M. 2. Thaw, bright A.M.: clear, frost P.M. 3. Thaw, showers A.M.: damp P.M. 4. Thaw, damp A.M.: showers P.M. 5. Showers A.M. and P.M. 6. Sleet-showers A.M.: clear P.M. 7. Clear, frost A.M.: snow-showers P.M. 8. Bright, frost A.M.: cloudy, frost P.M. 9. Clear, frost A.M.: cloudy, frost P.M. 10. Cloudy A.M. and P.M. 11. Snow-showers A.M. and P.M. 12. Snow-showers A.M.: snowing, aurora P.M. 13. Snow-drift A.M.: clear P.M. 14. Snow, bright A.M.: clear, aurora P.M. 15. Snow, bright A.M.: cloudy P.M. 16. Snow, bright A.M.: clear P.M. 17. Snow, thaw A.M.: cloudy P.M. 18. Snow, clear A.M.: clear P.M. 19. Snow, clear A.M.: cloudy P.M. 20, 21. Snow, clear A.M.: clear, aurora P.M. 22. Snow, clear A.M.: lunar halo P.M. 23. Snow, cloudy A.M.: snow drift P.M. 24. Snow drift A.M. and P.M. 25. Snow, cloudy A.M.: snow, clear P.M. 26. Snow-showers A.M.: snow, clear P.M. 27. Snow, bright A.M.: snow clouds P.M. 28. Thaw A.M.: rain P.M.

Mean temperature of Feb. for twenty-eight previous years .	38°·24
Mean temperature of this month .....	31°·64
Mean temperature of Feb. 1854 .....	39°·22
Average quantity of rain in Feb. for fourteen previous years	3·39 inches.

The mean temperature of this month is lower than that of any month for the last twenty-eight years—the whole period of observation—except February 1838, when it was 31°·31, and when there was snow during all the month and for three weeks previously. This month it lay from the 11th till the last day, and the drift on the 23rd and 24th formed high wreaths in many places, rendering the roads impassable to vehicles.

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

Days of Month.	Barometer.				Thermometer.				Wind.			Rain.		
	Chiswick.		Orkney, Sandwick.		Chiswick.		Orkney, Sandwick.		Chiswick, 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
	Max.	Min.	9½ a.m.	8½ p.m.	Max.	Min.	9½ a.m.	8½ p.m.						
1855.														
1.	30'002	29'786	29'60	30'22	33	17	30	35	ne.	ene.	w.	.....	.....	'08
2.	29'990	29'752	29'85	30'21	32	27	27	37½	e.	ene.	e.	.....	.....	'08
3.	29'510	29'429	29'38	29'73	39	23	34	35	e.	enc.	c.	.....	.....	'10
4.	29'384	29'229	29'13	29'60	39	33	32'5	36½	e.	ne.	e.	'10	'10	'03
5.	29'308	29'287	29'03	29'76	40	31	35'5	36	sw.	n.	nne.	'01	'73	'04
6.	29'308	29'318	29'20	29'86	35	29	33	36½	nc.	n.	nne.	'02	'31	'03
7.	29'811	29'744	29'55	30'09	36	27	33	35	ne.	nne.	nne.	.....	'15	'04
8.	29'792	29'662	29'64	30'24	32	27	31	33½	e.	e.	sse.	.....	'11	'03
9.	29'883	29'871	29'68	30'34	30	15	28'5	31½	ne.	e.	.....	.....	.....	.....
10.	29'867	29'635	29'64	29'89	30	1	27	37½	ne.	ene.	ne.	.....	.....	'01
11.	29'534	29'472	29'35	29'83	35	20	24	34½	ne.	nne.	nne.	.....	.....	.....
12.	29'481	29'465	29'30	29'70	36	23	28	33	ne.	ene.	ne.	.....	.....	'12
13.	29'400	29'356	29'22	29'58	29	0	28	29	ne.	calm	.....	.....	.....	'15
14.	29'925	29'922	29'10	29'69	36	17	21'5	26	n.	n.	e.	.....	.....	.....
15.	29'925	29'699	29'49	29'87	34	10	22	25	n.	calm	sw.	.....	.....	.....
16.	29'954	29'811	29'72	29'88	32	19	29	31	ne.	se.	ssw.	.....	'09	'11
17.	29'897	29'869	29'70	29'74	27	2	24	35	nc.	ne.	whw.	.....	.....	'06
18.	30'010	29'980	29'80	29'95	29	3	12	34½	nc.	w.	nw.	.....	.....	'01
19.	29'966	29'883	29'77	30'01	33	20	24	31½	nc.	e.	nw.	.....	.....	'07
20.	29'732	29'691	29'58	29'91	35	8	23'5	28	e.	ne.	.....	.....	.....	.....
21.	29'855	29'837	29'60	30'00	30	10	24	31	ne.	nne.	ssw.	.....	.....	.....
22.	29'934	29'877	29'72	29'93	34	23	11	30	nc.	nhw.	e.	.....	.....	'03
23.	29'986	29'853	29'65	29'70	37	18	28	29½	nc.	s.	w.	.....	.....	'02
24.	30'007	29'391	29'70	29'37	44	33	37	33	sw.	ssw.	whw.	'60	'12	.....
25.	29'354	29'314	29'05	29'46	49	37	37	33	sw.	n.	n.	'10	'15	.....
26.	29'532	29'469	29'25	29'66	40	32	35	30	nw.	nc.	nw.	'04	'15	.....
27.	29'811	29'602	29'50	29'56	42	37	33	27	s.	s.	se.	'06	'13	.....
28.	29'777	29'737	29'43	29'17	50	29	36	38½	sw.	s.	sse.	'13	'29	.....
Mean.	29'738	29'619	29'48	29'820	35'64	29'39	27'7	32'44	.....	.....	.....	1'35	2'18	1'32

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AND  
JOURNAL OF SCIENCE.

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[FOURTH SERIES.]

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MAY 1855.

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XLVII. *On the Interference of Light near a Caustic, and the Phenomena of the Rainbow.* By PROFESSOR POTTER, A.M.\*

AT the meeting of the British Association for the Advancement of Science at Cambridge in 1833, I exhibited the experiment of the interference of light near a caustic, which I had a short time before discovered when examining the circle of aberration of a *spherical* mirror for a reflecting microscope. This circle of aberration was formed by using a luminous point, which was an image of the sun given by a small globule of mercury, for an object of which the image produced by the spherical mirror was to be examined. In place of the circle of aberration being a disc brightest at the edge and shading away gradually towards the centre, as we should expect from geometrical optics, it appeared a magnificent phænomenon of the interference of light, consisting of brilliant and black rings coloured on their edges.

The outer ring, instead of being brightest at its outer edge, as we should have expected from the undulatory theory, since the rays had travelled over equal spaces, shaded away gradually. I concluded that I had fallen upon a confirmation of what I had before found in experiments with the two mirrors slightly inclined, namely, that the central band of interference, where the rays have travelled over equal distances, was in normal circumstances a *black* band and not a *bright* one, as it had been asserted to be by the late M. Arago† and Professor Airy‡, in accordance with the undulatory theory of light.

\* Communicated by the Author.

† See *Annales de Chimie et de Physique* for 1819, vol. xi. p. 12.

‡ Tract on the Undulatory Theory of Optics, articles 51 and 54.

Whilst an undergraduate at Cambridge, on studying the theory of the rainbow I found that it belonged to the same class of interferences near a caustic, and was not aware, until informed by Professor Whewell, that Dr. Young had applied the principle of ray interference to explain the occurrence of supernumerary rainbows seen frequently within the primary bow, and sometimes exterior to the secondary bow, of which the Newtonian theory of the rainbow furnishes no account.

In a paper published in vol. vi. p. 140 of the Cambridge Transactions, I have shown the necessity of the doctrine of interference to solve the phenomena, and have traced the caustic for the primary bow. At the time of writing that paper in 1835, I had not entered into any adequate examination of the point whether the rainbow occurred at or within the caustic, but did so soon afterwards, and I urged that it was found within the caustic in a discussion in the Cambridge Philosophical Society in the spring of 1836. In the Philosophical Magazine for July 1838 will be found the calculations on which this conclusion was founded, showing that there was darkness at the place formerly attributed to the rainbow.

This has been fully confirmed by M. Bravais\* in numerous measures of the radii of fog-bows, which he found to be less than calculated for the rainbow, and to vary from  $34^{\circ}$  to  $42^{\circ}$ . Not being aware that the primary rainbow was also within its calculated place, he supposed that fog-bows required the occurrence of *vesicular* vapour for their formation. The existence of any such vapour has not, however, been demonstrated, and is generally disbelieved. His arguments from the absence of colour in the fog-bow, or *arc-en-ciel blanc*, are not of any weight as to its origin being different from that of the rainbow, for I believe the outer edge is always tinged with red.

Mr. Airy, the Astronomer Royal, has given an investigation in vol. vi. p. 379 of the Cambridge Transactions, of the intensity of light near a caustic, by employing M. Fresnel's principle, that each point in the front of a wave may be considered as the origin of a new wave, and the intensity at any given point is the resulting intensity from the surface of the wave in any of its previous positions considered as made up of such origins. This method of discussing interferences, which fails in so many cases when examined with impartial eyes, gave the maximum intensity of the primary bow to be within the caustic, but that the brightness was still nearly half the maximum at the place of the caustic, and that the light extended on its outside, shading gradually to darkness at an indefinite distance.

The measures before referred to were a sufficient guarantee

\* See *Répertoire d'Optique Moderne*, by the Abbé Moigno, p. 1610.



that this solution did not represent the natural phænomena. Professor Miller, however, undertook an experimental examination of the analogous phænomena, in M. Babinet's experiment, in which they are observed in a small cylindrical vertical stream of water, and he has published a paper in vol. vii. of the Cambridge Transactions, detailing the measures of some of the results. He found the light to extend outside the calculated place of the caustic, in accordance with Mr. Airy's investigation, and that the *dark bars* or minima near the caustic were nearly in the relative places as given by that investigation; but he has not given the places of the *bright bars* or maxima, which I find do not agree with their calculated places.

At that time I commenced a repetition of these experiments, and found like results with Professor Miller as to light extending outside the caustic. I found, however, as will be seen below, that the light extended further and further outside it as the velocity of the stream was greater. The discrepancy between these results and those from the natural phænomena evidently arose from the refractive index being slightly different, and a little less for water falling in such a stream and about to break into drops, from that for still water.

I did not at that time think of any method of experimenting with water at rest, which I now find is so easy to be employed, but I prepared a prism and small cylinder of plate-glass; the prism served to determine the refractive indices of the rays to be used, and the cylinder gave the irises in position in accordance with those from the natural phænomena of the rainbow, and not in accordance with those from the running stream. The experiments extended to within a few days of my leaving Cambridge to commence the duties of my Professorship at University College. They have been only resumed lately in consequence of my arriving at this part of physical optics in a general investigation of the whole science, which I hope before much longer time has elapsed to lay before the scientific public in a continuous treatise.

The following were obtained in Queens' College, Cambridge. A heliostat reflected the sun's light through a narrow vertical aperture, which falling at a distance of about 5 feet on a fine Munich prism with its edge vertical, furnished a very pure prismatic beam of light. Of this beam the rays from orange to azure were reflected at right angles by a plane metallic mirror across one room and into another which was dark, and there fell at about 41 feet from the prism on the small vertical stream of water. This stream of water was directly over the centre of the circle of an excellently divided protractor, which, being laid horizontal, had on the arms which carried the verniers a support

for a small telescope, of length  $4\frac{1}{2}$  inches, which magnified rather more than twice, and adjusted to show distant objects distinctly. It had micrometer lines at the common focus of the object and eye-glasses crossed at an angle of about  $35^\circ$ , the bisection of which was vertical. The image to be viewed in the telescope was brought to the bisection of this angle, and the measures were taken from the iris on one side of the perpendicular ray to that on the opposite side of it; the angle traversed being divided by two, gave the distance from the perpendicular ray for each iris.

On the 17th of September, 1841, in two good experiments with green light for the fixed line E, the extreme visible light was found to extend to  $42^\circ 21\frac{1}{2}'$  in one experiment, and to  $42^\circ 21'$  in the other, from the ray falling perpendicularly on the stream; and the calculated place of the primary rainbow, if it were at the caustic, for this colour is  $41^\circ 40'$ , therefore light was seen  $41'$  beyond the caustic.

In two experiments with orange light, fixed line D, the extreme visible light was found to extend to  $42^\circ 41'$  in one, and  $42^\circ 58\frac{1}{2}'$  in the other from the normal ray. The calculation for the caustic gives  $42^\circ 2'$  nearly, and hence the light was seen  $39'$  and  $56\frac{1}{2}'$  outside the caustic.

Another experiment with yellow green light between the lines D and E gave a distance  $42^\circ 33\frac{1}{2}'$ , to which the light could be traced. Measures of the first six maxima or bright bars were taken, and though probably correct, yet on returning to the first maximum for verification some derangement had occurred, so that no argument can be taken from them to have weight.

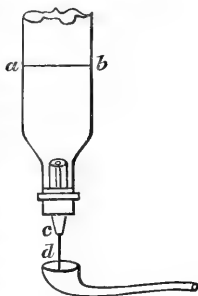
The stream was formed in passing through a small circular aperture of  $\frac{1}{25}$ th of an inch diameter, in a brass nozzle on a pipe furnished with a stopcock, which acted as a siphon from an elevated pneumatic trough, so that the velocity of the stream could be regulated by the stopcock.

On October 2, I tried the effect of different velocities on the position of the bars, and found with green light, when the interference bars were most distinct, the extreme visible light extended to a reading on the circle of  $269^\circ 9'$ , and when the velocity was greater, it extended to a reading of  $267^\circ 35'$ ; and from the nature of the reading, the light extended to  $1^\circ 34'$  further beyond the caustic in the latter than in the former case. In another trial with green light and the bars most distinct, the extreme visible light gave a reading of  $269^\circ 4'$ ; but with a greater velocity and the bars still visible, the reading was  $267^\circ 16'$ , or the difference was  $1^\circ 48'$ . When the full stream was allowed to run, the extreme light extended still further, but the bars were not seen.

These experiments, coupled with the measures of the natural

rainbow, lead us to conclude, that if the cylinder of water had been at rest, the extreme visible light might not have extended even up to the caustic.

In resuming the experiments lately, I have had as a reservoir for the water, a common glass bottle with the bottom broken off, as in the figure: a glass tube drawn with the blowpipe to a fine tapering aperture passed through the cork in its neck. When the water in the reservoir was at some height, as *a b*, and the stream *c d* was formed, it was received in a catch-tube, as in the figure, and led away. The part of *c d* which appeared cylindrical and without ripples, either from the lower end falling on the catch-tube, or from its breaking into drops, was adjusted so as to be over the centre of the horizontal protractor, and level with the axis of the telescope, carried on the arm of the vernier, before mentioned.



The stream of water, where used, was very nearly  $\frac{1}{30}$ th of an inch diameter, and its distance from the narrow slit of  $\frac{1}{100}$ th of an inch breadth between metallic edges, through which the sun's light was reflected by a mirror of a solar microscope fixed in the shutter of a dark room, was 22 feet.

Preliminary measures on the 5th of April last gave the distance from the perpendicular ray, to which the extreme red extended for the primary iris, with the height of the surface of the water in the reservoir  $4\frac{1}{2}$  inches above the aperture, equal to  $43^\circ 26'$ ; and with a height of water  $7\frac{1}{2}$  inches, equal to  $43^\circ 30'$ .

When the side of the tube had been oiled and the water had ceased running out, a drop was obtained hanging from the end of the tube, when readings for the extreme red were taken, which gave in one experiment the distance from the normal ray  $42^\circ 1'$ , and in another  $41^\circ 51\frac{1}{2}'$ . Calculating with the refractive index of the fixed ray B, the radius of the primary bow, if at the caustic, is  $42^\circ 22'$ . So that with the *vertical stream* the light is visible outside the caustic, and with the *stationary drop* it does not extend to it.

With the adjustments more perfect, I obtained on the 7th of April as follows:—

With $4\frac{1}{4}$ inches water pressure	the angle was	$43^\circ$	$3\frac{1}{2}'$
With average $7\frac{1}{4}$ inches	...	...	$43^\circ 17'$
...	$8\frac{7}{16}$	...	$43^\circ 24\frac{1}{2}'$
...	$8\frac{13}{16}$	...	$43^\circ 20\frac{1}{2}'$

I conclude that with this fine aperture the difference of the

pressures produces a slight but sensible effect upon the distance to which the light extends beyond the caustic.

The side of the tube being oiled, with a little care in adding small quantities of water after the stream had ceased to run, I obtained on the 10th of April a good drop at the extremity of the tube, which remained perfect to the end of the experiments. The drop obtained in this manner is larger than one would wish, but the telescope being carefully adjusted to distant objects, or for parallel rays, and the observations being always taken at the centre of the field of view, there is no correction of the observations required on that account. The iris being steady, was much better seen than in the running stream, and the maxima or bright bars were counted to upwards of forty when a piece of red glass was held before the eye.

The drop being  $\frac{1}{10}$ th of an inch in diameter, and the other circumstances as in the last experiments, the following measures were obtained for the limit beyond which no light could be discerned, from the perpendicular ray:—

42°	22'
42	23 $\frac{1}{2}$ '
42	24 $\frac{1}{2}$ '
42	21'

A good set of measures of the places of the first six maxima were also obtained, whose distances, counted from the caustic, approximated in progression to those of Newton's reflected rings, counting from a black centre as ordinarily considered, namely they approximated to the progression of the values of 1,  $\sqrt{3}$ ,  $\sqrt{5}$ ,  $\sqrt{7}$ , &c.

As the extreme limit of the visible red was taken, it is clear that since the asymptote of the caustic or place ordinarily assigned to the primary rainbow, for the fixed line B is 42° 22' from the perpendicular ray, therefore no light extends beyond this; and that the rays which have traversed equal spaces produce darkness in interfering, contrary to the fundamental requirement of the undulatory theory.

The support which that theory has so long had, from experiments made with running water compared with calculations for still water, must now cease.

London, April 13, 1855.

XLVIII. *Optical Researches.* By A. J. ANGSTRÖM\*.

[With a Plate.]

1. **T**HE dispersion, absorption, and diffusion of light, are effects, the complete solution of which, it may be assumed, is still very distant from us. Thus the explanation given by Cauchy, in conformity with the notions of Fresnel, that the sphere of attraction of the molecules is comparable in magnitude with the length of undulation, has not yet been applied to doubly refracting bodies; and so far as Cauchy's theory has reference to isophanous bodies it also needs completion, inasmuch as the action of the medium itself is not taken into account. Further, as regards the phenomena of absorption, there is certainly no lack of explanations, but the difficulty is to apply them in each particular case; and, as several causes work together, to appropriate to each its due proportion in the production of the phenomena to be explained. The grounds more particularly referred to in the explanation of the absorption and diffusion of light, are the following:—

*a.* An unsymmetrical distribution of the molecules of the medium, the consequence of which is, that in the differential equations for the motion, differential coefficients of an uneven degree are introduced. As far as I know, this cause was first assigned by Cauchy in a letter to Libri.

*b.* The principle adduced by Euler in his *Theoria Lucis et Caloris*, according to which the colour of a body is produced by the resonance of the oscillations, which can be assumed by the particles themselves.

*c.* The interference of light. On this principle Newton is known to have based his celebrated theory of colours, which was further expanded by Biot in the spirit of the theory of emission. M. von Wrede was the first to apply it in the sense of the theory of undulation, by showing that the phenomena, in the case of absorption by gases, &c., might be referred with ease to the principle of interference.

Now with regard to the first of the three grounds of explanation, it alone is by no means sufficient to explain the phenomena of colours. That in general the opacity of bodies might be referred to it as a cause is easy to discern; but it does not explain why a certain colour, to the exclusion of all others, is produced.

The principle of Euler also explains, not so much the colour *which a body actually exhibits*, as that *which it is unable to assume*, because most of the oscillatory motions which bodies assume in

\* From Poggendorff's *Annalen*, vol. xciv. p. 141.

consequence of absorption, make no impression on our organs of sight, but generally fall in the domain of feeling.

If therefore the colours of bodies are to be explained by resonance, this property must also be ascribed to the æther, and thus we arrive with ease at the third of the principles referred to, namely to that of interference.

2. It is necessary to distinguish strictly the absorption of light from its diffusion. Both, indeed, produce the common result of weakening the passing rays; but they differ from each other essentially in the circumstance, that the diffused light possesses the same properties as the incident light; the absorbed portion, on the contrary, exhibits itself as heat, or as a chemical agent; in other words, in the former case the time of oscillation is unchanged, but in the latter this is not the case. This is an essential distinction, which has not always been borne in mind in optical investigations, although Melloni proved it long ago in the case of heat. That the diffused light possesses the same properties as the incident light is most evident from the fact that the lines of Fraunhofer, in the solar spectrum, are also exhibited by the light of the planets, and by all bodies illuminated by the sun. This is not disproved by the newest investigations of Stokes, in which he seeks to show, that, in the case of absorbed light, the time of vibration is altered, because the facts there brought forward belong to the latter class.

*Diffused* light must therefore, according to the foregoing, be produced by modifications of the undulatory motion of the æther itself, while *absorbed* light is transferred to the molecules of the body. These are thereby moved from their positions of equilibrium and thrown into vibrations, which vary with the peculiar characteristics of the body. It is, however, to be well borne in mind, that a medium absorbs, not only those vibrations which it most readily assumes, but also those which occupy a simple relation to its own time of vibration, such as the octave, third, &c. As bodies in general are not luminous, it follows that the absorbed light must escape the cognizance of the eye.

That this, however, is not always the case, is proved by Brewster's remarkable discovery in the case of the alcoholic solution of the green colouring matter of leaves. When a beam of light is transmitted through such a medium, the liquid in the path of the beam is coloured blood-red by unpolarized light. This phenomenon, to which Brewster has applied the name of *inner dispersion*, furnishes a manifest proof that the absorbed light does not always pass to oscillating series of a lower order. The so-called epipolarized rays of Herschel belong to the ordinary case, in which the absorbed light reappears as heat, to the extent that here it is chemical rays which are absorbed, and which,

according to Stokes, produce the luminous effects discovered by Herschel.

3. Now, as according to the fundamental principle of Euler, a body absorbs all the series of oscillations which it can itself assume, it follows from this that the same body, when heated so as to become luminous, must emit the precise rays which, at its ordinary temperature, is absorbed. The proof of the correctness of this proposition is, however, surrounded with great difficulties; for the condition of the heated body, as regards elasticity, is altogether different from the state in which the light is supposed to be absorbed. An indirect proof of the truth of the proposition is furnished by the connexion, discovered by M. Nièpe de Saint Victor, between the colour imparted by a body to the flame of alcohol, and that developed by light upon a disc of silver which has been chlorinized by the body under consideration. As the disc of silver, treated with chlorine alone, assumes all the tints of the solar spectrum, and, when treated at the same time with a colouring body, exhibits almost exclusively the colour of the latter, this cannot occur otherwise than by the exclusive absorption on the part of the so-prepared silver disc of the precise tint which belongs to the colouring body.

4. One of the most convenient and most practicable means of studying a glowing body is presented by the electric spark, although the body in this case is probably in the gaseous condition. I have therefore believed that an investigation of the spectra obtained from electric sparks drawn from different metals would not be without interest for the theory of light. The subject has been already treated by Wollaston, Fraunhofer, Wheatstone, and lastly by Masson, in a memoir in the *Annales de Chimie et de Physique*, 1851. Masson has also measured the electric spectra of various metals, and has drawn them by means of the *Camera lucida*. Nevertheless I venture to hope that the present investigation will not be deemed superfluous, when the results obtained by me are compared with those obtained by my predecessors.

I have found that the spectrum of the electric spark must really be regarded as consisting of two distinct spectra; one of which belongs to the gas through which the spark passes, and the other to the metal or the body which forms the conductor.

The electric spectrum is traversed,—1. By a multitude of luminous lines, comparable in number and distribution with the lines of Fraunhofer in the solar spectrum. These lines are the same for all metals, but vary in intensity with the strength of the condenser, and the greater or less humidity of the air. The nature of the conductor does not appear to exercise any

other influence than that some of its own lines render, by their brightness, some of the others less perceptible.

2. By shining lines peculiar to each metal. These lines are easily distinguished from the foregoing by the fact, that when the condenser is not altogether too strong, and consequently the charge too feeble, they do not form complete transverse lines, but appear to proceed from both edges of the prism, and to be extinguished before they reach the centre. They are no less distinguished from the former lines by their brighter light, and seem, as it were, to stand forth against the dull ground on which the said lines are drawn in undiminished intensity through each point of the transverse section. This predominating brightness, particularly with strong charges, is doubtless the reason why Masson, in all the spectra which he has drawn, has not found more than four lines identical.

To explain the production of the first spectrum, I assume with Faraday and Masson that the electric spark is produced by a current which propagates itself across, and by means of, ponderable matter, which it heats in the same manner, and according to the same laws, as a voltaic current heats a metallic wire. The other spectrum, on the contrary, is produced by the heating and dispersion of the particles of the conductor which proceed simultaneously from both the poles, but on the way lose both their temperature and their luminous properties.

Among the constant lines there are two which were observed by Fraunhofer, and which exceed the others in brightness, namely, a double line in the yellow and one in the green light. In Masson's discovery, and also here in fig. 1, Plate II., they are denoted by  $\gamma$  and  $\delta^*$ . It is possible that these two lines are sometimes common to both spectra, and that their brightness is thus increased; I will not venture to deny this. In the case of bismuth, at least, this is the case with the line  $\gamma$ .

To render the comparison between the solar spectrum and that of the electric spark more evident, I have drawn both of them, side by side, in fig. 1. Both drawings are made from nature, an angle of one minute embracing a millimetre and a half of the drawings. The difference between the spectra is, that to form a correct notion of the upper or electric spectrum, the black lines must be regarded as luminous ones; luminous with the colour which corresponds to their position in the spectrum. For these observations, a flint-glass prism of Merz, with a refracting angle of  $46^{\circ} 34' 57''$ , was made use of.

The observations on which the drawing of the electric spec-

\* In Masson's memoir, however, the line  $\delta$  for antimony is incorrectly given; in like manner the line  $\beta$ , which is in general very weak, is in the case of copper confounded with the line which I call D.



trum is based are contained in the following table. It is to be remarked, that the measurements for the different metals were not determined at once, but that the prism was so fixed for each series that the line D gave the minimum of deflection\*.

	Tin.	Lead.	Zinc.	Platinum.	Mean.
C <sub>1</sub>	+° 16' 10"	° ' "	° ' "	+° 16' 30"	+° 16' 20"
β <sub>2</sub>	.....	.....	.....	+ 8 40	+ 8 40
γ <sub>4</sub>	- 9 11	- 8 36	- 9 3	- 9 3	- 8 58
ε	- 13 20	- 13 11	- 13 55	- 13 38	- 13 31
E <sub>6</sub>	- 29 35	- 29 26	- 30 5	- 29 33	- 29 40
δ <sub>7</sub>	- 39 37	- 39 1	- 39 23	- 38 32	- 39 8
8	.....	.....	.....	- 51 30	- 51 30
9	.....	.....	.....	- 56 20	- 56 20
F <sub>10</sub>	-1 1 52	-1 1 56	-1 2 12	-1 2 13	-1 2 4
11	-1 19 50	-1 16 11	-1 17 10	-1 17 15	-1 18 16
G <sub>12</sub>	-1 27 40	-1 27 11	-1 27 40	-1 26 20	-1 27 13
13	-1 38 35	.....	-1 36 0	-1 35 20	-1 36 38
H <sub>14</sub>	-1 58 50	.....	-1 58 45	-1 58 50	-1 58 48

In order to calculate the indices of refraction, and from them the wave-lengths which correspond to the principal lines of the spectrum, I have measured the absolute deflections of some of them; two spheres of zinc served as conductors.

	a.	b.	
D . .	241° 26' 5"	} 309° 9' 15"	Δ=33° 50' 45"
... ..	... .. 20		
γ . .	... .. 16' 55"	} 309 18 30	Δ=34 0 48
... ..	... .. 17 5		
E . .	240 56 15	} 309 39 0	Δ=34 21 22
... ..	... .. 40		
δ . .	... .. 46 30	} 309 48 15	Δ=34 30 50
... ..	... .. 40		
F . .	... .. 23 50	310 8 55	Δ=34 52 45

The observations for the position *b* of the prism were made *between* the double readings which correspond to the other position *a*.

Two corresponding readings gave for the lines of Fraunhofer, D and *b*, of the solar spectrum,

D . . . .	Δ=33° 51' 40"
<i>b</i> . . . .	Δ=34 21 57

\* A circumstance which interferes with the accuracy of the measurements is due to the fact that the sparks do not always spring from the same point. This may be prevented by coating the balls with a layer of wax. The first spark bursts the coating, and all the succeeding ones follow in its path.

that is to say, values which are nearly equal to those of D and E in the electric spectrum.

A direct experiment nevertheless showed that D in the electric spectrum does not completely coincide with the vivid yellow line formed in the spectrum of the flame of alcohol, but lies somewhat nearer to the red. The yellow line coincides with the line to the left of D which is common to all the metals, and which I have denoted by  $n$  in fig. 2, Plate II.

5. When the solar spectrum is compared with the electric one, it is found that some of the lines, such as C, D, E, G, and we may also say H, have their corresponding lines in the solar spectrum; but for the strongest,  $\gamma$  and  $\delta$ , this is not the case.

A complete correspondence, however, between the solar and the electric spectrum is the less to be expected, as the lines in the former, as is generally assumed, are not only due to the action of the atmosphere, but also to be referred to the action of the sun itself. For the present we are not in a position to separate the two systems of lines from each other, although Brewster's numerous researches on the peculiar influence of the atmosphere have furnished interesting results. Thus the atmosphere produces dark lines on the violet side of C, and on the red side of D; in like manner is produced, on the other side of D, a wide band, possibly in the neighbourhood of  $\gamma$ , &c.

Miller states that during an approaching thunder-storm he has seen dark lines between D and C, but nearer to D, which would seem to coincide with the line  $\gamma$ . In Broch's drawing of the solar spectrum, taken as the sun was near rising or setting, a dark zone also occurs near D, although the distance from D appears to be smaller than that between  $\gamma$  and D. Between F and G is a dark zone, which possibly corresponds to the line F in the electric spectrum.

The analogy between the two spectra may, however, be more or less complete when abstraction is made from all the minuter details. Regarded as a whole, they produce the impression that one of them is a reversion of the other. I am therefore convinced that the explanation of the dark lines in the solar spectrum embraces that of the luminous lines in the electric spectrum, whether this explanation be based upon the interference of light, or the property of the air to take up only certain series of oscillations. The first view has only one difficulty, and that is to explain how the different retardations, which are necessary for the effect, are produced; this will be the more difficult for the electric spectrum, inasmuch as all these retardations must occur in the inconsiderable mass of air which is in direct contact with the electric current.

6. In fig. 2 I have given a sketch of the principal lines

which exhibit themselves in spectra of different metals. That by the use of stronger charges, particularly with the metals which melt at high temperatures, such as platinum and iron, other lines may appear, is probable; I believe, however, that I have omitted none of the more important ones. The simple inspection of the figure shows, however, a great regularity in the distribution of the lines; also, in the case of some metals, a certain tendency to appear in groups. In this manner they show themselves with lead, particularly in the yellow and blue-violet field; with zinc in the red and blue; with bismuth several distinct groups are formed. In general, with the more fusible metals the lines seem to spread themselves more. With mercury and lead the longest go into violet. It is singular that gold and silver have a common line in green; but besides this, silver has a few common with mercury.

To all the metals the line  $n$  is common; also the line  $m$  in red, although in consequence of its feebleness it is difficult to determine its position with accuracy, seems to be common to all; with mercury, however, I did not see it. Coal gave no lines, except those which were to be found in all spectra.

7. Wheatstone has observed that when the poles are formed of different metals, the spectrum contains the lines of both. It was therefore interesting to examine whether a compound of the same metals, particularly a chemical one, would also exhibit the lines of both metals, or distinguish itself by the exhibition of new lines. The first was found to be the case. The only difference observed was, that certain lines were absent, or showed themselves with greater difficulty; but when they appeared, it was always at the same places corresponding to the single metals. It is easy to convince oneself of this when one pole is formed, for example, of  $\text{Sn}^4\text{Pb}$ . From the side of the spectrum which corresponds to the chemical compound, lines proceed which belong to both metals, and the position of these lines may be controlled by making use of both metals successively for the opposite pole.

If the metals are combined in other proportions, no difference appears. With  $\text{Zn Sn}$  the lines in blue were displaced a little towards the violet, but very inconsiderably. This result is so far of interest for the theory, as it shows that the particles of both metals are not united in particular groups, but that each forms distinct centres of oscillation.

8. It does not seem that the lines peculiar to each metal in the electric spectrum can be due to interference. By interference lines of sharp outline cannot be produced except when they lie very near each other, otherwise they spread themselves over a larger portion of the spectrum. Through the union of unequal retardations, groups of fine lines may certainly be formed, but

not well isolated lines. What pronounces particularly against the assumption referred to, is the fact that these luminous lines seem to proceed from the edge of the prism, and to lose themselves in the centre, while the ground on which they exhibit themselves does not appear to be in the slightest degree changed.

In the same manner as we assume that the almost homogeneous light emitted by a flame of alcohol is due to the fact that its oscillations belong almost exclusively to the yellow light, although a trace of luminous lines appears on the rest of the spectrum also, we seem, on the other hand, to be forced to assume that the bright lines belonging to a metal in the electric spectrum, the number of which increases with the strength of the electricity, correspond to the vibrations of the metal in the gaseous condition.

9. In connexion with the foregoing, I will here mention an observation which I have made on the flame of a solution of iodine in alcohol. The alcohol flame alone gives a spectrum shown in the following figure:—



where *a* denotes the vivid yellow line, *b* a weaker one in the extreme blue, and *c* and *d* traces of two other lines. The solution of iodine produces, on both sides of *d*, several bright lines with equal intervals; their distance apart amounts to about 10'. Twice, when the wick was furnished with iodine, I saw dark bands formed between *b* and *c*, which appeared to be at about the same distance from each other. The phenomenon seems to be the same as in the case of the absorption by iodine; the bands were not sharply defined, but resembled real bands of interference.

10. The study of the spectra of the electric spark, it seems to me, can also contribute to the solution of a question equally interesting to both astronomy and optics. Doppler has endeavoured to explain the colour of the double stars by the assumption that the velocity of a body has an influence upon its colour. On the other hand, Petzval, in a discourse before the Academy of Sciences in Vienna, has proved analytically that the motion of a medium can have no influence on the time of oscillation, on which alone the colour depends. Although this proposition is probable in itself, still a practical proof of it is not, I think, without interest.

From Wheatstone's researches on the velocity of electricity,

we know, not indeed the actual velocity of the spark, but at least a limit value, under which it cannot fall. He found that a spark, 4 inches in length, passed in a less time than the millionth part of a second. But a stream of light 6 feet long, in a glass tube partially exhausted, showed itself also unchanged in the rotating mirror; and as, according to Masson, the spark cannot otherwise propagate itself *in vacuo* than by the particles from the poles themselves, it would follow that at least 3 feet were passed over in less than the millionth part of a second. This gives a velocity which in all probability far surpasses that of the double stars.

Now, if a spark be permitted to pass in an oblique direction, according to Doppler's theory the colour of the particles which stream out from one of the poles must be different from that of the particles which stream from the other; the more so, as they move in opposite directions, the velocity above mentioned being thus doubled as regards the production of the colour. Such a difference, however, is not observed. The bright lines correspond to each other perfectly as before. Hence, from both practical and theoretical grounds, we are justified in concluding, that time of oscillation and colour are independent of the velocity of the medium from which the light emanates.

11. In the foregoing pages I have shown that the spectrum of the electric spark is really to be regarded as an overlapping of two spectra, one of which belongs to the metal, and the other to the air through which the spark passes. From this it necessarily follows, that when the spark passes through a gas different from atmospheric air, the spectrum must exhibit a corresponding alteration. Partly to ascertain the nature of this alteration, and partly to place the fact of a superposition of an air and metal spectrum beyond all doubt, I have observed the phenomena exhibited by the electric spark in different gases. The gases which, up to the present time, I have had an opportunity of examining are,—oxygen, carbonic acid, nitric oxide, hydrogen, carburetted hydrogen and nitrogen. As the results obtained appear to me to be not without interest, I take the liberty of adding them to the foregoing.

The apparatus made use of in these observations is shown in fig. 4. It consists of a glass tube and two brass caps, the whole forming a closed cylinder, in which the electric spark passes between the two spheres of brass *a* and *a*. One of the brass balls can be raised or depressed by means of a screw. The tube *b* serves for the introduction of the gas, which then issues through a hole in the opposite end of the cylinder. To ensure that the cylinder contained no atmospheric air, the gas was generally permitted to stream for an hour through the vessel before the

observations began. I made use of the same flint-glass prism as before, so that the results obtained, strictly speaking, are only valid for those kinds of light for which the flint-glass is diaphanous. This is the more necessary to be mentioned here, as it is easy to forget how much glass in general absorbs the extreme ends of the spectrum. I must also mention that the gases made use of were not perfectly pure, although I do not believe that an erroneous result could be produced by the impurity present; the reader will be able to infer this from the statement of the manner in which the gases were prepared.

12. *Oxygen*.—The gas was obtained from chlorate of potash, and was introduced directly into the apparatus. The spectrum obtained in this instance is shown at O, fig. 3. If this spectrum be compared with that of the atmosphere, it is seen,—1st, that the strongest lines of the air spectrum, which in fig. 1 are denoted by  $\gamma$ ,  $\delta$ , D, E, have disappeared; and 2nd, that the blue and violet field exhibit new lines which do not appear in the other case.

With regard, further, to the metal lines in the case of copper and zinc, they were almost imperceptible at the end of the experiment. This was particularly the case with the zinc in the blue field. From this it seems that we may conclude, that the bright lines corresponding to the metal are not produced by the oxidation of the latter, for if this were the case, they must show themselves strongest in oxygen. Nevertheless the surface of the brass spheres showed that an oxidation had taken place, and it is possible that the cause of the phenomenon alluded to is to be sought here.

13. *Carbonic Acid*.—The gas was prepared in the ordinary way by means of sulphuric acid and chalk, and was carried, for the purpose of drying it, through a bottle filled with pumice-stone which had imbibed sulphuric acid. The spectrum obtained is shown at CO<sup>2</sup> in fig. 3; it resembled perfectly the spectrum of oxygen, as far as the strong lines in the blue and violet are concerned. Some difference was, however, observed in regard to the weaker lines; a bright band, corresponding to 13, fig. 1 of the air spectrum, was also observed, which did not appear in the oxygen spectrum. Nevertheless both spectra might be regarded as identical. This is easily explained by the fact, that, according to Berzelius, the electric spark decomposes the carbonic acid into carbonic oxide and oxygen, and the latter gas produces its own peculiar lines in the spectrum.

As it was possible that the violet and blue lines in the oxygen spectrum, and in that of carbonic acid, might have been produced by the oxidation of the metallic particles during their motion through the gas, and as, if this were the case, they must disappear if an

unoxidizable metal were made use of, I had the two spheres thickly gilt (with mercury gilding) and repeated the experiment with carbonic acid. There was no alteration of these lines, which I regard as belonging to the gas. The lines of the metal had, however, been changed, but without becoming quite like those observed in former instances with gold. A luminous band, broken in the middle, showed itself in the oxygen spectrum three lines higher, which corresponds to F in the air spectrum, without, however, coinciding with one of the zinc lines. Besides this, at 5 and E fine lines were observed.

14. *Binoxide of Nitrogen, NO<sup>2</sup>*.—A mixture of sugar and dilute sulphuric acid was heated, and the developed gas was conducted, first through water, and then through the drying bottle before mentioned. The spectrum of the gas showed a union of the air and oxygen spectra, although by no means complete. As, according to Berzelius, the electric spark decomposes the nitric oxide into nitrogen and nitrous acid, or more probably hyponitrous acid, the two lines  $\gamma$  and  $\delta$  must belong to the nitrogen. It is more difficult to explain the production of the oxygen lines, if we do not assume that the nitrous acid is still further decomposed.

15. *Hydrogen*.—This gas was produced from zinc and sulphuric acid, being dried in the ordinary manner. The spectrum is shown at H<sub>a</sub> and H<sub>b</sub> in fig. 3. H<sub>b</sub> represents, in an approximate manner, the luminous intensity of the different parts of the spectrum. Remarkable, in the case of hydrogen, are the strongly luminous and wide lines at the red end of the spectrum, which, moreover, besides a feeble line in the vicinity of the former, contains only two bright portions, one at the limit of blue and green, and the other in the extreme blue.

In general the hydrogen spectrum bears a great resemblance to that of the flame of alcohol, only the red lines must be conceived to be moved nearer to the yellow light. The facility with which hydrogen transmits electricity is remarkable. When the spheres were almost 2 inches asunder, the sparks passed with the greatest ease.

16. *Carburetted Hydrogen, CH*.—This gas was developed from alcohol and sulphuric acid, then conducted through a solution of potash, and afterwards through the drying apparatus. The spectrum was nearly the same as that obtained with hydrogen, and for this reason I have not drawn it. It was however brighter, particularly in the green, and also showed to the left of E, fig. 1, a bright line, which I did not find in the hydrogen spectrum. As the electric spark decomposes carburetted hydrogen into its elements, this result is not to be wondered at, particularly as coal exhibits no lines peculiar to itself in the spectrum.

17. *Nitrogen*.—It follows from what has gone before, that the bright lines in the air spectrum belong almost exclusively to the nitrogen. To submit this conclusion to a direct test, I enclosed a stick of phosphorus in the apparatus and ignited it by means of a heated copper wire, introduced through the small opening which permitted the issue of the gas. The aperture was then closed. The nitrogen thus obtained is not pure, but is mixed with a white smoke of phosphoric acid; this, however, falls and leaves the nitrogen alone behind. *The electric spark showed the same appearances as in atmospheric air*. This result not only corroborates the conclusion already referred to, but it also shows that the presence of the phosphoric acid does not produce any change, and that the air spectrum cannot be regarded as the result of the combustion of oxygen and nitrogen.

18. Another question follows:—Is the glowing of the gaseous particles produced by the propagation of the electricity, or is it a purely mechanical consequence of the motion of the metallic particles? The latter appears to be principally the case. When, for instance, the sparks pass between two points in the same horizontal plane, the metallic particles will nevertheless be scattered vertically, and the lines  $\gamma$ , E,  $\delta$  appear as in *a*, fig. 5. Hence we see that the glowing of the air particles is strongest in the direction in which the metallic particles are scattered, and that both directions commingle, as it were, in the centre. It is remarkable, also, that the metallic particles show no tendency whatever to combine with each other. Thus, for example, on one occasion the zinc lines appeared on the blue, as at *b*, fig. 5, without any sensible change being produced in the air spectrum. The latter observation seems, however, to be opposed to the foregoing, and hence to leave the question in respect to glowing unanswered. A further contribution to its solution is furnished, I believe, in what follows.

In experiments with the sulphurets, for example with sulphuret of silver and sulphuret of antimony, when the same charge was used, two kinds of sparks showed themselves alternately, one stronger than ordinary, the other much weaker and of a dull red appearance, but so that at the surface of the metal itself it possessed its ordinary character. Regarded through the prism, the spark produced at the edge of the spectrum some bright points, but beyond this the field was dark. I placed the prism at a distance of only 4 feet from the spark, but in this case also the field was so dark that it was not possible to determine directly the position of the bright points. Only by the circumstance that now and then stronger sparks showed themselves, was such a determination possible, and it was thus found that the bright points corresponded exactly to the lines peculiar to the metal,



which here showed themselves upon the surface of the metal only. It seems, therefore, that in this case the air alone transmitted the electricity, but did not impart to the ordinary spectrum a sufficient strength of light, because the metallic particles did not by their motion cause the particles of air to glow. It would be well, however, to make this the subject of further investigation. To the same portion of the subject belongs probably a fact which I have several times observed, namely, that when one of the poles is formed of a fluid, of sulphuric acid for example, the lines in the corresponding half of the air spectrum are much weaker than in the other.

19. The experiments which I made with different metallic sulphurets led to no remarkable result. In general they gave the composition of the metal more or less completely. There was no difference observed, whether the sulphuret received from the apparatus positive or negative electricity. As examples, I have given in fig. 2 the spectra for the sulphuret of lead and sulphuret of iron. The latter, however, is distinguished by the fact, that in the vicinity of the line  $\gamma$ , and also in violet, bright lines are shown which were not observed in my experiments with iron. These lines could not, however, belong to the sulphur, because they must then exhibit themselves with other metallic sulphurets.

Sulphur, like carbon, seems to have no peculiar lines. I have made the experiment, not only with charcoal, but also with graphite, and have obtained no lines which could be regarded of the same character as those exhibited by the metals. The lines of the air spectrum, indeed, particularly in the violet, appeared stronger than ordinary; and in several of the bright portions fine lines were plainly visible (*e. g.* at H, fig. 1), but I have had no reason to conjecture that these lines belong to the carbon, particularly as platinum also shows the violet end of the spectrum in a peculiarly manifest manner. The department must be referred to the fact, that the coal particles, by their finer division, set more easily the air particles in a glow.

20. Summing up the observations on the spectra of gases, it follows that, in the oxygen spectrum, the greatest number of bright lines occur in the blue and violet; in the nitrogen spectrum, in green and yellow; and in the hydrogen spectrum, in red. These phenomena must necessarily be connected with the chemical and thermic properties of the gases. Regarded from the theory of undulation, the chemical activity must exert itself chiefly in the motion of the molecules; and it may be assumed with some probability, that two bodies, whose vibrations are isochronous, may combine more easily than two others. In this way the possibility of a combination is finally referred to the

possibility of exciting in two bodies coincident molecular vibrations, where under ordinary circumstances such do not exist.

The chemical activity belongs, nevertheless, to a certain class of rays. Becquerel states, "*que chaque substance sensible voie le rayonnement à sa manière,*" and this is capable of very extensive application. Thus hydrogen, in contact with spongy platinum, is thrown into a condition in which it can enter into combination with oxygen; and the blue colour of sunlight excites in chlorine the power of entering into union with hydrogen. These blue rays act also most powerfully on the solid constituents of plants, while the yellow rays have most to do in producing the green colour, and so on. Hence there are no rays in the solar spectrum which do not possess a chemical power, although the violet end of the spectrum, on account of the part acted by oxygen in most chemical changes, shows itself here most influential. Further, the combustion of most bodies and their combination with oxygen produces a blue flame; zinc has a great affinity for oxygen, and nitrogen a feeble one; and all this seems to speak in favour of the above coincidence in the motions of vibration, which I have stated to be a condition of chemical combination, and proves at the same time that the oscillations of oxygen belong more particularly to the blue and violet portions of the spectrum.

That isomeric bodies possess different chemical properties seems also natural, as a change of position of the molecules must of necessity be accompanied by other vibrations.

21. It is more difficult to explain how, in some cases, a moderate heating calls chemical action into play; for the electric relations of the medium cannot thereby have suffered changes so considerable as to produce new series of oscillation; or, in other words, how it is that one and the same kind of oscillation, simply by increasing its intensity, is capable of producing quite different effects. This phenomenon seems to be explicable in the following manner:

Let  $\xi$ ,  $\eta$ ,  $\zeta$  be the coordinates of a molecule with reference to its position of equilibrium, and let them be supposed to be so small, that, in the differential equations for the motion, their higher powers may be neglected. For a certain kind of motion

$n = \frac{2\pi}{t}$ , we may then set

$$\xi = a \cdot \cos(nt + \lambda)$$

$$\eta = b \cdot \cos(nt + \lambda)$$

$$\zeta = c \cdot \cos(nt + \lambda).$$

Let the amplitudes be now supposed to increase, so that finally  $\xi^2$ ,  $\eta^2$ ,  $\zeta^2$  become sensible; in this case I have found that

the equations of motion will be satisfied by the following assumption:—

$$\xi = \epsilon + a' \cos (nt + \lambda) + \alpha \cos 2 (nt + \lambda)$$

$$\eta = \epsilon' + b' \cos (nt + \lambda) + \beta \cos 2 (nt + \lambda)$$

$$\zeta = \epsilon'' + c' \cos (nt + \lambda) + \gamma \cos 2 (nt + \lambda),$$

so that a simple augmentation of the amplitudes can excite vibrations of a higher order, which in their turn can produce chemical effects.

22. A question of great interest, to the solution of which, however, great difficulties oppose themselves, is the following:— Why is it that the spectrum obtained from the glowing metallic particles of the electric spark exhibits only certain colours, and does not, like the glowing metal itself, show all colours in a continuous series? A glowing platinum wire gives a spectrum without shining lines; the electric spark from platinum gives such lines in numbers. Not without some hesitation as to whether I have hit upon the true cause of this difference, do I venture upon the following explanation.

The general theory of small motions teaches us that the number of the molecular motions of a body amounts to thrice the number of molecules within the sphere of attraction; and from this it follows, that when the number of the molecules is infinite the number of their motions must also be infinite, and hence the spectrum must exhibit itself as a continuous whole. This, however, is not necessarily the case with the metallic particles which are scattered by the electricity. These particles may, in magnitude, remain considerably under the sphere of attraction of the molecules; by this the number of the possible oscillations becomes limited, and hence also the spectrum cannot form a connected whole.

23. I have had only recently an opportunity of becoming fully acquainted with the interesting memoir of Stokes "On the Change of Refrangibility of Light." I see with satisfaction that Stokes's explanation of the remarkable phenomena of dispersion in the green colours of plants, in sulphate of quinine, and in an infusion of horse-chestnut bark, namely, that the medium, when illuminated by the sun, becomes itself luminous, is exactly the same as that which I have given in the foregoing pages of the same phenomenon.

24. The law at which Stokes has arrived is, that in the case of inner dispersion, the refrangibility of the incident light is changed, being generally diminished; or, what is the same, that the time of oscillation of the dispersed light is, in comparison with the incident light, augmented. This law is by no means opposed to dynamical principles. The unchangeability of the

time of oscillation is only true of wave motion; and as long as the luminous ray exists in this form, the time of oscillation is unchanged. When, however, it is absorbed by a medium, the particles of which are thereby set in vibration, the wave motion passes into a pendulum motion, and the law of unchangeability ceases to be true. The molecular motions, however, communicate themselves to the æther, and a new wave motion is thus generated, which, as regards the time of oscillation, is different from that of the incident light, but the same as that of the pendulum motion from which it originates.

As before remarked, we have long possessed in the absorption of calorific rays an example of a similar change. Between the dispersed light and the absorbed heat there is, however, an important difference; the former ceases with the illumination, the latter not. What can be the cause of this difference? It is difficult to answer with certainty. But the reason is probably to be sought in the circumstance, that the substances under examination do not possess any capacity of conduction for colours similar to their conductivity for heat; so that the molecular motions of the medium are immediately transferred to the æther, and become dispersed in the real sense of the term. It is also possible that the molecular motions produced by the action of light are not infinitely small, and hence, in accordance with what has been adduced, a higher order, such as the octave, may be communicated to the æther-vibrations as long as the body is illuminated, but that on the withdrawal of the latter they become insensible. In such a case the medium would, however, have its temperature increased.

Stokes also assumes that the molecular motions of the absorbing medium are not infinitely small, but he draws from this the conclusion, that the periods of motion of the medium are longer than the æther waves which produce them, while according to my investigation exactly the opposite must be the case; premised, moreover, that the parts of the medium swing isochronous with the æther, and not in conformity with its own elasticity.

XLIX. *On the Oblique Aberration of Lenses.*

By JOHN BRIDGE, M.A. *University College, London*.\*

THE importance of destroying oblique aberration in the construction of photographic lenses has induced me to attempt the problem. This I have done in the manner which seemed most suited to the purpose, which was to ascertain the point where a given ray of a pencil emerging from any point of a flat

\* Communicated by the Author.

object meets a flat screen after refraction, both object and screen being perpendicular to the axis of the lens, and the points where they intersect the axis being conjugate foci. The formulæ give immediately the amount of the diffusion, or spread of the rays of any pencil over the surface of the photographic plate, and the effects due to obliquity and aperture respectively are easily deduced. Though they are adapted directly to the case just mentioned, they may without much difficulty be made to suit any other. The following investigation might, in fact, be accurately described as a general view of the theory of spherical mirrors and lenses; this will appear by the deduction of the most important cases usually treated, as well as that which is the immediate object of inquiry. I cannot find that the method of reference to coordinates in a plane perpendicular to the axis at the prime conjugate has been previously adopted, nor that the problem has been solved in its general form\*.

To find the direction of a ray after refraction at a spherical surface.

Let  $\lambda, \mu, \nu$  be the cosines of the angles made by the incident ray with three rectangular coordinates having the centre of the sphere for origin;  $x, y, z$  the point of incidence of the ray;  $m$  the refractive index.

Then  $\frac{x}{r}, \frac{y}{r}, \frac{z}{r}$  are the direction-cosines of the normal

$$1 - \left( \frac{\lambda x + \mu y + \nu z}{r} \right)^2 = \sin^2 \text{ angle of incidence,}$$

$\mu z - \nu y$ , &c. proportional to the direction-cosines of the plane of incidence.

Let  $\lambda', \mu', \nu'$  be the direction-cosines of the refracted ray; then the equations for determining  $\lambda', \mu', \nu'$  are

$$(1.) \quad \lambda'(\mu z - \nu y) + \mu'(v x - \lambda z) + \nu'(\lambda y - \mu x) = 0.$$

$$(2.) \quad \lambda' x \quad + \mu' y \quad + \nu' z \quad = r$$

$$\sqrt{1 - \frac{1}{m^2} \left( 1 - \frac{(\lambda x + \mu y + \nu z)^2}{r^2} \right)} = A \text{ suppose.}$$

$$(3.) \quad \lambda'^2 \quad + \mu'^2 \quad + \nu'^2 \quad = 1.$$

These equations express,—that the refracted ray is in the plane of the normal and the incident ray—the proportionality of the sines of the angles of incidence and refraction—and that  $\lambda', \mu', \nu'$  are direction-cosines.

Whence

$$\lambda'(\mu r^2 - y \cdot \lambda x + \mu y + \nu z) = \mu'(\lambda r^2 - x \cdot \lambda x + \mu y + \nu z) - \Lambda(\lambda y - \mu x),$$

\* For previous inquiries into the subject of oblique aberration, see Potter's 'Optics,' and Clairaut, *Mémoires de l'Institut*, 1762.

and

$$v'(\mu r^2 - y \cdot \overline{\lambda x + \mu y + v z}) = \mu'(v r^2 - z \cdot \overline{\lambda x + \mu y + v z}) + A(\mu z - v y);$$

$$\begin{aligned} \therefore \overline{\mu r^2 - y(\lambda x + \mu y + v z)}^2 &= \mu'^2(r^4 - r^2 \cdot \overline{\lambda x + \mu y + v z}^2) \\ &\quad + 2\mu' \cdot A(\overline{\lambda x + \mu y + v z})^2 - r^2 y \\ &\quad + A^2(y^2 + \mu^2 r^2 - 2\mu y \cdot \overline{\lambda x + \mu y + v z}), \end{aligned}$$

whence

$$\mu' = \frac{y}{m r^2} \sqrt{m^2 r^2 - r^2 + (\lambda x + \mu y + v z)^2} \pm \frac{1}{m r^2} (\mu r^2 - y \cdot \overline{\lambda x + \mu y + v z}).$$

As the process is thus far symmetrical, similar expressions will hold for  $\lambda'$  and  $v'$ .

The equations above are satisfied by two lines in the plane passing through the normal and the incident ray, one on each side of the normal; the upper sign belongs to the refracted ray.

*Expansion of  $\mu'$  and  $v'$ ,*

$$x = \sqrt{r^2 - y^2 - z^2} = r \left( 1 - \frac{1}{2} \frac{y^2 + z^2}{r^2} \right) \quad \lambda = \sqrt{1 - \mu^2 - v^2} = 1 - \frac{1}{2} (\mu^2 + v^2).$$

By substitution we have

$$\begin{aligned} \lambda x + \mu y + v z &= r + \mu y + v z - \frac{1}{2} \cdot \frac{y^2 + z^2}{r} - \frac{r}{2} (\mu^2 + v^2) \\ &= r - \frac{r}{2} \left( \frac{y^2 + z^2}{r^2} - 2 \frac{\mu y + v z}{r} + \mu^2 + v^2 \right) \\ r^2 - (\lambda x + \mu y + v z)^2 &= r^2 \left( \frac{y^2 + z^2}{r^2} - 2 \frac{\mu y + v z}{r} + \mu^2 + v^2 \right) \\ m^2 r^2 - r^2 + (\lambda x + \mu y + v z)^2 &= r^2 \left[ m^2 - \left( \frac{y^2 + z^2}{r^2} - 2 \frac{\mu y + v z}{r} + \mu^2 + v^2 \right) \right] \\ \frac{\sqrt{m^2 r^2 - r^2 + (\lambda x + \mu y + v z)^2}}{m r^2} &= \frac{1}{r} \left[ 1 - \frac{1}{2 m^2} \left( \frac{y^2 + z^2}{r^2} - 2 \frac{\mu y + v z}{r} + \mu^2 + v^2 \right) \right] \\ \frac{\lambda x + \mu y + v z}{m r^2} &= \frac{1}{m r} \left[ 1 - \frac{1}{2} \left( \frac{y^2 + z^2}{r^2} - 2 \frac{\mu y + v z}{r} + \mu^2 + v^2 \right) \right]; \\ \therefore \mu' &= \frac{y}{r} \left[ 1 - \frac{1}{2 m^2} \left( \frac{y^2 + z^2}{r^2} - 2 \frac{\mu y + v z}{r} + \mu^2 + v^2 \right) \right] + \frac{\mu}{m} - \frac{y}{r} \\ &\quad \left[ \frac{1}{m} - \frac{1}{2 m} \left( \frac{y^2 + z^2}{r^2} - 2 \frac{\mu y + v z}{r} + \mu^2 + v^2 \right) \right] \\ &= \frac{\mu}{m} + \frac{m-1}{m} \cdot \frac{y}{r} + \frac{m-1}{2 m^2} \cdot \frac{y}{r} \left( \frac{y^2 + z^2}{r^2} - 2 \frac{\mu y + v z}{r} + \mu^2 + v^2 \right). \end{aligned}$$

As the axis of  $x$  is no longer to be symmetrical with the other two, it will be more convenient that  $\mu$ ,  $v$ ,  $\mu'$ ,  $v'$  should represent

the tangents of the angles made with the axis of  $x$  by the projections on the planes of  $xy, xz$ . We must therefore write for  $\mu$ ,

$\mu\left(1 - \frac{1}{2}\overline{\mu^2 + \nu^2}\right)$ , &c.; this gives

$$\begin{aligned} \mu' &= \frac{\mu}{m} + \frac{m-1}{m} \cdot \frac{y}{r} + \frac{m-1}{2m^2} \cdot \frac{y}{r} \left( \frac{y^2+z^2}{r^2} - 2\frac{\mu y + \nu z}{r} + \mu^2 + \nu^2 \right) \\ &\quad + \frac{1}{2} \cdot \frac{\mu}{m} \left[ \frac{\mu^2 + \nu^2}{m^2} + 2\frac{\mu y + \nu z}{r} \cdot \frac{m-1}{m^2} + \frac{y^2+z^2}{r^2} \cdot \frac{m-1}{m^2} \right] \\ &\quad - \frac{1}{2} \frac{\mu}{u} (\mu^2 + \nu^2) \\ &\quad + \frac{1}{2} \cdot \frac{m-1}{m} \cdot \frac{y}{r} \left[ \frac{\mu^2 + \nu^2}{m^2} + 2\frac{\mu y + \nu z}{r} \cdot \frac{m-1}{m^2} + \frac{y^2+z^2}{r^2} \cdot \frac{m-1}{m^2} \right] \\ &= \frac{\mu}{m} + \frac{m-1}{m} \cdot \frac{y}{r} \\ &\quad + \frac{m-1}{2m^3} \cdot \mu \left[ \overline{m-1} \cdot \frac{y^2+z^2}{r^2} + 2\frac{\mu y + \nu z}{r} - (m+1) \cdot \overline{\mu^2 + \nu^2} \right] \\ &\quad + \frac{m-1}{2m^3} \cdot \frac{y}{r} \left[ \overline{m^2 - m + 1} \cdot \frac{y^2+z^2}{r^2} - 2\frac{\mu y + \nu z}{r} + \overline{m+1} (\mu^2 + \nu^2) \right]. \end{aligned}$$

Next, to find the direction after refraction at the second surface of the lens whose central thickness is  $t$ .

Let  $s$  be the radius of this surface, the curvature being supposed in the same direction as before;  $y_2 z_2$  the point of incidence on it;  $\mu_1 \nu_1$  the direction-tangents of the emergent ray.

The refractive index is here  $\frac{1}{m}$ ; then by substitution in the above, we have

$$\begin{aligned} \mu_1 &= \mu' m - (m-1) \cdot \frac{y_2}{s} \\ &- \mu' \cdot \frac{(m-1)m^2}{2} \left[ -\frac{m-1}{m} \cdot \frac{y_2^2 + z_2^2}{s^2} + 2\frac{\mu' y_2 + \nu' z_2}{s} - \frac{m+1}{m} \cdot (\mu'^2 + \nu'^2) \right] \\ &- \frac{y_2}{s} \cdot \frac{(m-1)m^2}{2} \left[ \frac{m^2 - m + 1}{m^2} \cdot \frac{y_2^2 + z_2^2}{s^2} - 2\frac{\mu' y_2 + \nu' z_2}{s} + \frac{m+1}{m} \cdot (\mu'^2 + \nu'^2) \right]. \end{aligned}$$

Taking now the centre of the second surface as origin,

$$x_2^2 + y_2^2 + z_2^2 = s^2, \quad \therefore x_2 = s - \frac{1}{2} \frac{y^2 + z^2}{s} \text{ nearly;}$$

$$(x+r-s-t)^2 + y^2 + z^2 = r^2, \quad \therefore x+r-s-t = r - \frac{1}{2} \cdot \frac{y^2 + z^2}{r};$$

whence

$$x - x_2 = t - \frac{1}{2} (y^2 + z^2) \left( \frac{1}{r} - \frac{1}{s} \right),$$

and

$$y_2 = y - \mu'(x - x_2) = y - t \left( \frac{\mu}{m} + \frac{m-1}{m} \cdot \frac{y}{r} \right) \\ + \left( \frac{\mu}{m} + \frac{m-1}{m} \cdot \frac{y}{r} \right) \frac{y^2 + z^2}{2} \cdot \left( \frac{1}{r} - \frac{1}{s} \right).$$

Substituting for  $y_2$  and  $z_2$ ,

$$\mu_1 = \mu' m - (m-1) \frac{y}{s} + \frac{m-1}{m} \cdot \frac{t}{s} \left( \mu + \overline{m-1} \cdot \frac{y}{r} \right) \\ + \mu' \cdot \frac{m \cdot (m-1)}{2} \left[ (m-1) \frac{y^2 + z^2}{s^2} - 2m \cdot \frac{\mu' y + \nu' z}{s} + (m+1)(\mu'^2 + \nu'^2) \right] \\ - \frac{y}{s} \cdot \frac{m-1}{2} \left[ (m^2 - m + 1) \frac{y^2 + z^2}{s^2} - 2m^2 \cdot \frac{\mu' y + \nu' z}{s} + m(m+1)(\mu'^2 + \nu'^2) \right] \\ - \mu \cdot \frac{m-1}{2m} \left( \frac{1}{r} - \frac{1}{s} \right) \frac{y^2 + z^2}{s} \\ - \frac{y}{r} \cdot \frac{(m-1)^2}{2m} \cdot \left( \frac{1}{r} - \frac{1}{s} \right) \cdot \frac{y^2 + z^2}{s}.$$

By substituting for  $\mu'$  and  $\nu'$ , and collecting, this becomes

$$\mu_1 = \mu + (m-1) y \left( \frac{1}{r} - \frac{1}{s} \right) + \frac{m-1}{m} \cdot \frac{t}{s} \left( \mu + \overline{m-1} \cdot \frac{y}{r} \right) \\ + \mu \cdot \frac{m-1}{2m} \left[ (y^2 + z^2) \left( \frac{m \cdot \overline{m-1}}{r^2} - \frac{2m^2 - 2m + 1}{rs} + \frac{m^2 - m + 1}{s^2} \right) \right. \\ \left. + 2(\mu y + \nu z) m \left( \frac{1}{r} - \frac{1}{s} \right) \right] \\ + y \cdot \frac{m-1}{2m} \left[ (y^2 + z^2) \left( \frac{m^3 - 2m^2 + m + 1}{r^3} - \frac{3m^3 - 5m^2 + 2m}{r^2 s} \right. \right. \\ \left. \left. + \frac{3m^3 - 4m^2 + 2m - 1}{rs^2} - \frac{m^3 - m^2 + m}{s^3} \right) \right. \\ \left. + 2(\mu y + \nu z) \left( \frac{m^2 - m - 1}{r^2} - \frac{2m^2 - m - 1}{rs} + \frac{m^2}{s^2} \right) \right. \\ \left. + (\mu^2 + \nu^2) \overline{m+1} \cdot \left( \frac{1}{r} - \frac{1}{s} \right) \right].$$

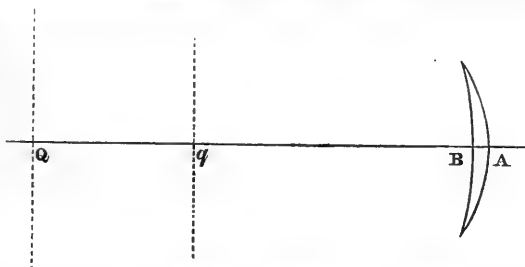
For  $\frac{1}{r} - \frac{1}{s}$  write  $\frac{1}{n}$ , then

$$\mu_1 = \mu + (m-1) \frac{y}{n} + \frac{m-1}{m} \cdot t \left( \frac{1}{r} - \frac{1}{n} \right) \left( \mu + \overline{m-1} \cdot \frac{y}{r} \right) \\ + \mu \cdot \frac{m-1}{2mn} \left[ \overline{y^2 + z^2} \cdot \left( -\frac{1}{r} + \frac{m^2 - m + 1}{n} \right) + 2m \cdot \overline{\mu y + \nu z} \right]$$



$$\begin{aligned}
 & + y \cdot \frac{m-1}{2mn} \left[ \overline{y^2+z^2} \left( \frac{m+2}{r^2} - \frac{m^2+m+1}{rn} + \frac{m^3-m^2+m}{n^2} \right) \right. \\
 & \quad + 2 \cdot \overline{\mu y + \nu z} \left( -\frac{m+1}{r} + \frac{m^2}{n} \right) \\
 & \quad \left. + \overline{\mu^2 + \nu^2} (m+1) \right].
 \end{aligned}$$

Let  $u$  be the distance AQ along the axis of the focus of a



direct pencil of rays before incidence;  $v$  the distance Bq of the focus of an indefinitely small central pencil of these rays after refraction by the lens. Let planes perpendicular to the axis of the lens be drawn through Q and  $q$ , and let  $b, c$  be the coordinates in the directions of  $y, z$  of a point in the plane of Q towards which a ray is proceeding before incidence;  $\eta, \zeta$  the point in the plane of  $q$  towards which the same ray proceeds after refraction. Then

$$\mu = \frac{y-b}{u - \frac{y^2+z^2}{2r}} = \frac{y-b}{u} + (y-b) \frac{y^2+z^2}{2ru^2}$$

$$\mu_1 = \frac{y_2 - \eta}{v - \frac{y^2+z^2}{2s}};$$

$$\therefore \frac{y_2 - \eta}{v - \frac{y^2+z^2}{2s}} = \frac{y-b}{u} + (m-1) \frac{y}{n} + \frac{m-1}{m} \cdot t \left( \frac{1}{r} - \frac{1}{n} \right) \left( \frac{y-b}{u} + \overline{m-1} \cdot \frac{y}{r} \right)$$

$$+ (y-b) \frac{y^2+z^2}{2ru^2}$$

$$- \frac{b}{u} \cdot \frac{m-1}{2mn} \left[ P \cdot (y^2+z^2) + \frac{Q}{u} (y^2+z^2) - \frac{Q}{u} (by+cz) \right]$$

$$+ y \cdot \frac{m-1}{2mn} \left[ (y^2 + z^2) \left( P' + \frac{Q'}{u} + \frac{R'}{u^2} \right) - (by + cz) \left( \frac{Q'}{u} + \frac{2R'}{u^2} \right) + \frac{b^2 + c^2}{u^2} \cdot R' \right]$$

$$+ y \cdot \frac{m-1}{2mn} \cdot \left[ (y^2 + z^2) \left( \frac{P}{u} + \frac{Q}{u^2} \right) - \frac{by + cz}{u} \cdot \frac{Q}{u} \right]^*$$

P, Q, &c. being written for the coefficients of  $(y^2 + z^2)$ ,  $(\mu y + \nu z)$  &c. in  $\mu$ ;

$$\therefore \frac{y_2 - \eta}{v} = \frac{y - b}{u} + \dots + \frac{b}{2sv} (y^2 + z^2) - \frac{y}{2sv} (y^2 + z^2) - (m-1) \frac{y}{n} \cdot \frac{y^2 + z^2}{2sv}$$

$$\frac{y - \eta}{v} = \frac{y - b}{u} + \dots + \dots + \frac{t}{mv} \left( \frac{y - b}{u} + \overline{m-1} \cdot \frac{y}{r} \right) - \frac{y^2 + z^2}{2mnv} \left( \frac{y - b}{u} + \overline{m-1} \cdot \frac{y}{r} \right)$$

$$= (m-1) \frac{y}{n} + \frac{y}{u} - \frac{b}{u} + \frac{t}{m} \cdot \left( \overline{m-1} \cdot \frac{y}{r} + \frac{y}{u} - \frac{b}{u} \right) \left( \frac{m-1}{r} + \frac{1}{u} \right)$$

$$+ y \cdot \frac{m-1}{2mn} \left[ \overline{y^2 + z^2} \cdot \left( \frac{m+2}{r^2} - \frac{4m+4}{ru} + \frac{3m^2+m}{un} - \frac{2m^2+m}{rn} + \frac{3m+2}{u^2} + \frac{m^3}{n^2} \right) + 2 \cdot \frac{by + cz}{u} \left( \frac{m+1}{r} - \frac{m^2}{n} - \frac{2m+1}{u} \right) + \frac{b^2 + c^2}{u^2} (m+1) \right]$$

$$+ \frac{b}{u} \cdot \frac{m-1}{2mn} \left[ \overline{y^2 + z^2} \cdot \left( \frac{m+1}{r} - \frac{m^2}{n} - \frac{2m+1}{u} \right) + 2m \cdot \frac{by + cz}{u} \right].$$

Since  $\frac{1}{v} = \frac{m-1}{n} + \frac{1}{u} + \frac{t}{m} \left( \frac{m-1}{r} + \frac{1}{u} \right)^2$ , this gives

$$\frac{\eta}{v} = \frac{b}{u} + \frac{b}{u} \cdot \frac{t}{m} \left( \frac{m-1}{r} + \frac{1}{u} \right) - y \cdot \frac{m-1}{2mn} \left[ A \cdot \overline{y^2 + z^2} + 2B \frac{by + cz}{u} + (m+1) \frac{b^2 + c^2}{u^2} \right] - \frac{b}{u} \cdot \frac{m-1}{2mn} \left[ B \cdot \overline{y^2 + z^2} + 2m \cdot \frac{by + cz}{u} \right],$$

\* Whence, by putting  $b=0$ ,  $c=0$ , and diminishing  $y$  and  $z$  without limit, we have  $\frac{1}{v} = \frac{m-1}{n} + \frac{1}{u} + \frac{t}{m} \left( \frac{m-1}{r} + \frac{1}{u} \right)^2$  for the focus of a small central direct pencil.

where

$$A = \frac{m+2}{r^2} - \frac{4m+4}{ru} + \frac{3m^2+m}{un} - \frac{2m^2+m}{rn} + \frac{3m+2}{u^2} + \frac{m^3}{n^2}$$

$$B = \frac{m+1}{r} - \frac{m^2}{n} - \frac{2m+1}{u}.$$

The expressions in [ ] are symmetrical with respect to  $y$  and  $z$ , so that the expression for  $\frac{\zeta}{v}$  is found by writing  $c$  and  $z$  for  $b$  and  $y$  outside these brackets.

Putting  $c=0$ , that is, supposing the ray, when incident, to be proceeding to a point of the plane of Q where it is cut by the plane of  $xz$ , we have

$$\frac{\eta}{v} = \frac{b}{u} + \frac{b}{u} \cdot \frac{t}{m} \left( \frac{m-1}{r} + \frac{1}{u} \right)$$

$$- y \cdot \frac{m-1}{2mn} \left[ A(y^2 + z^2) + 2B \frac{by}{u} + \frac{b^2}{u^2}(m+1) \right]$$

$$- \frac{b}{u} \cdot \frac{m-1}{2mn} \left[ B(y^2 + z^2) + 2m \frac{by}{u} \right]$$

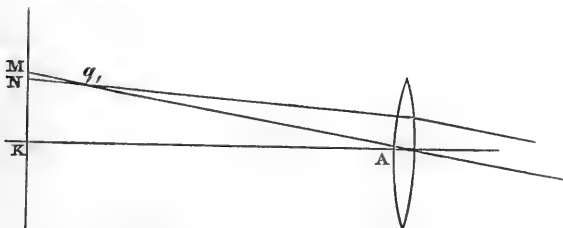
$$\frac{\zeta}{v} = -z \cdot \frac{m-1}{2mn} \left[ A(y^2 + z^2) + 2B \frac{by}{u} + (m+1) \frac{b^2}{u^2} \right].$$

It is obvious that when  $b=0, c=0$ , the above gives the expression for the aberration in the same form as that in Encyc. Britt., art. *Telescope*\*. I have found that form more convenient for the calculation of a large number of cases than those in Coddington.

It is satisfactory to find that these expressions very easily yield those for particular cases as found in Coddington and elsewhere, as follows:—

*Primary and secondary foci of a small central oblique pencil.*

For the primary focus, that is, for rays incident in the axis of  $y$  ( $c$  being supposed  $=0$ ),



MK the plane perpendicular to the axis at the focus for a

\* Clairaut is, I find, the author of this form.

small, direct, central pencil. Then

$$\frac{MN}{Mq_1} = \frac{y}{AM} \text{ nearly,}$$

or

$$Mq_1 = x_1 = -\frac{AM}{y} \cdot \Delta\eta = -\frac{v}{y} \Delta\eta = +v^2 \cdot \frac{b^2}{u^2} (3m+1) \frac{m-1}{2mn}$$

$$x_1 = \frac{v^2}{2f} \cdot \frac{b^2}{u^2} \left(3 + \frac{1}{m}\right).$$

For the secondary focus,

$$x_2 = -\frac{AM}{z} \Delta\zeta = -\frac{v}{z} \Delta\zeta = v^2 \frac{b^2}{u^2} (m+1) \frac{m-1}{2mn}$$

$$= \frac{v^2}{2f} \cdot \frac{b^2}{u^2} \cdot \left(1 + \frac{1}{m}\right).$$

(See Coddington, p. 119.)

*Primary and secondary foci of a small excentrical pencil which crosses the axis ( $z=0$ ).*

For the primary,

$$\frac{\Delta\eta}{x_1} = \frac{\Delta y}{v} \quad x_1 = -v \frac{d\eta}{dy} \text{ nearly,}$$

$$= +v^2 \cdot \frac{m-1}{2mn} \left(3 \cdot \sqrt{Ay^2 + 2B\frac{by}{u} + m\frac{b^2}{u^2} + \frac{b^2}{u^2}}\right)$$

$$= \frac{v^2}{2f} \cdot \left(3 \cdot \frac{Ay^2 + 2B\frac{by}{u} + m\frac{b^2}{u^2}}{m\frac{b^2}{u^2}} + \frac{1}{m}\right) \frac{b^2}{u^2}$$

$$= \frac{v^2}{2f} \cdot \frac{b^2}{u^2} \left(3V + \frac{1}{m}\right);$$

since it is found that

$$\frac{Ay^2 + 2B\frac{by}{u} + m\frac{b^2}{u^2}}{m\frac{b^2}{u^2}}$$

is equal to what in Coddington is called  $V^*$ . (See Coddington, pp. 137 and 138, or the *résumé* in pp. 196, &c.)

\* This is a much more convenient form than Coddington's  $V$ , since it enables us to distinguish immediately the effects due to aperture and obliquity.

In a similar manner for the secondary focus,

$$x_2 = \frac{v^2}{2f} \cdot \frac{b^2}{u^2} \left( V + \frac{1}{m} \right).$$

Also, by substituting for  $\mu$  and  $\mu'$  in the expression for refraction at one spherical surface, their values

$$\frac{y-b}{u} + \frac{y-b}{u} \cdot \frac{y^2+z^2}{2ru}$$

and

$$\frac{y-\eta}{v} + \frac{y-\eta}{v} \cdot \frac{y^2+z^2}{2rv} = \frac{y-\eta}{v} + \frac{2y}{r} \cdot \frac{y^2+z^2}{2rv} - \frac{y-b}{u} \cdot \frac{y^2+z^2}{2rv} \text{ nearly,}$$

and  $-1$  for  $m$ , we find

$$-\frac{\eta}{v} = \frac{b}{u} + \left[ y \left( \frac{1}{r} - \frac{1}{u} \right) + \frac{b}{u} \right] \frac{1}{r} \left( \frac{1}{y^2+z^2} \left( \frac{1}{r} - \frac{1}{u} \right) + 2 \frac{by+cz}{u} \right)$$

for the point of intersection of a reflected ray with a plane perpendicular to the axis through the focus of rays proceeding to the projection on the axis of the focus of incident rays.

By limiting the expressions to the case considered in Potter's 'Optics,' part 2. p. 110, &c., they will be found to give easily the same results as those in pp. 117, 118, 119 of the same work.

To find the plane perpendicular to the axis of the lens on which the aberration of a given ray of an oblique pencil is a minimum, that is, the distance between the points of intersection by it and by the central ray.

The equations of the ray, taking the origin at distance  $v$  from the lens, are (putting  $\mu$  for  $\mu_1$ ,  $\mu_0$  for the central ray)

$$Y = \mu X + \eta$$

$$Z = \nu X + \zeta;$$

and for the central ray,

$$Y_0 = \mu_0 X + \eta_0$$

$$Z_0 = \nu_0 X + \zeta_0;$$

the distance of the points of intersection of these rays with a plane perpendicular to the axis at distance  $X$  from the origin is

$$D = \sqrt{(Y - Y_0)^2 + (Z - Z_0)^2}$$

and

$$D^2 = [(\mu - \mu_0)^2 + (\nu - \nu_0)^2] X^2 + 2(\mu - \mu_0 \cdot \eta - \eta_0 + \nu - \nu_0 \cdot \zeta - \zeta_0) X + ((\eta - \eta_0)^2 + (\zeta - \zeta_0)^2).$$

This is a minimum when

$$X = - \frac{(\mu - \mu_0)(\eta - \eta_0) + (\nu - \nu_0)(\zeta - \zeta_0)}{(\mu - \mu_0)^2 + (\nu - \nu_0)^2};$$

and then

$$D^2 = \frac{((\mu - \mu_0)(\zeta - \zeta_0) - (v - v_0)(\eta - \eta_0))^2}{(\mu - \mu_0)^2 + (v - v_0)^2}.$$

Now

$$\eta - \eta_0 = -\left(My + N \frac{b}{u}\right)v$$

$$\mu - \mu_0 = \frac{y}{v} \text{ nearly,}$$

where M and N are symmetrical about the axis,

$$X = + \frac{M(y^2 + z^2) + N \frac{by + cz}{u}}{y^2 + z^2} \cdot v^2 = v^2 \left( M + N \cdot \frac{by + cz}{u(y^2 + z^2)} \right)$$

$$D = \frac{v}{u} \cdot N \frac{bz - cy}{\sqrt{y^2 + z^2}}$$

$$Y - Y_0 = -v \cdot N \cdot \frac{bz - cy}{u(y^2 + z^2)} \cdot z \quad Z - Z_0 = +v \cdot N \frac{bz - cy}{u(y^2 + z^2)} \cdot y$$

$$\frac{Z - Z_0}{Y - Y_0} = -\frac{y}{z}.$$

The line joining the two points in the intersection is therefore perpendicular to the line joining the two points of incidence.

From these formulæ may be deduced the expressions for the primary and secondary foci of an oblique central pencil.

If the focus of incident rays be supposed to be a point in the plane of  $zx$ ,  $b=0$ .

Then for a ray incident in the axis of  $z$ ,  $y=0$ , and it is clear that  $D=0$ , or it meets the central ray.

$X_1 = \frac{v^2}{2mf} (3m+1) \frac{c^2}{u^2}$ , if  $\frac{z}{r}$  be supposed very small as compared with  $\frac{c}{u}$ .

For a ray incident in the axis of  $y$ ,  $z=0$ ,  $N=0$ ;  $\therefore D=0$ , and  $X_2 = \frac{v^2}{2mf} (m+1) \frac{c^2}{u^2}$ .

If for brevity we write the expressions for  $\frac{\eta}{v}$  in the form  $\frac{\eta}{v} = \frac{b}{u} + T + U$ , we have for the coordinate  $\eta_2$  of the point where a ray after refraction at two lenses in contact meets the plane perpendicular to their common axis through the focus for central direct rays,

$$\frac{\eta_2}{v_2} = \frac{\eta_1}{v_1} + T_2 + U_2 = \frac{b}{u} + T_1 + T_2 + U_1 + U_2.$$

In the small terms  $y_2, z_2, \frac{\eta_1}{v_1}, \frac{\zeta_1}{v_1}$  may be considered as equal to  $y_1, z_1, \frac{b}{u}, \frac{c}{u}$ .

The conditions that all rays proceeding from any one point in a plane perpendicular to the axis may meet in one point on the screen placed to receive the image, are

$$U_1 + U_2 + \dots = 0 \text{ (and a similar one for } \zeta)$$

for all values of  $y, z, b, c$ , involving among other conditions, that

$$\frac{m_1 - 1}{n_1} + \frac{m_2 - 1}{n_2} + \dots = 0, \text{ or } \frac{1}{f_1} + \frac{1}{f_2} + \dots = 0,$$

$f_1, f_2$ , &c. being the principal focal lengths of the lenses, the thicknesses being neglected. This equation is impossible so long as the lenses remain in contact.

For two lenses not in contact, but whose interior surfaces are separated by an interval  $d$ , the aberration is found as follows.

Let  $v_1 - d = u_2$ , and let  $y_2, z_2$  be the point of incidence on the second lens;  $C_2 = m_2 + 1, 2m = 2(C_2 - 1)$ . Then

$$y_2 = \frac{u_2}{v_1} y + d \cdot \frac{b}{u};$$

and by substitution we obtain

$$\begin{aligned} \frac{\eta_2}{v_2} = & \frac{\eta_1}{u_2} + \frac{v_1}{u_2} \cdot \frac{b}{u} \cdot \frac{t_2}{m_2} \left( \frac{m_2 - 1}{r_2} + \frac{1}{u_2} \right) \\ & - y \cdot \frac{m_2 - 1}{2m_2 n_2} \left( (y^2 + z^2) A_2 \frac{u_2^3}{v_1^3} + 2 \frac{by + cz}{u} \left( A_2 \frac{u_2^2}{v_1^2} d + B_2 \frac{u_2}{v_1} \right) \right. \\ & \quad \left. + \frac{b^2 + c^2}{u^2} \left( A_2 \frac{u_2}{v_1} d^2 + 2B_2 d + C_2 \frac{v_1}{u_2} \right) \right) \\ & - \frac{b}{u} \cdot \frac{m_2 - 1}{2m_2 n_2} \left( (y^2 + z^2) \left( A_2 d \frac{u_2^2}{v_1^2} + B_2 \frac{u_2}{v_1} \right) + 2 \frac{by + cz}{u} \right. \\ & \quad \left. \left( A_2 \frac{u_2}{v_1} \cdot d^2 + 2B_2 d + C_2 \frac{v_1}{u_2} - \frac{v_1}{u_2} \right) \right. \\ & \quad \left. + \frac{b^2 + c^2}{u^2} \left( A_2 d^3 + 3B_2 \frac{v_1}{u_2} d^2 + 3C_2 \frac{v_1^2}{u_2^2} \cdot d - 2 \frac{v_1^2}{u_2^2} \cdot d \right) \right). \end{aligned}$$

It may sometimes be convenient to use the polar forms of some of the expressions, as follows.

$$\text{If } z = \rho \sin \alpha, y = \rho \cos \alpha, b = \sigma \cos \beta, c = \sigma \sin \beta, \frac{Z - Z_0}{Y - Y_0} = \tan \theta,$$

then

$$X = v^2 \left( M + N \frac{\sigma \cos(\alpha - \beta)}{\rho u} \right)$$

$$D = N \frac{v}{u} \sigma \sin(\alpha - \beta)$$

$$\tan \theta = - \frac{1}{\tan \alpha},$$

and

$$M = \frac{1}{2mf} \left( A\rho^2 + 2B \frac{\rho\sigma}{u} \cos(\alpha - \beta) + (m+1) \frac{\sigma^2}{u^2} \right)$$

$$N = \frac{1}{2mf} \left( B\rho^2 + 2m \frac{\rho\sigma}{u} \cos(\alpha - \beta) \right).$$

The practical object will of course be to find those forms of lenses which by their combination reduce  $\Delta\eta$  and  $\Delta\zeta$  to nothing, if possible, for all values of  $y, z, b, c$ . This involves the solution of five equations, upon the discussion of which I do not propose to enter at present. It may be desirable at some future time to extend the solution so as to introduce the 4th powers of  $b$  and  $c$ , but not of  $y$  and  $z$ . I would suggest also the want of some fixed principle of selection among imperfect foci. I have found the results of assuming a law which answers the obvious requirements of the case, as follows:—If that be called the best section (focus) of a pencil which gives the sum of the squares of the distances from their centre of intensity of the points where the several rays meet the plane of the section, we find that the circle of least confusion conforms accurately to this condition, and that for a large direct pencil the distance of the best focus from the least circle of aberration is  $\frac{1}{3}$ th of the whole longitudinal aberration. I think some satisfactory conclusion on this point might be attained by careful experiment.

L. *Examinations of some Felspathic Rocks.*

By T. S. HUNT, of the Geological Commission of Canada\*.

**T**HE oldest known group of rocks in North America is that to which the name of the Laurentian System has been given in the Reports of the Geological Survey of Canada, and is made up of very much disturbed and highly crystalline strata, which are in great part gneissoid and quartzose. Interstratified with these, however, are beds of crystalline limestone, often of great thickness, and holding scapolite, pyroxene, phlogopite, serpentine, fluor, apatite, spinel, corundum, condroidite, sphene,

\* Communicated by the Author.



graphite, and other characteristic minerals. Associated with these limestones, there is another rock forming an important member of the group, and consisting chiefly of felspar, with small portions of black mica, green pyroxene, and occasional grains of garnet, epidote, and more rarely of quartz. This rock is often marked by the presence of small portions of hypersthene; from the presence of this mineral, these masses have been described by the New York geologists as *hypersthene rock*. In addition to these minerals we may add ilmenite, or titaniferous iron ore, which occurs sometimes in large masses, and at other times in small disseminated grains, which like the hypersthene appear to mark the planes of stratification. If to these we add small portions of iron pyrites and a little disseminated carbonate of lime, we have the mineralogy of the rock so far as yet known.

The texture of these felspar rocks is varied; sometimes the mass is a confusedly crystalline aggregate, exhibiting cleavage surfaces three or four lines in diameter, with a fine-grained, somewhat calcareous paste in the interstices. Sometimes the whole rock is uniformly granular, while more frequently a granular base holds at intervals cleavable masses of felspar, often several inches in diameter. The colours of these rocks vary from grayish and bluish-white to lavender and violet-blue; flesh-red, greenish and brownish tints are also met with; the colours are rarely brilliant. These felspars seldom occur in distinct crystals, but their cleavage is triclinic, a fact which, coupled with their densities varying from 2.66 to 2.73, shows them to belong to the group of which albite and anorthite may be taken as the representatives. The bluish cleavable varieties often exhibit the opalescence of labradorite, to which species American mineralogists have hitherto referred them; but with the exception of a few analyses by myself, we have no published analyses of any of these felspars. My investigations show, that while all of them are felspars with a base of lime and soda, the composition varies very much, being sometimes that of labradorite, andesine or intermediate varieties, and at other times approaching to that of anorthite. The results of these examinations, as far as yet completed, I propose to give in the present paper, as the first part of the history of these felspathic rocks.

One of the most interesting localities of these felspars is in the parish of Château Richer, in the county of Montmorenci near Quebec, where they cover a breadth of two or three miles across the strike, bounded by crystalline limestone on one side, and a quartzo-felspathic rock on the other, and rising into small hills. In this region several varieties of the rock appear, but the most interesting is made up of a finely granular base, greenish or grayish-white in colour, holding masses of a reddish cleavable

felspar, which are sometimes from one-tenth to one-half an inch in diameter, but often take the form of large imperfect crystals, frequently 12 inches long, and 4 or 5 inches wide. These dimensions correspond to the faces M and T, while the face P, characterized by its perfect cleavage, is from half an inch to 2 inches broad. Twin crystals sometimes occur having a composition parallel to M.

Hypersthene is met with throughout the rock in flattened masses, which although variable and irregular in their distribution, exhibit a general parallelism; they are occasionally 4 or 5 inches in length and breadth, by an inch or more in thickness, and are separated from the granular felspathic rock by a thin film of brownish-black mica. Titaniferous iron ore is also found in grains and lenticular masses, occasionally an inch or two in thickness; these occur in the granular base, and generally near the hypersthene, but grains of the mineral are occasionally found in the crystalline felspar. Quartz in small grains is imbedded in the titaniferous iron, but was not observed elsewhere in the rock, nor have any other minerals than these been detected. In the specimens of the rock which I selected on the spot for examination, the crystalline felspar constitutes from one-half to seven-eighths, while the hypersthene does not equal more than  $\frac{2}{100}$  dths, and the titaniferous iron more than  $\frac{1}{100}$  dth of the whole; the amounts of quartz and mica are insignificant. In other portions of the rock, however, the proportion of the ore may equal 5 per cent., and in some parts the amount of hypersthene is nearly as great. By the action of the elements, the surface of the rock becomes of a dull opake white; the cleavable masses of felspar are, however, less affected than the granular portion, and by their obscure reddish colour are distinctly visible on the weathered surfaces; this change extends but a very little distance into the rock. The iron ore of course remains unaltered, but the dark brown hypersthene becomes lighter, and inclines to a pinchbeck-brown.

The felspar is triclinic in cleavage; the angle of P : M = about  $80^{\circ} 80'$ . Cleavage with P perfect, with the other planes distinct; P is often delicately striated, and sometimes curved. Hardness = 6, and density 2.667 to 2.674. Lustre vitreous, sometimes pearly on P; colour flesh-red, passing into reddish, greenish and grayish-brown; the surfaces sometimes mottled, but the red always predominating.

The following analyses were made of three different specimens, which were carefully selected, and after being pulverized, dried at  $212^{\circ}$  F. The earthy ingredients were determined after fusion with carbonate of soda, and the alkalis by the method of Dr. J. L. Smith, which consists in igniting for some time the finely

levigated mineral with five or six parts of carbonate of lime and three-fourths its weight of sal-ammoniac. The agglutinated mass slakes by the action of water, and yields to that liquid its alkalis in the form of chlorides, with a mixture of chloride of calcium. A second ignition of the undissolved residue with two-thirds the first amount of sal-ammoniac ensures the separation of the last portions of alkali. These processes were adopted in all the analyses here given, with some exceptions to be noticed in their places.

	I.	II.	III.
Silica . . . .	59·55	59·85	59·80
Alumina . . . .	25·62	25·55	25·39
Peroxide of iron . .	·75	·65	·60
Lime . . . .	7·73	6·94	7·78
Magnesia . . . .	traces	·11	·11
Potash . . . .	·96	·96	1·00
Soda. . . . .	5·09	5·09	5·14
Loss by ignition . .	·45	·30	·00
	<hr/> 100·15	<hr/> 99·45	<hr/> 99·82

In another specimen the amount of lime was found to equal 7·89 per cent. The composition of this felspar is very nearly that of andesine, which, according to Abich, yields silica, 59·60; alumina, 24·18; peroxide of iron, 1·58; lime, 5·77; magnesia, 1·08; potash, 1·08; soda, 6·53 = 99·82.

The greenish base of the rock is generally finely granular and strongly coherent; the grains possess the cleavage, lustre, and hardness of felspar; the density of carefully chosen fragments was from 2·665 to 2·668. The greenish-white of the powder is changed to a fawn colour by ignition. When pulverized and digested with acetic acid, the mineral loses two or three thousandths of carbonate of lime, with traces of magnesia, iron oxide, and alumina. A portion which had been thus treated and carefully dried gave the following results:—

	IV.
Silica . . . . .	58·50
Alumina . . . . .	25·80
Peroxide of iron . .	1·00
Lime . . . . .	8·06
Magnesia . . . . .	·20
Potash . . . . .	1·16
Soda . . . . .	5·45
Loss by ignition . .	·40
	<hr/> 100·57

It is therefore a felspar differing but little from the crystalline andesine in composition.

The hypersthene occurs in foliated masses with curved surfaces. Besides the basal cleavages thus exhibited, it cleaves readily with the sides of an oblique prism of  $87^\circ$ , and with its longer diagonal. The hardness of the mineral is 6, and its density 3.409 to 3.417. Lustre vitreous, submetallic; colour blackish-brown, in thin laminae yellowish-brown; streak and powder ash-gray, the powder turning reddish-gray on ignition. Subtranslucent, brittle, fracture uneven. The fragments which had served to determine the density still contained flakes of felspathic matter between the laminae; these were, as far as possible, removed in breaking up the hypersthene for analysis. The results of two analyses by fusion with carbonate of soda were as follows:—

	V.	VI.
Silica . . . .	51.85	51.35
Alumina . . . .	3.90	3.70
Protoxide of iron . . . .	20.20	20.56
Lime . . . . .	1.60	1.68
Magnesia . . . .	21.91	22.59
Manganese . . . .	traces	
Loss on ignition . . . .	.20	.10
	<hr/> 99.66	<hr/> 99.98

It is almost identical in composition with the hypersthene from Labrador, analysed by Damour.

The accompanying ilmenite was more or less interpenetrated with felspar and quartz, which could not easily be separated. Its hardness was 6, and the density of selected fragments from 4.65 to 4.68. Colour and streak iron-black, lustre submetallic; not magnetic. When decomposed by fusion with bisulphate of potash, it gave,—

	VII.
Titanic acid . . . . .	39.86
Peroxide of iron . . . . .	56.64
Magnesia . . . . .	1.44
Insoluble quartz, &c. . . . .	4.90
	<hr/> 102.84

A large portion of the iron is to be regarded as protoxide.

Another variety of the lime-felspar rock from Château-Richer is pale greenish or bluish-gray, with occasional reddish grains, and is finely granular. The lustre is vitreous upon the cleavages, but waxy elsewhere. The only foreign mineral observed in the rock was brownish-black mica in small scattered patches. The density of the greenish-gray portion was 2.681, and its analysis gave,—

	VIII.
Silica . . . . .	55·80
Alumina . . . . .	26·90
Peroxide of iron . . . . .	1·53
Lime . . . . .	9·01
Magnesia . . . . .	·27
Potash . . . . .	·86
Soda . . . . .	4·77
Loss by ignition . . . . .	·45
	99·59

In Château-Richer and its vicinity there are found boulders of a well-defined variety of the felspar rock, which has not yet been met with in place. The base is a coarsely granular felspar of a light reddish-gray colour and vitreous lustre, exhibiting everywhere distinct cleavages, and holding imbedded small bright grains of ilmenite, surrounded with thin films of brownish mica. The imbedded crystals of felspar are numerous, and are often 3 or 4 inches in length and breadth by an inch in thickness. The faces of perfect cleavage are beautifully striated, and the smaller crystals, which are often slender and well defined, are sometimes curved. Hardness, 6; density, 2·680 to 2·692; lustre, vitreous; colour, pale lavender blue, with pearly opalescence. Semitransparent, fracture conchoidal. Analysis IX. is a cleavable fragment from a boulder found at Château-Richer, and X. and XI. are from a similar and larger mass in the neighbouring parish of St. Joachim.

	IX.	X.	XI.
Silica . . . . .	57·20	57·55	57·35
Alumina . . . . .	26·40	27·10	27·30
Peroxide of iron . . . . .	·40		
Lime . . . . .	8·34	8·73	
Potash . . . . .	·84	·79	
Soda . . . . .	5·83	5·38	
Loss by ignition . . . . .	·65	·20	·25
	99·66	99·75	

The district of Montreal also affords extensive exposures of these same felspar rocks, associated with crystalline limestone, in the counties of Leinster and Tenebonne. In the townships of Rawdon and Chertsey they are often fine-grained and homogeneous, and constitute an exceedingly tough rock, with an uneven subconchoidal fracture, and a feeble vitreous lustre; this variety is bluish or grayish-white in colour, somewhat translucent, and exhibits here and there the cleavage of grains of felspar. Great bodies of this rock are almost free from foreign minerals, while other portions abound in a green granular pyroxene, arranged in thin interrupted parallel layers with ilmenite. These layers of pyroxene are seldom more than 4 or 5 lines in thickness,

and may be an inch or two apart, while those of ilmenite are still thinner, and often enclosed in the pyroxene, along the surfaces of which deep red grains of garnet are occasionally seen. These different minerals appear in relief upon the white weathered surface of the rock, and give a picture of its stratified structure, which is, however, not less apparent upon the surfaces of recent fracture. Small, rounded, bluish masses of cleavable felspar are frequently disseminated in the same planes as the other minerals. In some places the pyroxene appears to pass into, or is replaced by, foliated hypersthene. A homogeneous fragment of this rock from Rawdon had a density of 2·691. It was bluish-white, granular and translucent, and gave by analysis,—

XII.	
Silica . . . . .	54·45
Alumina . . . . .	28·05
Peroxide of iron . . . . .	·45
Lime . . . . .	9·68
Potash . . . . .	1·06
Soda . . . . .	6·25
Loss by ignition . . . . .	·55
	100·49

In connexion with this variety, which has nearly the composition assigned to labradorite, I may give the analyses of two felspars which differ from it principally in their greater amount of lime. No. XIII., from the township of Morin, belongs to the same area as the last, and forms large cleavable masses in a fine-grained base. Its density is 2·684 to 2·695, and its colour greenish-gray, passing into bluish-gray, with an occasional pearl-gray opalescence. The greenish paste of this felspar is softer than the others, and yields with effervescence about four hundredths of carbonate of lime to dilute acids; the insoluble residue has nearly the composition of the felspar. No. XIV. is a cleavable, lavender-blue, opalescent variety, from a rolled mass found in Drummond, Canada West, and having a density of 2·697. The determination of the alkalis in this was not made by the same process as in the other analyses, and probably does not give the full amount.

	XIII.	XIV.
Silica . . . . .	54·20	54·70
Alumina . . . . .	29·10	29·80
Peroxide of iron . . . . .	1·10	·36
Lime . . . . .	11·25	11·42
Magnesia . . . . .	·15	traces
Potash } by diff.	3·80	·23
Soda } by diff.	...	2·44
Loss by ignition . . . . .	·40	·40
	100·00	99·35

At La-Chute on the Rivière du Nord, there is a felspar rock associated like the others with crystalline limestone, and holding in a greenish granular base a cleavable felspar resembling andesine in composition. Its lustre is vitreous, and the face of perfect cleavage, as in all these felspars, is finely striated. Density, 2.687; colour, lavender-blue, passing into sapphire-blue; semitransparent. Its analysis gave—

	XV.
Silica . . . . .	58.15
Alumina . . . . .	26.09
Peroxide of iron . . . . .	.50
Lime . . . . .	7.78
Magnesia . . . . .	.16
Potash . . . . .	1.21
Soda . . . . .	5.55
Loss by ignition . . . . .	.45
	99.89

The bytownite of Thompson appears to be one of these granular felspar rocks, and can scarcely be distinguished from some of the varieties just described. In 1850 I examined an authentic specimen of the mineral, and found it to have a hardness of 6.5, and a density of 2.732; it gave by analysis,—

	XVI.	
Silica . . . . .	47.40	47.30
Alumina . . . . .	30.45	
Peroxide of iron . . . . .	.89	
Lime . . . . .	14.24	
Magnesia . . . . .	.87	
Potash . . . . .	.38	
Soda . . . . .	2.82	
Loss by ignition . . . . .	2.00	
	99.05	

I remarked at the time, the undoubted felspathic character of the mineral, which I described as corresponding to the thior-sanite of Genth, and as probably anorthite with an admixture of quartz\*.

The frequent association of ilmenite with these felspars, derives additional interest from the fact, that the immense deposits of this ore at Bay St. Paul are accompanied with a lime felspar. Here, besides many smaller masses, a body of titaniferous iron ore, 300 feet long by 90 feet wide, is exposed on the side of a hill, and a still larger mass is said to occur in the vicinity. The

\* See this Magazine, S. 4. vol. i. p. 324. Also Report of the Geological Survey of Canada for 1850-51, p. 38, where analysis XIV. has also appeared.

ore is coarsely crystalline; its colour and streak are iron-black, and its lustre submetallic; it affects the magnetic needle very feebly. Hardness, 6; density, 4.56 to 4.66. Its analysis gave me titanio acid, 48.60; protoxide of iron, 37.06; peroxide of iron, 10.42; magnesia, 3.60=99.68. Disseminated through portions of the ore, are small, garnet-red, translucent grains, which have an adamantine lustre, a conchoidal fracture, and a hardness of 6. They are found by analysis to be pure oxide of titanium, and are to be referred to the species rutile or brookite.

We have in the rocks which have been the subject of these examinations, a series of felspars in which the amount of silica varies from 47.40 to 59.80 per cent., and that of the lime from 7.73 to 14.24 per cent., the amount of the alkalis decreasing as that of the lime augments. These results only help to confirm the conclusion which may be drawn from all the previous analyses of triclinic felspars, that there are no defined limits for those species which, like vospite, labradorite, andesine, and oligoclase, have been created between albite on the one hand, and anorthite on the other. I therefore proposed some time since to regard all of the intermediate felspars as mixtures of these two species, which, being homœomorphous, may be supposed to crystallize together in indefinite proportions. Multiplying and expanding the received formulæ of albite and anorthite, I represented them as follows (silica being  $\text{SiO}$ , and alumina  $\text{alO} = (\text{Al}^2 \text{O}^3) \div 3$ )\*:—

		Eq. wt.	Density.	Eq. vol.
Albite	. . . $(\text{Si}^{48} \text{al}^{12} \text{Na}^4)\text{O}^{64}$	= 1054.4	÷ 2.62	= 402.4
Anorthite	. . . $(\text{Si}^{32} \text{al}^{24} \text{Ca}^8)\text{O}^{64}$	= 1118.4	÷ 2.72	= 405.0.

The composition and density of the intermediate felspars permit us to regard them, for the most part, as mixtures of a soda-albite and a lime-anorthite. In the analyses of many albites and anorthites, however, we have evidence of similar admixtures; for some albites contain from 1 to 2.5 per cent. of lime, and anorthites from 3 to 4 per cent. of alkalis. Of a like significance is the constant presence of a small amount of potash with the soda of these felspars, and the magnesia, sometimes amounting to 5 per cent. in anorthite, leading us to infer the existence of lime and potash-albites (orthoclase?), and soda and magnesia anorthites. The difficulties presented by the varying composition of these felspars are obviated by admitting such a mixture of species as constantly takes place in the crystallization of homœomorphous salts from mixed solutions, and this consideration should never be lost sight of in the study of mineral chemistry.

It was not until after I had published this view of the constitution of the triclinic felspars (a view which must also be ex-

\* American Journal of Science, 2nd series, vol. xviii. p. 270.



tended to the scapolites), that I became aware that Th. Scheerer had recently proposed to regard all feldspars as combinations of anorthite with labradorite, anorthite with albite (orthoclase), or labradorite with albite\*. It was gratifying to find that this distinguished chemist had already arrived at a solution of the problem of the feldspars not unlike my own, but I must object to admitting labradorite as a distinct species, or as having any higher value than oligoclase, andesine, or the feldspars X. and XV. in the present paper.

Montreal, Canada,  
March 1855.

LI. *Note on a doubtful point in Climatology.*

By JOHN BALL, Esq., M.P.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

IN common with many persons who have paid attention to glacier phenomena, I have often found it difficult to reconcile the mass of testimony which seems to indicate the continued increase and accumulation of ice and snow in certain regions of the earth, with the equally strong evidence which goes to prove that the general climate of the inhabited portions has not sensibly varied throughout the historic period. Having lately had occasion to discuss the subject with somewhat stricter accuracy, I think that I am enabled to offer an explanation of the difficulty which I am desirous of submitting to the judgement of your readers.

I may commence by observing, that in the course of frequent visits to the higher regions of the European Alps, I had been struck by appearances which indicate that, even at the present time, there are certain spots of limited extent in which this process of accumulation still proceeds, and where, if we only admit that at some period since the glacial epoch the mean temperature of this portion of the earth was slightly greater than it now is, simple physical considerations show that such accumulation must have ensued. This, in fact, merely amounts to the assertion, that there are natural reservoirs or plateaux in the upper regions of the Alps where the annual increase of *névé* or consolidated snow is greater than the annual loss by melting, evaporation, and outflow of glacier towards the lower regions. Several such instances might, I think, be pointed out in the central

\* Poggendorff's *Annalen*, vol. lxxxix. p. 19, cited in Liebig and Kopp's *Jahresbericht*, 1853, p. 805.

chain, and elsewhere in the Alps; as, for instance, the plateau, about 13,000 English feet above the sea, which lies between the highest ridge of the Monte Rosa, the Zumstein Spitz, and the eastern end of the Lys Kamm, or the extensive and slightly inclined slopes which lie to the east of the Pass of St. Théodule. It would seem as if the existence of such centres of accumulation would alone serve to explain the fact established by the unanimous current of history and tradition, which affirm the gradual extension and advance of the great glaciers throughout the entire chain of the Alps, simultaneously with a general constancy in the climatal conditions of the surrounding countries. A few unimportant exceptions may be pointed out, such as that of the glacier of Findelen near Zermatt, which has retrograded for the last forty or fifty years, while the Gorner glacier originating in the same ice-fields has advanced to more than a proportionate extent. These cases may probably be explained by landslips altering the direction of the upper portion of certain glacier streams, but they cannot invalidate the universal consent of all available evidence as to the general extension of the glaciers. For this we rely, not merely upon concurrent tradition and documentary evidence, but on the unimpeachable declarations of those ancient tracks and paved footways which we now find invaded by ice and *névé*.

But if I do not mistake, there is evidence of a similar phænomenon on a much larger scale in the polar regions. Some of this evidence afforded by vegetable and animal remains in regions now chiefly covered with snow and ice may be open to question as to its true date, but it appears to me that the remarkable and well-known facts proving a local diminution of temperature in the plains of Iceland and Greenland, extending over the last 700 or 800 years, may far more easily be explained by the continuous accumulation of ice in the upper portions of those and the adjoining countries, than by any other hypothesis with which I am acquainted. It is true that, in regard to Iceland, the change of climate which has gradually driven out the cultivation of grain and reduced the tree vegetation to the dwarf birch scrub which alone now remains, may be in some degree connected with the enormous outburst of lava in the eruption of the Skaptar Yokul, substituting in the interior of the island a vast tract of surface of much greater radiating power; but it is questionable whether, under skies so charged with clouds as those of Iceland are in summer, this explanation can serve much, while the arbitrary assumption of a change in the force and direction of the Gulf-stream is scarcely reconcilable with the absence of any sensible change in the coast climate of north-western Europe. This latter explanation, again, can still less easily be applied to

account for the change of temperature in Greenland and in the northern part of Baffin's Bay.

But then comes the question, how it is possible to reconcile this extension of ice and snow in certain regions with the general constancy of the mean temperature of the earth, which may be inferred from historical evidence proving that the limits of cultivation of the best known vegetable products have not perceptibly altered for the last 3000 years, or even a much longer period. If no cosmical causes have interfered to alter the annual supply of heat received by the earth, and if no important change has occurred in the distribution of land and sea, it may be thought impossible that any reduction should occur in the general mean temperature, such as would be implied in the asserted cooling down of some districts, without a corresponding increase of temperature elsewhere. To this I venture to reply, that the increase of alpine glacier and polar ice does imply *an increase of mean temperature in those spots where the accumulation originates*. To return to the Alps from whence I started, I assert that a reservoir wherein the annual deposit of snow exceeds the annual melting and outflow, must have a mean temperature below the freezing-point, that before the rock or soil was constantly covered with snow or ice its mean temperature must have been still lower, and that the result of the accumulation is to raise that mean temperature.

To illustrate this, let us suppose that, instead of the intense dryness that prevails in the frigid regions of northern Siberia, between the Lena and the Jenisei, the prevalence of moist winds should determine a great annual fall of snow, we cannot doubt but that an enormous accumulation of *névé* and ice would proceed for ages until a condition was produced similar to that of the antarctic polar lands, the mean temperature of the region itself would be considerably increased, while that of some neighbouring districts would be diminished, the general mean for the whole earth and for countries not within reach of the immediate operating causes remaining unaltered.

Not having seen this question anywhere satisfactorily discussed, I have offered these somewhat crude considerations to the examination of those interested in its decision, and have the honour to remain, Gentlemen,

Your obedient Servant,

JOHN BALL.

LIII. *Further Researches into the Properties of the Sulphate of Iodo-quinine or Herapathite, more especially in regard to its Crystallography, with additional facts concerning its Optical relations.* By WILLIAM BIRD HERAPATH, M.D.\*

[With a Plate.]

**A**FTER referring to the observations of Professors Stokes† and Haidinger‡, as well as to papers already published by himself on this subject in the Philosophical Magazine, the author gives an account of a set of prisms perfectly complementary in their optical characters to those previously described by him, and proves this fact by an elaborate comparison of their various optical relations; from which it appeared, that whilst the  $\alpha$ -prisms (those described in the Philosophical Magazine for March 1852) were totally impervious to a beam of polarized light, reflected from glass plates, when the plane of the length of the prism was at right angles to the plane of primitive polarization, the  $\beta$ -prisms (those now examined) were equally absorbent of a vertically polarized beam, when the plane of their length lay parallel to that of primitive polarization. See Plate III. diagram II.

These prisms may be readily made by the following process:—

Dissolve 10 grains of disulphate of quinine in half a fluid ounce of spirit wine, having 3 grains of benzoic acid dissolved in it also; add 2 drachms of water and warm the whole to complete solution, then upon adding a few drops of spirituous solution of iodine and placing in repose, prismatic crystals having the following properties are produced:—

1. When two are crossed in superposition at right angles to each other or even at  $30^\circ$ , the overlapped space is “black” if the crystals are sufficiently thick, but reddish-brown or violet-red if very thin; in these respects they perfectly coincide with the  $\alpha$ -prisms. Pl. III. diagram III.

2. When examined by polarized light reflected from glass, the *vertical* crystals now appear black, whilst the horizontal prisms are almost perfectly transparent to the polarized beam. Pl. III. diagram II.  $\beta$ .

3. Upon introducing the selenite stages beneath the prisms, those which previously obstructed the polarized beam and appeared “black,” transmitted the “red” or “blue” colours of the selenite films, whilst the transparent, horizontal prisms exhibited the complementary colours, viz. the green or yellow. Pl. III. diagram II. fig.  $\beta$ .

\* Reprinted from the Abstracts of the Papers communicated to the Royal Society of London.

† In a paper read at Belfast before the British Association (Athen. Sept. 1852) by Prof. Stokes.

‡ In Poggendorff's *Annalen*, June 1853, and *Phil. Mag.* Oct. 1853, by M. Haidinger.

The last two completely decided their complementary character, for whilst the  $\beta$ -prisms were "black," "red" or "blue," when the planes of their length were *vertical*, the  $\alpha$ -prisms were "black," "red" or "blue" whilst the planes of their length were *horizontally* placed. Pl. III. diagram II. figs. 1, 2, 3,  $\alpha$  &  $\beta$ .

4. But by mixing the two prisms upon one slide their perfectly complementary character was completely demonstrated, for when an  $\alpha$ -prism was crossed at right angles by a  $\beta$ -prism, the square space where they overlapped was still transparent to common light; but if a  $\beta$ -prism were overlapped by an  $\alpha$ -prism, the length of the two being parallel to each other, the oblong space where they overlapped became black from their doubly absorbent properties; in short, the optical examination fully proved the complementary character of these crystals length for length. Pl. III. diagrams II. IV. V. & VI.

It was further shown that two varieties of hexagons existed; those produced from strong spirituous solutions were longer than broad, and possessed two acute angles of  $65^\circ$  and four equal angles of  $147^\circ 30'$ . Examined by a vertically plane-polarized beam, these were "black" when a line passing through the long diameter or acute angles was parallel to the plane of primitive polarization: these the author called "acute" or  $\beta$ -hexagons. The hexagons produced from acetic acid solutions, on the other hand, were "obtuse" in their character, and had two angles of  $115^\circ$  and four equal angles of  $122^\circ 30'$ . These, when examined by vertically plane-polarized light, were always "black" when a line passing through the two angles of  $115^\circ$  was perpendicular to the plane of primitive polarization: the author called these "obtuse" or  $\alpha$ -hexagons. Pl. III. diagram I.  $\alpha$ ,  $\beta$ .

The rhombic crystals which presented themselves were also "black" (examined in the same way) when their long diameters lay parallel to the plane of primitive polarization, but transparent when they were rotated  $90^\circ$ . Pl. III. diagram I. figs. 1, 2, 3, & VI.

It was also shown that the doubly absorbent powers of the primary rhombic crystal extended more or less on each side of the vertical position, through an arc of  $60^\circ$ , so that upon rotating a crystal through the whole circle, there were two arcs of  $120^\circ$ , in which the crystals were more or less darkened, and two of  $60^\circ$  each, in which the light passed through without loss; this transparency was of course at its maximum when the longer diameter of the rhombic plate lay in a horizontal position, and gradually became less evident through an arc of  $30^\circ$  above and below this line. By a careful measurement of the angles of the various crystalline forms and by the results of their optical examination, it appeared that they may all be obtained from the right rhombic prism, the acute angle of which is  $65^\circ$  and the obtuse

115°, the major axis being to the minor as 1·57 to 1, whilst the prism itself had scarcely appreciable length. It was a mere rhombic scale; but considered as a short prism, the axes were  $P^a_{\frac{a}{0\cdot00001}}$ ,  $M^a_{\frac{a}{1\cdot57}}$ ,  $T^a_{1\cdot0}$ , the quantity for  $P^a$  not being absolutely determinable in consequence of its variability and minuteness. Pl. III. diagram VII. fig. 1.

It appeared also that the  $\alpha$ -prism and obtuse hexagon were the results of truncation of the *acute* angles of the primary rhombic plate or prism by planes at right angles to the plane of primitive polarization, when the crystals were "black," when examined by vertically plane-polarized light; whilst the  $\beta$ -prism and acute hexagon resulted from truncation of the *obtuse* angles of the same primary form by planes parallel to that of primitive polarization, the crystals being "black" when examined by a vertically plane-polarized beam; and that the octagonal and square plates, and rectangular parallelogram resulted from the coincidence of these truncation planes in the same individual form. Pl. III. diagrams I. II. & VI.

It was shown that the solvent medium had the power of developing these truncation planes, and it appeared that *water* produced the  $\alpha$ -truncation, and *spirit* the  $\beta$ -truncation; and if the two opposing forces of water and spirit were made equal in intensity, they neutralized each other, so that the pure primary rhombic prism resulted without truncation.

It was further shown that hyponitric æther developed other crystalline forms, converting the rhombic plate into one of 75° and 105° by truncation planes upon the acute angles of the primary rhombic plate, cutting off portions equal to half the long axis, and leaving the shorter or transverse axis untouched. By this means the new rhombic crystal appeared "black" when the longer diameter was at right angles to the plane of vertically polarized light, as if it had been rotated through 90°, whilst it absorbed the polarized light as before. Pl. III. diagram VII. fig. 2.

As from the examination of certain rectangular quadrilateral prisms of the  $\alpha$  and  $\beta$  varieties, it appeared that Herapathite possesses doubly absorbent powers of nearly, if not perfectly equal intensity, in directions coincident with all three rectangular axes  $P^a$ ,  $M^a$  and  $T^a$ , the author inferred that the development of their optic axes could not be reasonably expected in these directions. He considered that the biaxial systems of rings would be found to exist in thicker crystals (when discovered), having triangular replacement planes upon the solid angles, either of the shorter or longer oblique diameters of the right rhombic prism, and the results of these optic axes would be seen in a direction perpendicular to these surfaces of replacement. Pl. III. diagram VII. figs. 3 & 4.

The various formulæ necessary to produce different crystalline forms are given in detail.

The author enters into a re-discussion of the double refractive powers, and attempts to show that Herapathite possesses a principal axis having "positive" characters. He also endeavoured to measure the thickness of certain very thin plates, by the differential tints produced by the exercise of their doubly refracting powers upon the colours of the selenite stages, and found that these thin plates raised the blue of the second order to the yellow and violet; the red of the second to the violet and blue-gray; and the green of the same to orange and violet of the second order. Assuming, therefore, this substance to have similar doubly refracting powers to selenite, and of equal intensity, and also the same index of refraction, their thickness may be estimated as equal to that of a plate of selenite necessary to give the same difference in tint, viz.  $\cdot 000517$  of an inch.

These very thin plates no longer possessed the Cantharides-like or brilliant metallic green colour when examined by reflected light. They appeared more like portions of the elytra of the *Blatta germanica*; still reflecting a polarized beam, but of a brown colour, and also retaining their doubly absorbent powers when examined by transmitted light.

The author considers that his recent investigations indicate that whatever properties Professor Stokes may have assigned to the ray reflected from the  $\alpha$ -prisms (which alone were experimented on by him) in the principal plane of the breadth, must be equally true in that reflected from the primary rhombic prism in the principal plane of its length, *i. e.* its long diameter; and whatever properties he may have assigned to the reflected ray from the principal plane of the length of the  $\alpha$ -prism, will be equally true of that reflected from the primary rhombus in the principal plane of the breadth, *i. e.* its short diameter.

The paper concludes with some recently determined facts relating to the chemical characters of this peculiar substance.

Its sp. gr. at  $60^\circ$  Fahr. was found to be 1.895.

It is very sparingly soluble in æther, turpentine or water at the ordinary temperatures, scarcely more than  $\frac{1}{2000}$ th part; boiling water does not dissolve  $\frac{1}{1000}$ th part. It does not appear more soluble in æther or turpentine by boiling. Chloroform does not dissolve it, but has a great attraction of surface for it.

*Alcohol* of  $\cdot 837$  dissolves  $\frac{1}{200}$ th part at  $57^\circ$  Fahr., and by boiling, it takes up  $\frac{1}{30}$ th part, which readily crystallizes on cooling in  $\beta$ -hexagons.

*Acetic acid*, sp. gr. 1.042, dissolves  $\frac{1}{730}$ th part at  $60^\circ$  Fahr., and  $\frac{1}{60}$ th part at boiling temperature—iodine volatilizing if the heat is prolonged.

*Diluted sulphuric acid*, sp. gr. 1·0682, does not dissolve it at the ordinary temperatures, but does so readily with the aid of heat. *Sulphuric acid*, sp. gr. 1·845, rapidly dissolves it.

*Diluted hydrochloric acid* has but little action on it, but when concentrated, it almost instantly reddens, without dissolving it; upon boiling, it forms a yellow solution, from which minute, dark, opaque, radiating aciculæ deposit on cooling.

*Nitric acid* immediately decomposes it, even in the cold; upon raising the temperature, iodine at first volatilizes; then nitrous acid vapours are evolved. Iodine is probably partially converted into iodic acid.

*Hydro-sulphuric acid* passed through its alcoholic or acetic acid solution at once decomposes it, converting the iodine into hydriodic acid, with separation of sulphur.

*Alkalies and alkaline earths* in solution at once decompose it, removing the sulphuric acid and leaving a Naples yellow residue containing the quinine and a portion of the iodine; a soluble iodide of quinine is also formed in ammoniacal liquids.

LIII. *The Theory of the Moon's Motion.—Results of a Third Approximation.* By the Rev. Professor CHALLIS, M.A., F.R.S.\*

IN two previous communications I have shown in what manner terms that may increase indefinitely with the time are introduced into the development of the radius-vector, and what processes are proper for avoiding them. That this is an important point in the lunar theory is evident from the circumstance, that the occurrence of such terms has in a great measure regulated the course which the solution of the problem has taken. It has been thought necessary, in order to get rid of them, to introduce *à priori* into the investigation expressions suggested by the observed motions of the nodes and apses of the moon's orbit. (See Pontécoulant, *Théorie de la Lune*, Chap. I. No. 5.) But it is certain on general principles that there must be a course of reasoning by which the expressions appropriate to the problem may be developed without the necessity of any assumption respecting their form; and as the solution I have commenced in this Magazine appears to be of this kind, I propose now to carry it on to a higher approximation. Although this method may lead to no numerical results differing in any sensible degree from those already obtained, I cannot but think that it is worth while to give to a problem of so great interest and importance as logical a solution as possible.

For the sake of clearness, I shall first recapitulate the previous

\* Communicated by the Author.



part of the argument, and add such additional elucidation as it may seem to require. In the Supplement to the Philosophical Magazine for December 1854 (p. 521), an exact first integral of the differential equations is obtained by neglecting the eccentricity of the sun's orbit. Then, after neglecting also the inclination of the moon's orbit to the plane of the ecliptic, two equations (*a*) and (*b*) are found, which, if we omit terms multiplied by the first and higher powers of the ratio of the moon's radius-vector to the sun's mean distance, reduce themselves to the following:—

$$\frac{dr^2}{dt^2} + r^2 \frac{d\phi^2}{dt^2} + C_1 - \frac{2m'}{a'} = \frac{2\mu}{r} + \frac{3n'^2 r^2}{2} (1 + \cos 2\phi) \quad . \quad (A)$$

$$d \cdot r^2 \left( \frac{d\phi}{dt} + n' \right) = - \frac{3n'^2 r^2}{2} \sin 2\phi dt. \quad . \quad . \quad . \quad (B)$$

In these equations *r* is the moon's radius-vector at the time *t*, and, *θ* being the moon's true longitude at the same time, *φ* is the excess of *θ* above *n't + ε'* the sun's mean longitude; *μ* is the sum of the attractions of the earth and moon, and *m'* is the sun's attraction, at the unit of distance; *a'* is the sun's mean distance, so that *a'<sup>3</sup>n'<sup>2</sup> = m'*; and *C<sub>1</sub>* is the constant introduced by the integration above mentioned.

The above limitations are made for the sake of simplicity, my object being to exhibit a method of solution rather than to obtain numerical results. By taking account of the eccentricity of the sun's orbit and terms of the fourth order containing the ratio of *r* to *a'*, the investigation becomes much more complicated, but involves no processes differing in principle from those required under the proposed limitations. The terms depending on the moon's latitude may be considered separately on a future occasion.

The equations (A) and (B) not admitting of exact integration, are to be integrated approximately, and the approximation is to proceed both according to the eccentricity of the orbit and the disturbing force. Hence the first approximation to the orbit is a circle described by the action of the central force  $\frac{\mu}{r^2}$ . If *a* be the radius of the circle, it will be readily seen that

$$a = \frac{\mu}{C_1}, \quad n^2 = \mu a^3, \quad \theta = nt + \epsilon.$$

These values are to be used for substitution in the small terms of (A) and (B) in order to proceed to the next approximation, which I have hitherto called the *first* approximation, and for the sake of uniformity shall continue to designate in the same way. In this approximation it is proposed to obtain values of the radius-vector and true longitude to the first order of small quantities.

Consequently the value of  $\frac{dr^2}{dt^2}$  must include all small quantities of the second order. I have shown that by merely making use of the equation  $\theta = nt + \epsilon$ , the following equation may be obtained,

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{n'^2 r^2}{2} + C = 0, \quad . . . . \quad (C)$$

in which C is put for  $C_1 - \frac{2m'}{a'} - 2n'h$ ,  $C_1$  and  $h$  being the arbitrary constants introduced by the integration. It may here be remarked, that as this equation was obtained by proceeding to small quantities of the second order, it is needless to retain the general value of  $r$  in the term  $-\frac{n'^2 r^2}{2}$ . Putting  $a$  for  $r$  in that term, and substituting  $C'$  for  $C - \frac{n'^2 a^2}{2}$ , we have

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} + C' = 0.$$

This equation gives by integration,

$$r = a(1 - e \cos \psi), \quad \frac{C'^{\frac{1}{2}}}{\mu} (t + T) = \psi - e \sin \psi,$$

$$a = \frac{\mu}{C'}, \quad e^2 = 1 - \frac{C' h^2}{\mu^2}.$$

Hence by substituting for  $C'$  its value, and putting  $m$  for the ratio of  $n'$  to  $n$ , it will be found that

$$a = \frac{\mu}{C} \left( 1 + \frac{m^2}{2} \right), \quad e^2 = 1 - \frac{C h^2}{\mu^2} + \frac{m^2}{2}.$$

But as the approximation gives the values of  $r$  only to the first order of small quantities, the above value of  $a$  is not true to small quantities of the second order, and in fact is not confirmed by the next approximation. Hence we have simply  $a = \frac{\mu}{C}$ .

It is evident that the orbit, as determined by this first approximation, is a fixed ellipse of arbitrary eccentricity  $e$ , and that if  $\varpi$  be the longitude of the perihelion, the values of the radius-vector and true longitude are the following:

$$r = a(1 - e \cos(nt + \epsilon - \varpi))$$

$$\theta = nt + \epsilon + 2e \sin(nt + \epsilon - \varpi).$$

These values are to be substituted in the small terms of the equations (A) and (B) to proceed to the second approximation.

In the Supplement to the Philosophical Magazine for December

1854 (p. 527), I have shown how by including small quantities of the third order, the following equation is obtained :

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{n^2 r^2}{2} + C = n^2 a^2 e (3 \cos(2q-p) - \cos(2q+p)), \quad (C')$$

$q$  being put for  $nt + \epsilon - \overline{nt + \epsilon'}$ , and  $p$  for  $nt + \epsilon - \varpi$ . Now the mean apsidal distances may be defined to be the values of  $r$  for which the part of  $\frac{dr^2}{dt^2}$ , which is independent of the sun's longitude, vanishes, the mean distance to be the mean between the two apsidal distances, and the mean eccentricity to be the ratio of the difference between the mean apsidal distances to their sum. Hence by equating to zero the left-hand side of the above equation, we have the equation of an orbit in which the mean distance and eccentricity are the same as the mean distance and mean eccentricity of the actual orbit, and which is proper for determining the motion of the apsides. Putting the equation under the form

$$r^2 \frac{dr^2}{dt^2} + h^2 - 2\mu r - \frac{n^2 r^4}{2} + Cr^2 = 0,$$

it will be seen that the integration cannot be exactly performed if the general value of  $r$  be retained in the small term. The equation might, however, be integrated by conducting the approximation solely according to the disturbing force, in the manner I have exhibited in the Philosophical Magazine for last February (p. 132). But that process would here be illegitimate, because the equation (C') was obtained by approximating according to the eccentricity as well as the disturbing force. The following is the process required by the strict rules of approximation. If  $a$  be the mean between the two apsidal distances, and  $r = a + v$ , by hypothesis  $v$  will be a small quantity. Hence put  $a + v$  for  $r$  in the foregoing equation, and in the small term expand to the first power of  $v$ , because the approximation includes small quantities of the third order. Or, what is equivalent, simply put  $a + r - a$  for  $r$  in the small term, and expand to the first power of  $r - a$ . The equation will then become

$$r^2 \frac{dr^2}{dt^2} + h^2 - 2\mu' r + Cr^2 = 0, \quad . . . . (D)$$

where  $h^2 = h^2 + \frac{3n^2 a^4}{2}$ , and  $\mu' = \mu + n^2 a^3$ . By integration this equation gives

$$r = a(1 - e \cos \psi), \quad \frac{C^{\frac{3}{2}}}{\mu'}(t + T) = \psi - e \sin \psi,$$

$$a = \frac{\mu'}{C}, \quad e^2 = 1 - \frac{Ch^2}{\mu'};$$

and if  $cn = \frac{C^{\frac{3}{2}}}{\mu'}$ , the apsidal motion in one revolution of the moon from an apse to the same apse again is  $2\pi(1 - e)$ .

These inferences having been drawn from the mean orbit defined by the equation (D), we may proceed to integrate the equation (C') in the manner I have employed in the Supplement to the Philosophical Magazine for December 1854, the quantity  $p$  now representing  $cnt + \epsilon - \varpi$ . The resulting value of  $r$  to small quantities of the second order is as follows:—

$$r = a \left( 1 - e \cos p + \frac{e^2}{2} - \frac{e^2}{2} \cos 2p - m^2 \cos 2q \right).$$

Also since to the same approximation

$$\frac{d\theta}{dt} = \frac{h}{r^2} + \frac{3n^2}{4n} \cos 2q,$$

we have by integration,

$$\theta = \epsilon + \frac{h}{a^2} \left( (1 + e^2)t + \frac{2e}{cn} \sin p + \frac{5e^2}{4cn} \sin 2p + \frac{m^2}{n} \cos 2q \right) + \frac{3m^2}{8} \cos 2q.$$

Hence by the definition of  $n$ ,

$$n = \frac{h}{a^2} (1 + e^2).$$

Also

$$\mu' = \mu \left( 1 + \frac{n^2 a^3}{\mu} \right) = \mu (1 + m^2)$$

$$h^2 = h^2 \left( 1 + \frac{3n^2 a^4}{2h^2} \right) = h^2 \left( 1 + \frac{3m^2}{2} \right);$$

because by the former approximation  $n^2 a^3 = \mu$ , and  $h = na^2$ . Consequently to small quantities of the second order,

$$a = \frac{\mu'}{C} = \frac{\mu}{C} (1 + m^2)$$

$$e^2 = 1 - \frac{h^2 C}{\mu'^2} = 1 - \frac{h^2 C}{\mu^2} + \frac{m^2}{2}$$

$$c = \frac{C^{\frac{3}{2}}}{\mu' n} = \frac{a C^{\frac{3}{2}}}{h \left( 1 + \frac{e^2}{2} \right)} = \frac{\mu}{h \sqrt{C}} \cdot \frac{1 + m^2}{1 + \frac{e^2}{2}}.$$

But by the above value of  $e^2$ ,  $\frac{h\sqrt{C}}{\mu} = 1 - \frac{e^2}{2} + \frac{m^2}{4}$  nearly. Hence it follows that

$$c = 1 + \frac{3m^2}{4}.$$

Here it is to be remarked, that we have arrived at a value of  $c$  which is not true to the second order of small quantities, the true value, as is well known, being  $1 - \frac{3m^2}{4}$ . The explanation of the above result is, that the approximation has not been carried far enough to determine the value of  $c$  to the second order of small quantities. The first term in the development of the radius-vector which contains  $c$  is  $e \cos (cnt + \epsilon - \varpi)$ ; and if, after putting  $1 + \alpha$  for  $c$ , this term be expanded according to powers of  $\alpha$ , the second term of the expansion contains the factor  $e\alpha$ , which if  $\alpha$  be of the second order, is of the *third* order. Consequently, as our approximation only extends to quantities of the second order in the development of  $r$ , it does not embrace the value of  $c$  to quantities of the second order; it only proves that that quantity contains no term of the first order. Hence, as far as this approximation shows,  $c = 1$ .

In the paper already referred to (p. 528), I have shown how, in the second approximation, to take account of terms which rise in value one order by integration, and have obtained the following complete values of  $r$  and  $\phi$  to the second approximation :

$$\frac{r}{a} = 1 - e \cos p + \frac{e^2}{2} - \frac{e^2}{2} \cos 2p - m^2 \cos 2q - \frac{15me}{8} \cos (2q - p)$$

$$\phi = q + 2e \sin p + \frac{5e^2}{4} \sin^2 p + \frac{11m^2}{8} \sin 2q + \frac{15me}{4} \sin (2q - p).$$

These are the values to be employed in proceeding to the third approximation.

Before entering upon the next approximation, I propose to indicate another method of obtaining the above values of  $r$  and  $\phi$ , which will be found to be convenient in the higher approximations, and adapted to the consideration of the terms that rise in order of value by integration. In this method it will first be necessary to inquire in what way terms of that kind are introduced. It is plain that they must be terms the argument of whose circular function is a multiple of  $q - p$ , and that, as shown by equations (A) and (B), such terms rise in value by integration when they occur either in the expression for  $r^2 \sin 2\phi$ , or in that for  $\frac{d\theta}{dt}$ . It may be readily shown that terms of the form  $\Lambda \cos 2p$  in the value of  $r$ , and of the form  $\Lambda' \sin 2p$  in the value of  $\phi$ ,

give rise to terms of the form  $M \cos 2(q-p)$  in the value of  $r^2 \sin 2\phi$ . Also a term of the form  $B \cos 2(q-p)$ , and of the third order in  $r$ , will produce a term of the same form and of the second order in the expansion of  $\theta$ . Let us, therefore, suppose the values of  $r$  and  $\phi$ , which are to be substituted in the small terms of the equations (A) and (B) for a second approximation, to be the following:

$$\frac{r}{a} = 1 - e \cos p + A \cos 2p + B \cos 2(q-p)$$

$$\phi = q + 2e \sin p + A' \sin 2p.$$

On going through the calculation, and retaining terms of the *fourth* order which involve the argument  $q-p$ , the following result is found for the equation C':

$$\begin{aligned} \frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{n^2 r^2}{2} + C = \\ n^2 a^2 \left\{ 3e \cos (2q-p) - e \cos (2q+p) + \right. \\ \left. + \left( \frac{51e^2}{8m} + \frac{3(A-A')}{2m} + \frac{57e^2}{8} \right) \cos 2(q-p) \right\}. \end{aligned}$$

In order to integrate this equation, assume

$$\frac{r}{a} = 1 - e \cos p + \Sigma . P \cos (\alpha q + \beta p),$$

and substitute in the first side of the equation. Then by making the terms of like argument identical, different corresponding values of  $P$ ,  $\alpha$ , and  $\beta$  are found, which determine the different terms of the development of  $r$ . Those of the development of  $\theta$  may then be obtained by integration. For the values  $\alpha=2$ ,  $\beta=-1$ , there results

$$P = -\frac{3m^2}{4e} \left( \frac{17e^2}{4m} + \frac{A-A'}{m} \right);$$

and as  $A$  is the coefficient of  $\cos 2p$  in the development of  $r$ , and  $A'$  the coefficient of  $\sin 2p$  in that of  $\theta$ , we have

$$A = -\frac{e^2}{2}, \quad A' = \frac{5e^2}{4}, \quad \text{and } P = -\frac{15me}{8}.$$

The corresponding coefficient in the development of  $\theta$  is plainly  $+\frac{15me}{4}$ . It appears also that the coefficient  $B$  is  $-\frac{15me^2}{16}$ , and that the coefficient of the term containing the argument  $2(q-p)$  in the expansion of  $\theta$  is reduced to zero.

*Third Approximation.*

Small quantities of the *fourth* order are now to be included. Hence from the complete values of  $r$  and  $\phi$  given by the second approximation, it is required to obtain values of  $r^2 \cos 2\phi$  and  $r^2 \sin 2\phi$  to the second order of small quantities for substitution in the right-hand sides of the equations (A) and (B). By the ordinary processes of approximation the following results will be obtained :

$$\begin{aligned} \frac{r^2}{a^2} \cos 2\phi &= \cos 2q + e(\cos(2q+p) - 3\cos(2q-p)) \\ &+ e^2 \left( -\frac{5}{2} \cos 2q + \cos 2(q+p) + \frac{5}{2} \cos 2(q-p) \right) \\ &+ me \left( \frac{15}{8} \cos(4q-p) - \frac{45}{8} \cos p \right) \\ &+ m^2 \left( -\frac{19}{8} + \frac{3}{8} \cos 4q \right) \end{aligned}$$

$$\begin{aligned} \frac{r^2}{a^2} \sin 2\phi &= \sin 2q + e(\sin(2q+p) - 3\sin(2q-p)) \\ &+ e^2 \left( -\frac{5}{2} \sin 2q + \sin 2(q+p) + \frac{5}{2} \sin 2(q-p) \right) \\ &+ me \left( \frac{15}{8} \sin(4q-p) - \frac{45}{8} \sin p \right) + \frac{3m^2}{8} \sin 4q. \end{aligned}$$

By substituting for  $r^2 \sin 2\phi$  in the equation (B), and then after integrating and squaring the result, replacing  $\frac{h}{r^2} - n'$  by its approximate value  $\frac{d\phi}{dt} - \frac{3n'^2}{4n} \cos 2q$ , the following equation is obtained :

$$\begin{aligned} r^2 \frac{d\phi^2}{dt^2} - \left( \frac{h}{r} - n'r \right)^2 &= \\ \frac{3n'^2 a^2}{2} \cdot \left\{ + \cos 2q + e \left( \frac{5}{3} \cos(2q+p) - 5 \cos(2q-p) \right) - \frac{5e^2}{2m} \cos 2(q-p) \right. \\ &+ e^2 \left( -\frac{47}{6} \cos 2q + \frac{29}{12} \cos 2(q+p) - \frac{9}{4} \cos 2(q-p) \right) \\ &+ em \left( \frac{7}{9} \cos(2q+p) - 5 \cos(2q-p) + \frac{25}{8} \cos(4q-p) - \frac{75}{8} \cos p \right) \\ &+ \frac{e^3}{m} \left( -\frac{5}{2} \cos(2q-p) - \frac{5}{2} \cos(2q-3p) \right) \\ &\left. + m^2 \left( \frac{19}{16} + \frac{11}{8} \cos 4q \right) \right\}. \end{aligned}$$

When the value of  $r^2 \frac{d\phi^2}{dt^2}$  given by this equation and the foregoing value of  $r^2 \cos 2\phi$  are substituted in the equation (A), there will be found on the right-hand side of the equation the following quantity which does not contain the sun's longitude, viz.

$$\frac{3n^2 a^2}{2} \left( -\frac{57m^2}{16} + \frac{15me}{4} \cos p \right).$$

After substituting for  $e \cos p$  its approximate value  $1 - \frac{r}{a}$ , this quantity is to be transferred to the left-hand side of the equation. Then putting  $a + r - a$  for  $r$  in the small terms, and expanding  $a + r - a$  so as to include small quantities of the fourth order, the final result is

$$\begin{aligned} & \frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu'}{r} + C' = \\ & \frac{3n^2 a^2}{2} \cdot \left\{ e \left( -\frac{2}{3} \cos(2q+p) + 2 \cos(2q-p) \right) + \frac{5e^2}{2m} \cos 2(q-p) \right. \\ & \quad + e^2 \left( \frac{16}{3} \cos 2q - \frac{17}{12} \cos 2(q+p) + \frac{19}{4} \cos 2(q-p) \right) \\ & \quad + em \left( -\frac{7}{9} \cos(2q+p) + 5 \cos(2q-p) - \frac{5}{4} \cos(4q-p) \right) \\ & \quad \left. + \frac{e^3}{m} \left( \frac{5}{2} \cos(2q-p) + \frac{5}{2} \cos(2q-3p) \right) - m^2 \cos 4q \right\} \quad (C'') \end{aligned}$$

where

$$h^2 = h^2 - \frac{3n^2 a^4}{2} - \frac{45n^2 m a^4}{4}$$

$$\mu' = \mu - 2n^2 a^3 - \frac{135}{16} n^2 m a^3$$

$$C' = C - 3n^2 a^2 - \frac{45n^2 m a^2}{8} + \frac{171m^2 n^2 a^2}{32}.$$

Consequently the equation applicable to the mean orbit is

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu'}{r} + C' = 0,$$

which gives by integration,

$$r = a(1 - e \cos \psi), \quad \frac{C'^{\frac{3}{2}}}{\mu'} (t + T) = \psi - e \sin \psi,$$

$$a = \frac{\mu'}{C'}, \quad e^2 = 1 - \frac{C' h^2}{\mu'^2};$$



and if  $cn = \frac{C'^{\frac{3}{2}}}{\mu'}$ , the apsidal motion in one revolution of the moon is  $2\pi(1-c)$ .

Since by the second approximation

$$\frac{a^4}{h^2} = \frac{1+e^2}{n^2}, \quad \frac{a^3}{\mu} = \frac{1-\frac{m^2}{2}}{n^2}, \quad \frac{a^2}{C} = \frac{1-\frac{3m^2}{2}}{n^2},$$

the foregoing values of  $h^2$ ,  $\mu'$ , and  $C'$  become

$$h^2 = h^2 \left( 1 - \frac{3m^2}{2} - \frac{45}{4}m^3 - \frac{3m^2e^2}{2} \right)$$

$$\mu' = \mu \left( 1 - 2m^2 - \frac{135}{16}m^3 + m^4 \right)$$

$$C' = C \left( 1 - 3m^2 - \frac{45}{8}m^3 + \frac{459}{32}m^4 \right).$$

In these values the terms of the *fourth* order are to be rejected, because the present approximation extends only to quantities of the third order. Hence it will be found that

$$a = \frac{\mu}{C} \left( 1 + m^2 - \frac{45}{16}m^3 \right)$$

$$e^2 = 1 - \frac{Ch^2}{\mu^2} + \frac{m^2}{2}.$$

*This result proves that the expression for  $e^2$  contains no term involving  $m^3$ .*

Again, in obtaining the value of  $c$ , terms of the *third* order are to be omitted, because, for reasons already adduced, this third approximation can give the value of  $c$  only to terms of the second order. Now to that order we have, as in the second

approximation,  $na^2 = h \left( 1 + \frac{e^2}{2} \right)$ . Hence

$$c = \frac{C'^{\frac{3}{2}}}{\mu'n} = \frac{C^{\frac{3}{2}}a^2}{\mu h} \cdot \frac{1 - \frac{9m^2}{2}}{\left( 1 + \frac{e^2}{2} \right) (1 - 2m^2)};$$

and as  $a^2 = \frac{\mu^2}{C^2} (1 + 2m^2)$  and  $\frac{\mu}{hC^{\frac{1}{2}}} = 1 + \frac{e^2}{2} - \frac{m^2}{4}$  nearly, it will be seen that

$$c = 1 - \frac{3m^2}{4},$$

which is the known approximation, obtained by a strictly deductive process.

It would lead me into too complicated calculations, to add to the equation ( $C''$ ) the terms of the fifth order which receive small denominators by integration, and to obtain the complete values of  $r$  and  $\theta$  to the third order of small quantities. I have indicated above a process proper for this purpose; and having made trial of this process to some extent, I found terms of the third order identical with those given in Pontécoulant's Lunar Theory. In fact, I have no reason to conclude that my solution differs in ultimate analytical expression from previous solutions. The order of the reasoning differs from that of any former method, and is, I think, strictly logical. There is also one important particular in which the reasoning itself is unlike that of any other method. I have commenced the process of approximation after obtaining an exact integral, which had not been previously discovered. On that integration, and on results to which it leads, depends the proof of the new theorem I have published respecting the relation of the eccentricity of the moon's orbit to the disturbing force. I propose to conclude the present communication with some further consideration of this point.

In the Philosophical Magazine for February (p. 135), I have adduced the following argument to show that the eccentricity of the moon's orbit has a special value determined by the disturbing force. The fluctuations of value of the radius-vector, as the moon returns in successive revolutions to the same longitude, depend on the disturbing force in such a manner that the change in each complete revolution would vanish if the disturbing force vanished. The total fluctuation, in the case of a uniform apsidal motion, is the same in all directions after a large number of revolutions of the moon, and depends both on the partial fluctuations and on the interval during which they are additive or subtractive, that is, on the rate of motion of the apses. Hence as the motion of the apses, as well as the partial fluctuations, would vanish with the disturbing force, it follows that the total fluctuation in any given direction, and consequently the eccentricity of the orbit, is a quantity which contains the disturbing force as a factor. This conclusion does not hold good when the force is wholly central, as the orbit in that case may be a circle, and the eccentricity is consequently not restricted to one value.

If the foregoing general argument be true, the same conclusion must be obtainable in a special manner in the course of the investigation of the moon's motion, and I am ready to admit that the new theorem is not established unless it can be so obtained. It will, therefore, be worth while to inquire how far the theorem receives support from the results of the third approximation. First, it may be remarked, that as it has been proved that the expression for  $e^2$  contains no term involving  $m^3$ , if the arbi-

trary constants  $C$  and  $h$  satisfy the equation  $Ch^2 = \mu^2$ , the consequent value of the eccentricity is the same as that given by the second approximation. Thus the third approximation is not contradictory to the second. At the same time it is incapable of establishing the truth of the theorem, as appears from the following considerations.

In the method of treating the lunar theory which I have proposed, no use is made, as in previous methods, of the radius-vector and longitude of an assumed revolving ellipse; but instead of this process, the terms containing the sun's longitude are separated from those which do not contain it explicitly, and the former being omitted in the values of  $\frac{dr^2}{dt^2}$  and  $\frac{d\theta}{dt}$ , equations are formed which define the orbit that I have called *the mean orbit*. These equations, as given by the third approximation, are the following:

$$\frac{dr^2}{dt^2} + \frac{h'^2}{r^2} - \frac{2\mu'}{r} + C' = 0, \quad \frac{d\theta}{dt} = \frac{h}{r^2}.$$

The development of  $\frac{d\theta}{dt}$  was found to contain an additional term independent of the sun's longitude, but as it is of the fourth order, it is omitted in the third approximation. By the above two equations the mean orbit is *proved* to be a revolving ellipse for which  $c = \frac{h'}{h}$ , and the value of  $h'^2$  already obtained shows that to the second order of small quantities  $\frac{h'}{h} = 1 - \frac{3m^2}{4}$ . It thus appears that the apsidal motion is deduced in the third approximation from equations which are the same in form as if the force were wholly central, and consequently, so far as this approximation shows, the eccentricity is arbitrary. But on proceeding to the *fourth* approximation, the term above mentioned must be taken into account, in consequence of which  $r^2 \frac{d\theta}{dt}$  is no longer constant for the mean orbit, and the force cannot be regarded as wholly central. The mean motion of the apse and the form of the mean orbit are now dependent in part on a *tangential* force; and it is perhaps not too much to assert, that under these circumstances the mean orbit cannot be a circle, and consequently that the eccentricity is not arbitrary. Again, the process by which I obtain the value of  $c$  appears to indicate that in succeeding approximations, this quantity will be a function of  $e$  and  $m$ . If so, it cannot be a function of  $m$  exclusively, unless the arbitrary constant  $C$  satisfy the condition  $Ch^2 = \mu^2$ . It would be necessary, however, in order to settle these points, to complete

the fourth approximation, which would require an amount of calculation that I am not prepared to undertake. I must content myself with having thrown out these suggestions.

If the equation  $Ch^2 = \mu^2$  be true, it follows as a consequence that the equations (C), (C'), (C''), &c. contain the disturbing force as a factor. In my first researches I endeavoured to show antecedently that this is the case. But on reviewing the reasoning on this point, given in the *Philosophical Magazine* for August 1854, and in the *Supplementary December Number* (p. 524), I find that the inconsistencies in the approximations to the mean distance and mean motion of the apse there spoken of, disappear when due regard is paid to the order of magnitudes proper to each approximation, and in consequence that the argument founded on the occurrence of those inconsistencies is invalid. This point is fully cleared up in the present communication.

Cambridge Observatory,  
April 16, 1855.

#### LIV. *Note on the Mineral Wilsonite.*

By T. S. HUNT, of the *Geological Commission of Canada*.\*

IN the *Supplementary Number* of this *Magazine* for July 1854, p. 499, I published descriptions and analyses of two minerals occurring in crystalline limestone in Bathurst, C.W., one of which I proposed as a new species with the name of Wilsonite. It was described as forming rose-red, translucent, cleavable masses, probably monoclinic in crystallization, with a hardness of 3·5, and a density of 2·765 to 2·776. Lustre vitreous, shining occasionally, pearly on the cleavage surfaces. One of three analyses gave silica, 43·55; alumina, 27·94; oxides of iron and manganese, 20; lime, 6·50; magnesia, 3·81; potash, 8·37; soda, 0·95; water (loss by ignition), 8·61=99·93. With two other specimens the amounts of silica, alumina, magnesia, and alkalis were very nearly the same, while the loss by ignition was 9·40 and 9·00, and the amounts of lime respectively 6·94 and 6·72.

The subsequent examination of more perfect specimens in the possession of Prof. E. J. Chapman has shown him that this mineral is triclinic, with inclination to the right, and he has kindly furnished the following notes:—"I give you as approximations, the following angles from cleavage faces by the common goniometer:—P : T = 94°; P : e = 145°; T : e = 129°; P : M and T : M = 110° to 115°. Cleavage with P and T perfect, less so with M; the cleavage with e is not easily obtained, but is very

\* Communicated by the Author.

distinct and even. Hardness, 3·5 on the more facile cleavages and their combination edges; 5·5 on the ends of the prismatic concretions. Density, 2·77.”

Prof. Chapman also pointed out to me the existence of intermingled carbonate of lime in the specimens which I had previously analysed, and showed that the loss by ignition was in part carbonic acid. I have since found that the finely pulverized mineral yields up nearly the whole of its lime with effervescence to cold dilute hydrochloric acid, which does not attack the silicate. This still retains its rose colour, and is a hydrous silicate of alumina and potash with a little magnesia. The following are the results of the analyses by myself of two specimens, purified by dilute acid (one being from the same mass as the one given above), and a partial analysis of a third specimen by Prof. H. Croft, which he has kindly communicated to me.

	I.	II.	III.
Silica . . . .	47·50	47·70	47·42
Alumina . . . .	31·17	31·22	}
Magnesia . . . .	4·25	4·14	
Lime . . . .	1·51	·39	·73
Potash . . . .	9·22	9·38	·42
Soda . . . .	·82	·95	
Water (by ignition)	5·50	5·35	
	99·97	99·13	

The mineral freed from carbonate of lime seems to be constant in composition; and from its crystallization and density, as well from its chemical constitution, to pertain to the felspars.

Montreal, March 22, 1855.

### LV. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 309.]

January 18, 1855.—Sir Benjamin Brodie, Bart., in the Chair.

**T**HE following communications were read:—

“On the Dots upon the Valves of the Diatomaceæ.” By J. W. Griffith, M.D., F.L.S.

In a former paper, it was stated that the markings or dots upon the valves of the Diatomaceæ, are the optical expressions of depressions existing upon the valves.

All those authors who have paid special attention to the Diatomaceæ, have considered the markings to denote cells; among these we find Ehrenberg\*, Kützing†, Ralfs‡, Smith§, and Quekett||.

\* Die Infusionsthierchen.

† Die Bacillarien, and Spec. Algarum.

‡ Annals of Nat. History, 1843.

§ British Diatomaceæ.

|| Histological Catalogue of the College of Surgeons; and Lectures delivered before the College of Surgeons.

The evidence I adduced in regard to the more coarsely marked Diatomaceæ, as *Isthmia*, &c., being furnished with depressions and not cells, is, I believe, satisfactory and conclusive; and this view has been admitted in a paper since read before the Royal Society\*.

A different view has been taken of the nature of the *finer* markings, as those upon some species of *Gyrosigma*, by the author of the paper last quoted, as by previous authors; and the object of this note is to direct attention to the support which the extended view argued for by me in the paper above referred to, viz. that the finer markings also correspond to depressions, derives from analogy.

The structure of the Diatomaceæ, and their modes of reproduction, are, as is well known, remarkable;—so much so, that these organisms have been claimed by botanists as members of the vegetable, and by zoologists as belonging to the animal kingdom. The preponderance of evidence is decidedly in favour of their vegetable nature; but, be this as it may, they must all be classed together,—they form a perfectly natural family. Hence we have a strong argument in favour of the markings upon their valves being identical, and as these are evidently depressions in the genera and species with coarsely marked valves (*Isthmia*, &c.), we should expect from analogy that the same would apply to those with finer markings. And this view receives further support, from the fact, that under varied methods of illumination, corresponding appearances are presented by the markings when viewed by the microscope, from those which are very large, as in *Isthmia*, through those of moderate and small size, as in the species of *Coscinodiscus*, down to those in which they are extremely minute, as in the species of *Gyrosigma*, &c. The angular (triangular or quadrangular) appearance assumed by the markings, arises from the light transmitted through the valves being unequally oblique. This may be readily shown in the more coarsely marked valves (*Isthmia*, *Coscinodiscus*), which present the true structural appearance when the light is reflected by the mirror in its ordinary position, and the spurious angular appearance when the light is rendered oblique by moving the mirror to one side.

“Researches on the Theory of Invariants.” By William Spottiswoode, M.A., F.R.S.

Invariants may be regarded from two points of view, the permutational and the functional. According to the former they are considered as arising from a process of permutation, according to the latter as derivatives from other functions. In this paper the latter course is adopted; and the following is an outline of the method:—

Let 
$$u = f(x, y, \dots, a_{\alpha\beta\dots}, a_{\alpha\beta\dots}, \dots)$$

be any homogeneous function of the degree  $n$  of the variables  $x, y, \dots$ , in which  $a_{\alpha\beta\dots}, a_{\alpha\beta\dots}, \dots$  multiplied by their respective multinomial coefficients are the coefficients of  $x^\alpha y^\beta \dots, x^{\alpha'} y^{\beta'} \dots, \dots$ . Let  $x, y, \dots$  undergo any linear transformation, and let  $l, m, \dots, l', m', \dots, \dots$  be the coefficients of transformation; also let

\* Proceedings of the Royal Society, June 15, 1854.

$$\Delta = \begin{vmatrix} l & m & \dots \\ l' & m' & \dots \\ \dots & \dots & \dots \end{vmatrix}$$

$$\left. \begin{aligned} \triangleright &= l' \frac{d}{dl} + m' \frac{d}{dm} + \dots \\ \triangleright' &= l'' \frac{d}{dl} + m'' \frac{d}{dm} + \dots \\ &\dots \end{aligned} \right\}$$

let  $a_{\alpha\beta\dots}$ ,  $a_{\alpha_i\beta_i\dots}$ , .. be the new values of  $a_{\alpha\beta\dots}$ ,  $a_{\alpha_i\beta_i\dots}$ , ...; also let

$$\nabla = l \frac{d}{dx} + m \frac{d}{dy} + \dots$$

$$\nabla' = l' \frac{d}{dx} + m' \frac{d}{dy} + \dots$$

then, à une facteur près,

$$a_{\alpha\beta\dots} = \nabla^\alpha \nabla'^\beta \dots u$$

and

$$\frac{da_{\alpha\beta\dots}}{da_{\alpha\beta\dots}}, \frac{da_{\alpha\beta\dots}}{da_{\alpha_i\beta_i\dots}}, \dots$$

will be respectively equal to the combinations of  $l, m, \dots, l', m', \dots, \dots$  by which the  $a$ s are multiplied in  $a_{\alpha\beta\dots}$ . Again, let  $f^{\wedge}$  represent a function similar to  $f$ , excepting that in the former the multinomial coefficients are dropped; then if

$$\square_{no\dots} = f^{\wedge} \left( l, m, \dots, \frac{d}{da_{\alpha\beta\dots}}, \frac{d}{da_{\alpha_i\beta_i\dots}}, \dots \right)$$

$$\square_{\alpha\beta\dots} = \frac{\triangleright^{n-\alpha} \triangleright'^\beta \dots}{1.2\dots(n-\alpha).1.2\dots\beta\dots} \square_{no\dots}$$

Now the fundamental condition of invariance is expressed by the following equation:—

$$\Delta^{\frac{mn}{q}} F(a_{\alpha\beta\dots}, a_{\alpha_i\beta_i\dots}, \dots) = F(a_{\alpha\beta\dots}, a_{\alpha_i\beta_i\dots}, \dots),$$

$$= F(a_{\alpha\beta\dots} \square_{\alpha\beta\dots}, a_{\alpha_i\beta_i\dots} \square_{\alpha_i\beta_i\dots}, \dots) U,$$

where  $U = F(a_{\alpha\beta\dots}, a_{\alpha_i\beta_i\dots}, \dots)$ ,

and the problem consists in determining the coefficients of  $U$ . This gives

$$\Delta^{\frac{mn}{q}} (\overline{\alpha\beta\dots}^i, \overline{\alpha_i\beta_i\dots}^i, \dots) = \square_{\alpha\beta\dots}^i \square_{\alpha_i\beta_i\dots}^i \dots U,$$

the quantity within brackets being the coefficient of

$$a_{\alpha\beta\dots}^i a_{\alpha_i\beta_i\dots}^i \dots$$

Then if the term be selected in which  $i=i_1=\dots=1$ , and if  $\Delta^{\frac{m\pi}{q}}$  be so transformed that it shall appear as a function linear in each of

the systems

$$\frac{da_{\alpha\beta..}}{da_{\alpha\beta..}} = (00), \quad \frac{da_{\alpha\beta..}}{da_{\alpha_i\beta_i..}} = (01)..$$

$$\frac{da_{\alpha_i\beta_i..}}{da_{\alpha\beta..}} = (10), \quad \frac{da_{\alpha_i\beta_i..}}{da_{\alpha_i\beta_i..}} = (11)..$$

and if

$$\alpha\beta.. \Pi = a_{\alpha\beta..} \frac{d}{d(00)} + a_{\alpha_i\beta_i..} \frac{d}{d(01)} + ..$$

$$\alpha_i\beta_i.. \Pi = a_{\alpha\beta..} \frac{d}{d(10)} + a_{\alpha_i\beta_i..} \frac{d}{d(11)} + ..$$

then

$$\alpha\beta.. \Pi \alpha_i\beta_i.. \Pi .. \Pi \alpha\beta.. \Pi \alpha_i\beta_i.. .. = 1$$

and U will be at once given by the equation

$$U = \overline{(\alpha\beta.., \alpha_i\beta_i.., ..)}_{\alpha\beta.. \alpha_i\beta_i.. \Pi} \Delta^{\frac{mn}{q}}.$$

This method has been applied in the case of two variables to the calculation of quadratic and cubic invariants.

But in the case of two variables the coefficient  $a$  may be expressed by a series of symbols with a single suffix, thus :

$$a_0, a_1, .. a_0, a_1, .. \quad \Pi_0, \Pi_1, .. \quad 0 \Pi, 1 \Pi, ..$$

Now since  $\Delta$  is of the same degree in  $l, m$ , and in  $l', m'$ , the coefficients of all powers and products of  $a_0, a_1, ..$  in which the degree of  $l, m$  is above or below that of  $l', m'$ , will vanish in the invariant. And from this and some other considerations it is shown, that not only

$$\Pi_0^m U = 0, \quad \triangleright \Pi_0^m U = 0, \quad .. \quad \triangleright^{\frac{mn}{q}-1} \Pi_0^m U = 0,$$

but that the coefficients of the invariant may be calculated from the equations arising from equating to zero each term of the last equation above written, operated upon by the symbol  $\triangleright$ . This always gives a system of linear equations for finally determining the coefficients. It does not appear possible fully to explain this method without entering into more details than can be given in an abstract.

From the general equations for so determining the coefficients, the number and degrees of the distinct invariants belonging to any given function may theoretically be determined; and this has been done for the simplest case, viz. quadratic functions. But the expressions for higher degrees appear so complicated that an answer to this important question can hardly be expected from this method, in any case not already known.

The view of invariants here taken has suggested a series of other functions of which invariants form the last term. These functions, which I propose to call Variants, may be thus expressed. If functions of the degrees  $r, s, ..$  ( $r, s, ..$  being less than  $n$ ) have invariants of the degree  $m$ , then writing



$$\partial = \frac{d}{dl}, \quad \partial_r = \frac{d}{dm}$$

$$\Delta^{\frac{mr}{2}} F\left(\partial^r a_0, \frac{1}{r} \partial^{r-1} \partial_r a_0, \dots \partial_r^r a_0\right) = F_{m, n-r}$$

$$\Delta^{\frac{ms}{2}} F\left(\partial^s a_0, \frac{1}{s} \partial^{s-1} \partial_s a_0, \dots \partial_s^s a_0\right) = F_{m, n-s}$$

$$\Delta^{\frac{mn}{2}} F\left(\partial^n a_0, \frac{1}{n} \partial^{n-1} \partial_n a_0, \dots \partial_n^n a_0\right) = F_{m, 0}$$

of which the last is a simple invariant, since, omitting the factor  $n(n-1) \dots 2.1$ ,

$$\partial^n a_0 = a_0, \quad \frac{1}{n} \partial^{n-1} \partial_n a_0 = a_1, \quad \dots \partial_n^n a_0 = a_n.$$

With the exact relation between these functions and covariants I am not at present acquainted.

Jan. 25, 1855.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

“On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; preceded by an Account of some Experiments on Molecular Influences.”—The Bakerian Lecture. By John Tyndall, Ph.D., F.R.S.

The paper commences with an introduction, in which the present aspect of the portions of science to which it refers is briefly sketched. A section is devoted to the examination of the magnetic properties of wood, which substance, the author finds, except where extraneous impurities are present, to be always diamagnetic, and to set in the magnetic field with its fibre equatorial. The influence of the shape of pointed and flat poles is studied, and those curious phenomena of rotation, first observed by M. Plücker, and attributed by him to the action of two conflicting forces, are referred to molecular structure as a cause. Between flat poles, it is proved that the line joining the centres of the two poles is the line of *minimum* force; that is to say, the force increases more quickly from the central point of the magnetic field in an equatorial direction than in an axial one. Reflecting on the great diversity of opinion at present existing with regard to the real nature of the diamagnetic force, the author deemed it necessary to commence at the foundation of the inquiry. A fundamental question in the present case is the following:—Are diamagnetic bodies repelled by a magnet in virtue of any constant property possessed by the mass? Is the force in question a mere repulsion of ordinary matter, or is the repulsion exercised in virtue of a state of magnetization into which the body is first thrown? This question is answered in a manner which admits of no doubt.

It is proved that the repulsion of diamagnetic bodies increases in a quicker ratio than the strength of the magnet which produces the repulsion. Within wide limits, indeed, the repulsion, instead of

being simply proportional to the strength of the magnet, is proportional to the square of the strength, which leads inevitably to the conclusion that the body thus repelled contributes to the effect produced; that its repulsion is due to an excited condition into which it is thrown by the influencing magnet, the intensity of this excitement varying within the limits already referred to, as the strength of the magnet which produces it. This conclusion is further arrived at by a close comparison of the repulsions of diamagnetic bodies with the attraction of paramagnetic ones: both are found subservient to one and the same law.

It is next proved that the diamagnetic excitement produced by one pole of a magnet is not the state which enables a pole of an opposite quality to repel the substance:—that each pole induces a condition peculiar to itself, or, in other words, that the excitement of diamagnetic bodies in the magnetic field is of a *dual* character.

These points being established, a searching comparison is instituted between the phenomena exhibited by paramagnetic and diamagnetic bodies in three distinct cases:—first, when operated on by the magnet alone; secondly, when operated on by the current alone; and, thirdly, when operated on by the magnet and current combined. A bar of iron was, in some of these cases, compared with a bar of bismuth, but it was soon found necessary, in order to avoid the proved errors of reasoning, to take strict account of the molecular structure of the bismuth. A bar of this substance, cut in a certain manner from the crystallized mass, exhibits between the poles of a magnet precisely the same visible deportment as a bar of iron, while it is well known that the normal deportment of bismuth is opposed to that of iron. The author, in his examination of the points before us, divided paramagnetic bars into two distinct classes, and classified diamagnetic bars in the same manner; one class he called *normal*, and the other class *abnormal*. A normal paramagnetic bar is one which sets its length from pole to pole in the magnetic field, and a normal diamagnetic bar is one which sets its length at right angles to the line joining the poles. An abnormal magnetic bar, on the contrary, is one which sets its length equatorially; while an abnormal diamagnetic bar is one which sets its length axially.

In all cases, whether operated on by the magnet alone, the current alone, or the magnet and current combined, the deportment of the normal paramagnetic bar is precisely antithetical to that of the normal diamagnetic one. In the magnetic field the former sets axially, the latter equatorially. Operated on by a voltaic current, the former sets its length at right angles to the current, the latter sets its length parallel to it. When magnet and current act together on the bars, it is found that the disposition of forces which produces a deflection from right to left of the paramagnetic bar produces a deflection from left to right of the diamagnetic bar. If the position of equilibrium of the former be from N.E. to S.W., the position of equilibrium of the latter is from N.W. to S.E. In short, the position of rest for the normal magnetic bar is always at right angles to the position of the diamagnetic bar. A precisely similar antithesis is

observed when we compare the abnormal paramagnetic bar with the abnormal diamagnetic one. The former, in the magnetic field, sets equatorially, the latter axially. The former sets parallel to an electric current, the latter perpendicular to the same. If the deflection of the former be from right to left, the deflection of the latter, under like conditions, is from left to right. Finally, the position of equilibrium of the former is always at right angles to that of the latter.

But if the deportment of the normal paramagnetic bar be compared with that of the abnormal diamagnetic one, it will be found that they are in all cases identical; and the same identity of deportment is exhibited when the abnormal paramagnetic bar is compared with the normal diamagnetic one. The necessity of paying attention to structure in experiments of this nature, could not, it is imagined, be more strikingly exhibited.

It is proved by these experiments that the simple substitution of an attractive force for a repulsive one would completely convert the phenomena exhibited by paramagnetic bodies into those exhibited by diamagnetic ones; that if that which Gauss has called the ideal distribution of magnetism in an iron bar be reversed, we have a distribution which would produce the phenomena of a bismuth bar of the same dimensions. All the phenomena of diamagnetic bodies become equally intelligible with those of paramagnetic ones, when we assume that the former class possess a polarity the same in kind, but the opposite in direction, to that of the latter.

It is well known that a bar of iron surrounded by a helix in which a voltaic current circulates is converted into a magnet, and exhibits that *twoness* of action,—those phenomena of attraction and repulsion at its two ends—to which we give the name of polarity. The present paper contains an account of experiments made with the view of ascertaining whether similar phenomena were exhibited by a bar of pure bismuth. A cylinder of the latter substance,  $6\frac{1}{2}$  inches long and 0.4 of an inch in diameter, was suspended by a suitable device within a helix of covered copper wire, so that it could vibrate freely from side to side. The ends of two electro-magnetic cores were brought to bear upon the two ends of the bismuth bar, and it was so arranged that the two magnetic poles acting upon the bar might be of the same or of opposite qualities. The helix being first excited by a strong current, a current of considerably less power was sent round the electro-magnetic cores, and their action upon the bismuth bar was observed: by means of a gyrotrope the current in the helix was reversed, and the effect of the reversion noted; permitting the current through the helix to flow in its last direction, by means of a second gyrotrope, the current which excited the cores was reversed; and finally, while the last magnetic polarity remained unaltered, the direction of the current in the helix was once more changed, and the consequent deportment of the bismuth bar was noted.

In making these experiments and exercising some judgment in the choice of the relation between the strength of the magnets and the strength of the current in the helix, the most complete mastery was attained over the motions of the bar. With one disposition of

the forces the ends of the bar of bismuth were promptly repelled, with another arrangement they were just as promptly *attracted*. If, when moving towards the cores, the direction of the current in the helix was reversed, the motion of the bar was at once arrested, and attraction was converted into repulsion. If, while receding from the bar, the direction of the current in the helix was changed, the recession was stopped and the ends of the bar were attracted. The same results were obtained when, instead of changing the direction of the current in the helix, the polarity of the electro-magnetic cores was reversed. When the latter were so excited that poles of the same quality were presented to the ends of the bismuth bar, the repulsion of the one pole was balanced by the attraction of the other, and under the influence of these opposing forces the bar stood still.

Pursuing the argument further, a south pole and a north pole were caused to act simultaneously upon *each* end of the bismuth bar; supposing one end of the latter to be repelled by a south pole, then, on the assumption of diamagnetic polarity, the same end would be attracted by a north pole; and permitting both poles to act upon it simultaneously from *opposite sides*, we may anticipate that the force tending to turn the bar will be greater than if only a single pole were used. To test this conclusion, *four* electro-magnetic cores were made use of; the two poles to the right of the bismuth bar were of the same name, while the two to the left of the bismuth bar were of an opposite quality: with this arrangement the mechanical action upon the bar was greatly augmented, and the foregoing anticipation completely verified.

The bar employed in these experiments is unusually large, but it does not mark the practical limit of success. All the results obtained with this bar were obtained with another solid cylinder of bismuth 14 inches long and 1 inch in diameter. The corresponding experiments were made with bars of iron, and it was always found that the arrangement of forces which caused the attraction of the ends of the paramagnetic bar caused the repulsion of the ends of the diamagnetic one; while the disposition which caused the repulsion of the ends of the paramagnetic bar produced, in the most manifest manner, the *attraction* of the ends of the diamagnetic one. As regards the abstract question of polarity, the only difference between iron and bismuth is the comparatively intense action of the former. In the case of a magnetic body, whose capacity for magnetization does not exceed that of bismuth, and in which coercive force is equally absent, no proof can be adduced in favour of the polarity of the former body that cannot be matched by proofs of equal value in every respect of the polarity of the latter.

The objections that have been and possibly may be used against diamagnetic polarity, are next considered, and some observations are made on the constitution of the magnetic field. The relation of our present knowledge of diamagnetic action to the theory of Weber, and to Ampère's hypothesis of molecular currents is stated; and in conclusion, the author dwells briefly upon those points of diamagnetic action wherein the views of M. Matteucci differ from his own.

“On Differential Transformation and the Reversion of Serieses.”  
By J. J. Sylvester, Esq., F.R.S.

With a view to its publication in the Proceedings of the Society, I take occasion to communicate the result of my investigations, as far as they have yet extended, into the general theory of differential transformations, containing a complete and general solution of the important problem of expanding a given partial differential coefficient of a function in respect of one system of independent variables in terms of the partial differential coefficients thereof, in respect to a second system of independent variables, each respectively given as explicit functions of the first set.

This question may be shown to be exactly coincident with that of the reversion of simultaneous serieses proposed by Jacobi, which may be thus stated: given  $(n+1)$  quantities, each expressed by rational infinite serieses as functions of  $n$  others; required to express any one of the first set in a rational infinite series in terms of the other  $n$  of the same set. This question has only been resolved by Jacobi for a particular case; the result hereunder given for the transformation of differential coefficients contains the solution of the general question. My method of investigation is entirely different from that adopted by the great Jacobi, and I hope in a short time to be able to lay it in a complete form before the Society, and probably to add a solution of the still more general question comprising the reversion of serieses as a particular case, viz. the question of expressing any one of  $n$  quantities connected by  $m$  equations in terms of any  $(n-m)$  others of the same.

Let there be any number of variables, say  $u, v, w$ , of which  $x, y, z$ ,  $\mathfrak{S}$  are given functions, it is required to expand

$$\left(\frac{d}{dx}\right)^f \left(\frac{d}{dy}\right)^g \left(\frac{d}{dz}\right)^h \mathfrak{S}$$

in terms of the partial differential coefficients of  $\mathfrak{S}, x, y, z$  in respect of  $u, v, w$ .

Form the determinant

$$\begin{array}{ccc} \frac{dx}{du} & \frac{dx}{dv} & \frac{dx}{dw} \\ \frac{dy}{du} & \frac{dy}{dv} & \frac{dy}{dw} \\ \frac{dz}{du} & \frac{dz}{dv} & \frac{dz}{dw} \end{array}$$

which call  $J$ .

The required expansion will contain in each term an integer numerical coefficient, a power of  $\frac{1}{J}$ , one factor of the form

$$\left(\frac{d}{du}\right)^p \left(\frac{d}{dv}\right)^q \left(\frac{d}{dw}\right)^r \mathfrak{S},$$

and other factors of the form

$$\begin{aligned} & \left(\frac{d}{du}\right)^l \left(\frac{d}{dv}\right)^m \left(\frac{d}{dw}\right)^n x \\ & \left(\frac{d}{du}\right)^{l'} \left(\frac{d}{dv}\right)^{m'} \left(\frac{d}{dw}\right)^{n'} y \\ & \left(\frac{d}{du}\right)^{l''} \left(\frac{d}{dv}\right)^{m''} \left(\frac{d}{dw}\right)^{n''} z. \end{aligned}$$

Let the latter class of factors be distinguished into two sets, those where  $l+m+n=1$ ,

$$\begin{pmatrix} l \text{ i. i } l=0 & m=0 & n=0 \\ \text{or } l=0 & m=1 & n=0 \\ \text{or } l=0 & m=0 & n=1 \end{pmatrix}$$

which I shall call uni-differential factors, and those in which  $l+m+n > 1$ , which I shall call pluri-differential factors.

First, then, as to the form of the general term abstracting from the numerical coefficient and the uni-differential factors (except of course so far as they enter into J). This will be as follows:—

$$\begin{aligned} & \left(\frac{d}{du}\right)^{1l_1} \left(\frac{d}{dv}\right)^{1m_1} \left(\frac{d}{dw}\right)^{1n_1} x \times \left(\frac{d}{du}\right)^{2l_1} \left(\frac{d}{dv}\right)^{2m_1} \left(\frac{d}{dw}\right)^{2n_1} x \times \dots \times \left(\frac{d}{du}\right)^{e_1 l_1} \left(\frac{d}{dv}\right)^{e_1 m_1} \left(\frac{d}{dw}\right)^{e_1 n_1} x \\ & \times \left(\frac{d}{du}\right)^{1l_2} \left(\frac{d}{dv}\right)^{1m_2} \left(\frac{d}{dw}\right)^{1n_2} y \times \dots \times \left(\frac{d}{du}\right)^{e_2 l_2} \left(\frac{d}{dv}\right)^{e_2 m_2} \left(\frac{d}{dw}\right)^{e_2 n_2} y \\ & \times \left(\frac{d}{du}\right)^{1l_3} \left(\frac{d}{dv}\right)^{1m_3} \left(\frac{d}{dw}\right)^{1n_3} z \times \dots \times \left(\frac{d}{du}\right)^{e_3 l_3} \left(\frac{d}{dv}\right)^{e_3 m_3} \left(\frac{d}{dw}\right)^{e_3 n_3} z \\ & \times \left(\frac{d}{du}\right)^p \left(\frac{d}{dv}\right)^q \left(\frac{d}{dw}\right)^r \mathfrak{J} \times \frac{1}{J^\omega}, \end{aligned}$$

subject to the limitations about to be expressed.

$$\begin{aligned} \text{Call} \quad & 1l_1 + 2l_1 + \dots + e_1 l_1 = L_1 \\ & 1l_2 + 2l_2 + \dots + e_2 l_2 = L_2 \\ & 1l_3 + 2l_3 + \dots + e_3 l_3 = L_3, \end{aligned}$$

and form the analogous quantities  $M_1, M_2, M_3; N_1, N_2, N_3$ . Then we must have

$$\begin{aligned} & L_1 + L_2 + L_3 + M_1 + M_2 + M_3 + N_1 + N_2 + N_3 + p + q + r \\ & = f + g + h + e_1 + e_2 + e_3; \end{aligned}$$

and as the sum of any group of indices  $l, m, n$  must be not less than 2, we have

$$f + g + h + e_1 + e_2 + e_3 + p + q + r, \text{ not less than } 2e_1 + 2e_2 + 2e_3,$$

so that  $e_1 + e_2 + e_3$  must not exceed  $f + g + h + p + q + r$ ; furthermore,  $p + q + r$  must not exceed  $f + g + h$ ; and finally,

$$\omega = f + g + h + e_1 + e_2 + e_3.$$

1. We may first take  $e_1 + e_2 + e_3 = E$ , giving to E in succession all integer values from  $f + g + h$  to  $2f + 2g + 2h$ , and find all possible so-

lutions of this equation with permutations between the values of  $e_1, e_2, e_3$ .

2. We may then take  $p+q+r=s$ , giving  $s$  in succession all integer values from 1 to  $f+g+h$ , and find all possible solutions of this equation with permutations between  $f, g, h$ .

3. We may then take  $L+M+N=f+g+h+E-s$ , and find all the values of  $L, M, N$ , with permutations allowable between the values of  $L, M, N$ .

4. We may then take

$$\begin{aligned} L_1 + L_2 + L_3 &= L, \\ M_1 + M_2 + M_3 &= M \\ N_1 + N_2 + N_3 &= N, \end{aligned}$$

and solve these several equations in every way possible, with permutations as before.

5. We must take

$$\begin{aligned} {}^1l_1 + {}^2l_1 + \dots + {}^el_1 &= L_1 & {}^1m_1 + {}^2m_1 + \dots + {}^em_1 &= M_1 & {}^1n_1 + {}^2n_1 + \dots + {}^en_1 &= N_1 \\ {}^1l_2 + {}^2l_2 & \dots & {}^el_2 &= L_2 & {}^1m_2 + {}^2m_2 & \dots & {}^em_2 &= M_2 & {}^1n_2 + {}^2n_2 & \dots & {}^en_2 &= N_2 \\ {}^1l_3 + {}^2l_3 & \dots & {}^el_3 &= L_3 & {}^1m_3 + {}^2m_3 & \dots & {}^em_3 &= M_3 & {}^1n_3 + {}^2n_3 & \dots & {}^en_3 &= N_3 \end{aligned}$$

and solve in every possible manner these equations, but without admitting permutations between the values of  ${}^1l_1, {}^2l_1, \dots, {}^el_1$ , or between the values of the members of the other of the third sets taken each *per se*, and subject to the condition that every such sum as  ${}^rl_i + {}^rm_i + {}^rn_i$  must be greater than unity. Every possible system of values of these nine sets will furnish a corresponding pluri-differential part to the general term.

Next, as to the uni-differential part, we may form the quantity

$$\begin{aligned} &\left(\frac{dy}{dv} \cdot \frac{dz}{dw} - \frac{dy}{dw} \frac{dz}{dv}\right)^{\lambda_1} \left(\frac{dy}{dw} \cdot \frac{dz}{du} - \frac{dy}{du} \frac{dz}{dw}\right)^{\mu_1} \left(\frac{dy}{du} \cdot \frac{dz}{dv} - \frac{dy}{dv} \frac{dz}{du}\right)^{\nu_1} \\ &\left(\frac{dz}{dv} \cdot \frac{dx}{dw} - \frac{dz}{dw} \frac{dx}{dv}\right)^{\lambda_2} \left(\frac{dz}{dw} \cdot \frac{dx}{du} - \frac{dz}{du} \frac{dx}{dw}\right)^{\mu_2} \left(\frac{dz}{du} \cdot \frac{dx}{dv} - \frac{dz}{dv} \frac{dx}{du}\right)^{\nu_2} \\ &\left(\frac{dx}{dv} \cdot \frac{dy}{dw} - \frac{dx}{dw} \frac{dy}{dv}\right)^{\lambda_3} \left(\frac{dx}{dw} \cdot \frac{dy}{du} - \frac{dx}{du} \frac{dy}{dw}\right)^{\mu_3} \left(\frac{dx}{du} \cdot \frac{dy}{dv} - \frac{dx}{dv} \frac{dy}{du}\right)^{\nu_3}, \end{aligned}$$

where

$$\begin{aligned} \lambda_1 + \lambda_2 + \lambda_3 &= L + p \\ \mu_1 + \mu_2 + \mu_3 &= M + q \\ \nu_1 + \nu_2 + \nu_3 &= N + r. \end{aligned}$$

These equations are to be solved in every possible manner with permutations between the members of the  $\lambda$  set, the  $\mu$  set, and the  $\nu$  set. Finally, we have to consider the numerical coefficient. To give a perfect representation of this, we must ascertain what identities exist in the factors of the pluri-differential part. Let us suppose that one set of operators upon  $x$  is repeated  $\theta_1$  times, another  $\theta_2$  times, and so on, giving rise to the powers  $\theta_1, \theta_2, \dots, \theta_x$  in the  $x$  line. Similarly, form  $\phi_1, \phi_2, \dots, \phi_\beta$  from the  $y$  line, and  $\psi_1, \psi_2, \dots, \psi_\gamma$

from the  $z$  line. Then the numerical part of the general term will be

$$\frac{\Pi(\lambda_1 + \mu_1 + \nu_1)\Pi(\lambda_2 + \mu_2 + \nu_2)\Pi(\lambda_3 + \mu_3 + \nu_3)}{\Pi\lambda_1\Pi\mu_1\Pi\nu_1\Pi\lambda_2\Pi\mu_2\Pi\nu_2\Pi\lambda_3\Pi\mu_3\Pi\nu_3}$$

$$\times \frac{\Pi(L+p)\Pi(M+q)\Pi(N+r)}{\left\{ \begin{array}{l} \Pi^1 l_1 \Pi^1 m_1 \Pi^1 n_1 \Pi^2 l_1 \Pi^2 m_1 \Pi^2 n_1 \dots \\ \Pi^1 l_2 \Pi^1 m_2 \Pi^1 n_2 \Pi^2 l_2 \Pi^2 m_2 \Pi^2 n_2 \dots \\ \Pi^1 l_3 \Pi^1 m_3 \Pi^1 n_3 \Pi^2 l_3 \Pi^2 m_3 \Pi^2 n_3 \dots \end{array} \right\}}$$

$$\times \frac{D}{\Pi\theta_1\Pi\theta_2\dots\Pi\theta_\alpha\Pi\phi_1\Pi\phi_2\dots\Pi\phi_\beta\Pi\psi_1\Pi\psi_2\dots\Pi\psi_\gamma},$$

where in general  $\Pi m$  means  $1.2.3\dots m$ : as regards  $D$ , it is the following determinant, viz.

$\lambda_1 + \mu_1 + \nu_1$	$\nu$	$\nu$		$L_3$	$M_3$	$N_3$
$\nu$	$\lambda_2 + \mu_2 + \nu_2$	$\nu$		$L_2$	$M_2$	$N_2$
$\nu$	$\nu$	$\lambda_3 + \mu_3 + \nu_3$		$L_1$	$M_1$	$N_1$
$\lambda_1$	$\lambda_2$	$\lambda_3$	$L_1 + L_2$	$L_3 + p$	$\nu$	$\nu$
$\mu_1$	$\mu_2$	$\mu_3$	$\nu$	$M_1 + M_2 + M_3$		
$\nu_1$	$\nu_2$	$\nu_3$	$\nu$	$\nu$	$N_1 + N_2 + N_3 + r$	

The result, for greater brevity, has been set out in the above pages for the case of  $\mathfrak{S}$ , a function of three variables, but the reader can have no difficulty in extending the statement to any number. In the case of a single variable, the formula can easily be identified with that given by Burman's law. It is noticeable that the determinant above written is of the form

$$Apqr + Bpq + Cqr + Dqrp + Epe + Fq + Gr,$$

the part independent of  $p, q, r$  being easily seen to vanish. Moreover,  $A, B, C, D, E, F, G, H$  are all essentially positive, so that  $D$  can only vanish (except for  $p=0, r=0, q=0$ ) by virtue of one condition at least more than the number of the variables.

Feb. 15, 1855.—Thomas Bell, Esq., V.P., in the Chair.

The following communications were read:—

“On the Computation of the Effect of the Attraction of Mountain-masses, as disturbing the apparent astronomical latitude of stations in Geodetic Surveys.” By George B. Airy, Esq., F.R.S., Astronomer Royal.

The author commences with remarking that his surprise had been excited by the result obtained by Archdeacon Pratt\*, namely, that the computed attraction of the elevated country north-east of India considerably exceeds the disturbance which it was sought to explain. But on consideration the author perceived that this result might have been anticipated, on the extensively received supposition that the

\* Proceedings of the Royal Society, December 7, 1854.



interior of the earth is a dense fluid or semi-fluid (which for convenience he calls *lava*), and that the exterior crust floats upon it. For, he remarks, this crust cannot be supposed at any part to be very high upwards (as in mountains), at least to any great horizontal extent, unless there is a corresponding projection downwards into the lava. Upon making a numerical calculation, even with the crust 100 miles thick, it was shown that there would be such a tendency of the table-land to crack and sink in the middle as no cohesion of rocks can resist. He conceives that the state of the ground may be properly illustrated by a raft of timber floating on water: if one piece of timber projects higher into air than the others, we are certain that it also projects lower into water than the others. Assuming this as established, then it is evident that the horizontal attraction of a mountain-mass on a point at a considerable distance is nearly evanescent, because the increase of attraction of the part which is above the general level is sensibly neutralized by the deficiency of attraction below it where the lighter crust displaces the heavier lava. In like manner, the horizontal attraction of a ship or other floating body is nothing. But the horizontal attraction upon a near point on the earth's surface will not vanish, because the mountain which produces the positive attraction is nearer than the lava-displacement which produces the negative attraction: even here, however, the efficient disturbing attraction will be much less than that computed by considering the dimensions of the mountain only.

Note to a paper entitled "Contributions to the Anatomy of the Brachiopoda," read June 15, 1854. By Thomas H. Huxley, Esq., F.R.S.

My attention having been called within the last two or three days, to an error in my paper on the Anatomy of the Brachiopoda, published in No. 5 of the Royal Society's Proceedings, I beg to be allowed to take the earliest opportunity of correcting it. At p. 111 of that paper the following paragraph will be found:—

"In 1843, however, M. Vogt's elaborate Memoir on *Lingula* appeared, in which the true complex structure of the 'heart' in this genus was first explained and the plaited 'auricle' discriminated from the 'ventricle;' and in 1845, Professor Owen, having apparently been thus led to re-examine the circulatory organs of the Brachiopoda," &c. &c.

Now, in point of fact, though M. Vogt *does* describe and accurately figure the structures called 'auricle' and 'ventricle' in *Lingula*\*, yet he has not only entirely omitted to perceive their connexion, or to indicate the 'auricular' nature of the former, but he expressly states that the so-called 'hearts' are "simple, delicate, pyriform sacs" (p. 13).

I presume that my recollection of M. Vogt's figures was more vivid than that of his text; for having been unable, notwithstanding repeated endeavours, to re-obtain the memoir when writing my paper, I felt justified in trusting to what seemed my very distinct recollection

\* Neue Denkschriften der allgemeinen Schweizerischen Gesellschaft für die gesammten Naturwissenschaften. Band VII.

of its sense. I had the less hesitation in doing this, as in M. Vogt's subsequently published '*Zoologische Briefe*\*,' he gives the received interpretation to the parts of the so-called 'hearts' without any indication of a change of opinion.

I make this statement in explanation of what might otherwise seem to be great carelessness on my part, and for the purpose of further pointing out that M. Vogt not having made the supposed discovery, it is quite impossible that Professor Owen's researches should have been suggested by it.

Feb. 22, 1855.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Temperature and Density of the Seas between Southampton and Bombay *via* the Mediterranean and Red Seas." By MM. Adolphe, Hermann, and Robert Schlagintweit.

In this communication the authors give the results of the observations they had made during their voyage, relative to the temperature and specific gravity of the sea-water, both near the surface and at depths ranging from about 18 to 30 metres, the latter being nearly the greatest depth which the motion of the vessel permitted them to reach. They reserve for a future report their observations on the temperature and moisture of the air, as well as the results of two experiments on the quantity of carbonic acid contained in the air on the Mediterranean and Red Seas.

The instruments employed in the observations here described were as follows:—

(1.) Four thermometers which had been carefully compared at the Kew Observatory previous to the authors' departure. At Bombay they repeated the determination of the zero-point and of another standard point, and found that the thermometers had not varied.

(2.) A dipping apparatus constructed by Mr. Adie. This apparatus, which held 5 or 6 litres, was furnished with two valves, so arranged that as it descended the water passed freely through, but as soon as a commencement was made of drawing it up the valves closed and rendered it water-tight. The authors assured themselves that the temperature of the enclosed water did not sensibly change during the process of drawing it up.

(3.) An areometer from Mr. T. G. Greiner at Berlin. This instrument permitted the specific gravity to be read off directly to three places of decimals, and the fourth could be supplied by estimation.

To render the observations of specific gravity comparable with one another, it was necessary to reduce them to a common temperature, which occasioned some difficulty, as the exact expansion of sea-water between the limits 20° and 25° C. was not accurately known. By means of a delicate voluminometer, constructed for the purpose by M. Geissler of Berlin, the authors determined the expansion to be

\* Frankfort, 1851, vol. i. p. 285.

0·000271 for 1° C. For distilled water Halström had found 0·000219. Another set of more direct experiments, made at Bombay, gave for the expansion of sea-water 0·000337. The difference between this value and the preceding the authors refer to a change of volume of the voluminometer itself, and they prefer the latter, which accordingly they use in their reductions.

The authors then give tables of the results of their observations, which are followed by some general remarks.

*Atlantic.*—The temperature of the Atlantic was found to be—

From lat. 46° to 41° N. . . . . 17°·5 to 18°·5 C.

From lat. 39° to 37° . . . . . 20° to 21° C.

Mean specific gravity reduced to 17°·5 C. = 1·0277.

The temperature and specific gravity showed very little variation in the open sea, so long as no currents were met with, but in the vicinity of land, disturbances of various kinds were noticed. In harbours and in small bays the temperature of the water was found to diminish sensibly at a depth of from 15 to 20 metres, but in the open sea the temperature at the very surface was generally found somewhat lower than at a depth of 30 metres, which no doubt was due to evaporation.

*Straits of Gibraltar.*—A current with a mean velocity of from three to six miles an hour usually flows through the Straits from the Atlantic into the Mediterranean. A counter current is supposed to exist underneath, but the great depth of the Straits prevented the authors from reaching any such current with their dipping apparatus.

East of the Straits the water of the Atlantic was met with in several places, in close proximity with water of the Mediterranean, from which it was distinguished by its temperature and colour. The stream from the Atlantic on passing the Straits seems to divide itself into several branches.

In connexion with the variability of the currents in the Straits, it is worthy of remark that the *unreduced* specific gravity of the water of the Mediterranean and of the Atlantic is nearly the same.

*Mediterranean* :—

From the Straits of Gibraltar to Malta—

Temperature of the water . . . . . 21°·7 to 22° C.

Specific gravity reduced to 17°·5 C. . . . . 1·0287.

From Malta to Alexandria—

Temperature . . . . . 23° to 24° C.

Reduced specific gravity . . . . . 1·0298.

*Red Sea.*—The maximum of specific gravity found during the voyage was in the northern end of the Gulf of Suez, 1·0393 (reduced).

From lat. 27° to 23° N., temperature 24° to 28° C. : reduced sp. gr. 1·0315.

From lat. 22° to 14° temperature 30° to 31°·5 C. : reduced sp. gr. 1·0306.

*Straits of Bab-el-Mandeb.*—The water of the Gulf of Aden being

less dense, though colder, than that of the Red Sea, flows into the latter on both sides of the Island of Perim. This colder water could be detected half a degree north of the Straits. After some further remarks about this current, the authors pass on to the

*Arabian Sea*—in which they found

From long. 44° to 50° East from Greenwich, temp. 28°·8, reduced sp. gr. 1·0275.

From the merid. of Cape Guardafui to Bombay, temp. 27° to 28°, red. sp. gr. 1·0278.

## LVI. Intelligence and Miscellaneous Articles.

ON SENSITIVE COLLODION. BY THOMAS WOODS, M.D.

**I**N the Number of the Photographic Journal for December last, Mr. Doyle asked me a question about the quantity of the gun-cotton which is precipitated by the solution of salt I recommend to be added to the collodion to prepare it for my process. I sent an answer to the succeeding Number of the Journal, but I was not aware at that time how much I ought to have explained. Questions since put to me on the same subject caused me to read over carefully the paper originally sent to the Philosophical Magazine of July last, and I find I have in it made an omission which no doubt has caused the process to be unsuccessful. When I recommended a saturated solution of salt to be used, I meant the solution to be made with alcohol, not with water. If made with the latter, it not only would precipitate too largely, but otherwise destroy the mixture; and I am afraid such has been the case with the operators who have tried the process. As my experience of the process with the iodide of iron has quite answered my own expectations, and, when the proper solutions have been used, in the hands of others the promises I have made for it, I am tempted to rewrite the formula I use, exactly as it should be, at the risk of occupying too much space by a repetition. It may not, however, be without its value just now, as the season is becoming more favourable for photographic operations, and I know of no process more sensitive or certain than that I describe.

Take of—

Sulphate of iron . . . . .	40 grs.
Iodide of potassium . . . . .	24 grs.
Chloride of sodium (common salt)	6 grs.
Alcohol . . . . .	2 ozs.
Strong water of ammonia . . . . .	3 drops.

Mix the powdered salts together and add them to the alcohol, then the water of ammonia. A few pieces of iron wire must be kept in the mixture to prevent the iron becoming peroxidized.

One part of this mixture is to be added to three parts of collodion holding in solution an *alcoholic* solution of common salt in the proportion of 1 fluid drachm of salt to 4 ozs. of collodion. Or, neglecting the salt solution, 5 drops of *chloroform* may be added to 1 drachm of the solution of iodide of iron and 3 drachms of *plain* collodion. The mixture of collodion and iodide of iron ought to be used shortly

after having been made, as the iron becomes peroxidized and spoilt by a prolonged contact with the collodion.

The nitrate of silver solution for exciting the plate should be of the strength of 30 grs. to the ounce of water.

The developing solution may be either the ordinary sulphate of iron, or the pyrogallic acid.

With a good lens and a good light an almost instantaneous picture may be produced by this process.

A little water of ammonia added to the hyposulphite bath brings out the picture more fully when a very short exposure is given.

Sometimes the common table salt contains a little carbonate of soda; if such be used in the process, it causes cloudiness.—*From the Journal of the Photographic Society*, April 21, 1855.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1855.

*Chiswick*.—March 1. Heavy rain: very fine: halo round the moon in the evening. 2. Cloudy: rain. 3. Low white clouds: heavy rain: clear and fine. 4. Clear: cloudy and fine. 5. Slight fog: very fine. 6. Cloudy and fine: foggy. 7. Frosty: fine. 8. Very clear: fine: sharp frost. 9. Frosty: few snow-flakes: overcast. 10. Snowing: sharp frost at night. 11. Cloudy: rain at night. 12. Cloudy: rain. 13. Fine. 14. Fine. 15. Cloudy: rain. 16. Densely overcast: fine. 17. Fine: rain: clear. 18. Cloudy and boisterous. 19. Showery. 20. Foggy: very fine. 21. Cloudy and cold: hail-shower. 22. Sleet: cold rain: overcast: boisterous. 23, 24. Cloudy and cold. 25. Fine: sharp frost at night. 26. Clear: cloudy: fine. 27. Very fine. 28. Drizzly: heavy rain. 29. Cloudy: clear and fine. 30. Cloudy. 31. Heavy clouds: clear and fine.

Mean temperature of the month .....	37°·61
Mean temperature of March 1854 .....	42·54
Mean temperature of March for the last twenty-nine years...	42·24
Average amount of rain in March .....	1·33 inch.

*Boston*.—March 1. Fine: rain P.M. 2. Fine. 3. Fine: stormy and rain A.M. 4, 5. Fine. 6, 7. Foggy. 8. Fine. 9. Cloudy. 10. Fine. 11. Fine: snow P.M. 12. Cloudy: rain P.M. 13. Rain: rain A.M. 14. Cloudy. 15. Fine. 16. Fine: rain A.M. 17. Cloudy: rain A.M. and P.M. 18, 19. Fine. 20, 21. Cloudy. 22. Cloudy: snow A.M. 23—25. Cloudy. 26. Fine. 27, 28. Cloudy. 29—31. Fine.

*Sandwick Manse, Orkney*.—March 1. Showers A.M. and P.M. 2. Rain A.M. and P.M. 3. Rain A.M.: drizzle P.M. 4. Clear A.M.: cloudy, large solar halo P.M. 5. Clear A.M.: cloudy P.M. 6. Bright A.M.: clear, fine P.M. 7, 8. Bright, hoar-frost A.M.: clear, frost P.M. 9. Bright, hoar-frost A.M.: clear, drops P.M. 10. Snow-showers A.M.: cloudy P.M. 11. Cloudy A.M.: snow-showers P.M. 12. Snow-showers A.M.: snow-drift P.M. 13. Snow A.M.: cloudy P.M. 14. Bright A.M.: clear, aurora P.M. 15. Bright A.M.: cloudy P.M. 16. Rain A.M.: cloudy P.M. 17. Clear A.M.: damp P.M. 18. Rain A.M.: showers, aurora P.M. 19. Bright A.M.: clear, aurora P.M. 20. Bright A.M.: clear P.M. 21. Clear A.M.: clear, aurora P.M. 22—24. Snow-showers A.M.: clear P.M. 25. Snow-showers A.M. and P.M. 26. Snow A.M.: clear, frost P.M. 27. Snow, clear A.M.: cloudy P.M. 28. Cloudy A.M.: clear P.M. 29, 30. Clear A.M. and P.M. 31. Bright A.M.: clear P.M.

Mean temperature of March for twenty-eight previous years...	40°·53
Mean temperature of this month .....	36·61
Mean temperature of March 1854 .....	45·14
Average quantity of rain in March for fourteen previous years	2·52 inches.

The mean temperature of this month is lower than that of March during any of the twenty-eight years of my observations, except in 1837, when it was 36°·54, and in 1839, when it was 36°·33. That of March last year was the highest during the whole period.



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LVII. *On the Action of Non-conducting Bodies in Electric Induction.* By P. RIESS\*.

**E**LECTRIC induction, that wonderful property which an electrified body possesses of eliciting two electricities in any body in its vicinity, has been the subject of many investigations which would have been quite superfluous had the clear insight into that phenomenon, already obtained, been strictly adhered to. According to the conclusion deduced from Franklin's theory, and accepted without question by *Æpinus*, *Coulomb* and *Poisson*, the body provided with two electricities does not act differently from what it would do if it were possible to withdraw the induction, and to electrify the induced body directly, and in exactly the same manner, by means of a rubbed glass rod and piece of sealing-wax. According to this view, every experiment in which an induced and an inducing body are allowed to act upon a third body, becomes a particular case of so complicated a nature as to render its investigation apparently quite useless. If we reflect that the determination of the electric distribution upon a body subject to the simple induction of another electrified body is a problem not yet solved, nor perhaps capable of general solution, it will be evident that to determine, according to general laws, the influence of three different quantities of electricity, whose distribution is unknown, upon any body, would be a fruitless undertaking. But, inasmuch as the true notions of induction have been departed from, it has become necessary to investigate particular cases of the above kind for the sole purpose of bringing the results of experiment into harmony with the theory above mentioned. Thus, a long time ago, I considered the sim-

\* From *Poggendorff's Annalen*, vol. xcii. p. 337.

plest case, where an induced body was momentarily touched by a neutral one, and together with others I partially succeeded in controverting the fundamental error according to which the electricity remaining bound or latent in the touched body was said to be either without any action at all, or only to act in a certain direction and within a certain distance. I say only partially succeeded; for although that fundamental error is no longer nakedly and unmistakably pronounced, yet a multitude of examples show that it is still secretly entertained, and still manifests its prejudicial influence. The whole of the following investigation might be reduced to a few reiterations, were it not advisable to advance experiments against experiments which, in consequence of the foregoing error, have obtained a false signification.

I may here be allowed to remark, that in the discussion on the non-activity of induced electricity, I have been censured for having expressed myself against the term bound or latent electricity as usually employed, and that my proposition to abandon the use of this term has hitherto met with little approval: I think, unjustly. It is not asserted that a wrong meaning is attached to a word, but that in other parts of physics a different interpretation is attached to this word. A quantity of heat is said to be latent when it has no effect upon the thermometer, but a quantity of bound or latent electricity has its full effect upon an electroscope. No doubt it is possible to attach a proper interpretation to an inappropriate term, but the necessity for doing so ought to be avoided. When, of a certain quantity of electricity, a part is said to be latent, we mean that, besides the action of the quantity under consideration, that of a smaller quantity of unlike-named electricity must be taken into account. But to require such an expression to be thus interpreted appears to me unadvisable. I proceed with the subject of the present memoir.

In the 11th, 12th, and 13th series of his imperishable Experimental Researches, Faraday has occupied himself with induction, and endeavoured to establish the notion that induction is not produced by the action of electricity across space, but that an electric body acts only on the contiguous particles of an insulating medium, and that this action is transmitted from particle to particle of that medium. Thus induction should depend essentially upon the nature of the medium which separates the inducing from the induced conductor, and on this account the medium is called a *dielectric*. A long time ago\* I showed that this theory was untenable, because it was based upon the assumption, proved by experiment to be incorrect, that of the three

\* Vide *Repertorium der Physik*, 1842, p. 129.



quantities of electricity, present whenever an electrified body acts upon a neutral one, two quantities being completely occupied with one another have no further action; I have intimated that the experiments which ought to support this theory may be explained by the simultaneous action of all three electricities. Such an intimation appeared to me sufficient, inasmuch as these experiments belong to those complicated cases which were mentioned at the commencement of this paper. There still exists, however, an uncertainty as to the signification of a part of those experiments, which are the more particularly important because they appear to establish an essential difference between the inductive actions of conducting and non-conducting bodies, and on that account it will not be deemed superfluous to remove that uncertainty in an experimental manner.

In order to determine what he calls the specific inductive capacities of insulators, Faraday employed an instrument called the differential inductometer\*, which consists of three, or simpler of two, insulated metallic discs placed at a short distance from one another with their faces parallel; one disc was charged with electricity directly, and the electric state of the other disc, which was charged by the inductive action of the first, examined. When between both plates, but without touching them, a non-conducting plate of shell-lac or sulphur was introduced, the amount of induced electricity was found to be greater than before, *i. e.* when both discs were separated from one another by air only. It follows from other experiments made by Faraday†, that the induction would have been diminished had a conducting plate been introduced between the two; for, according to Faraday's opinion, the introduction of the conducting plate would have caused the induction to take place in curved lines around the edges of the plate, instead of in right lines through the intervening stratum of air.

The experiments with insulating intermediate plates have been repeated with various results. In accordance with Faraday, Knoekenhauer‡ found, by introducing a plate of shell-lac between two discs, an augmentation of induced electricity upon the induced plate, but could not conceive the cause of this result. Müller§, who considered this result to be incompatible with the cause of the phenomenon, found on frequent repetition of the experiment a diminution of the induced electricity, and ascribed the results of both his predecessors to errors in the testing of the kind of electricity here excited.

\* Experimental Researches, 1307.

† Ibid. 1218.

‡ Pogg. Ann. vol. li. p. 126.

§ Bericht über die neusten Fortschritte der Phys., Braunschweig, 1849, p. 61.

In order to give certain and unequivocal results, the experiment in question requires some care when arranging the apparatus, which I will consequently describe at length. Two metallic discs, insulated on glass pillars, were placed vertically on a table with their faces turned towards each other, and both parallel to the edge of the table at which the experimenter sat. The second or most distant plate was connected, by means of a wire 18 inches long, with a gold-leaf electroscope; and in finer experiments, with a dry-pile electroscope, and the first or nearest disc was charged with positive electricity. The electroscope indicated positive electricity, which was removed by uninsulation. If, during the course of the experiment, the quantity of induced electricity on the disc in connexion with the electroscope was increased, in other words, if the induction of the charged disc was apparently *strengthened*, the electroscope again indicated *positive* electricity; on the contrary, if that induction was apparently *weakened*, it indicated *negative* electricity. In order to avoid all uncertainty about the result thus obtained, the electroscope was uninsulated after the change in the induced electricity, and the indication of the electroscope observed after the cause of this change had been removed: here an indication of positive electricity proved that in the original experiment the induced electricity had been diminished; of negative electricity, that the induction had been increased. After every experiment such a check was applied, so that no doubt can be entertained as to the correctness of the observations. At first two metallic discs, 7·21 inches in diameter and 0·06 inch thick, were used, whose distance asunder was 1·11 inch. A disc of shell-lac, 7·21 inches in diameter and 0·33 inch thick, which had received a smooth reflecting surface by pressure between two glass plates, was fixed by means of a clamp to a glass pillar, 5 inches long; the latter was covered with shell-lac and turned on a point, so as to admit of its being placed in any required position between the two metallic discs. When the disc of shell-lac, after being divested of all electricity by means of a spirit-lamp flame, was brought in the middle between the two discs and made concentric with them, the gold-leaf or dry-pile electroscope showed that the induced electricity upon the disc in connexion with the electroscope was considerably *increased*. On the contrary, when the disc of shell-lac only occupied a portion of the intermediate space (a spindle-shaped portion about half an inch in breadth), and its greatest part remained without that space, the induced electricity was, although only slightly, yet quite decidedly *diminished*. I mention this, because whatever may be the constitution and shape of the interposed disc, the electroscope always indicates a diminution in the amount of induced electricity at the moment when it

comes between the metallic discs, a diminution which will be disregarded in the following experiments; this experiment alone, however, is necessarily at variance with the assumption, that the induction of a charged metallic disc is stronger through shell-lac than through air, inasmuch as no reason can be assigned why a small piece of shell-lac should weaken an action which a greater piece strengthens. The experiments described at the end of the present memoir will furnish the explanation of this remarkable phenomenon.

In order to operate with smaller intermediate plates, the large metallic discs were replaced by smaller ones, 4.43 inches in diameter, at a distance of 1.65 inch from one another. Here, however, an important influence on the result of the experiment was produced by changing the position of the clamp. On this account the clamp was abandoned, and above the metallic discs a metallic cylinder 3 inches long was fixed, in whose axis a metallic rod could turn softly; this rod carried at one end a round nut for adjustment, and at the other a clamp  $1\frac{1}{2}$  inch long. With respect to this clamp, a convenient arrangement may be here noticed; one of its legs is immovable, and the other can be moved on a prism by means of a screw, so as to keep the two legs always parallel to one another. The handle of a plate being fixed in this clamp, a rotation of  $90^\circ$  given to the axis of the cylinder was sufficient either to bring the plate between the metallic discs, or to remove it sufficiently from them, without altering the position of the apparatus towards the discs, exception being made of the screw on the clamp, whose influence, when present, was taken into account. This arrangement of the apparatus enabled the observer to preserve his arm and hand unmoved during an experiment, inasmuch as a slight pressure of the finger was sufficient to bring the plate into the two required positions. The immovability of the observer is necessary in some of the following experiments, in which importance is attached to small motions of the gold leaves on the electroscope. In the experiments immediately following, the indications of the electroscope were so decided as to render this precaution unnecessary. The following table gives the changes which, according to the two-fold indications of the electroscope, were produced in the induced electricity of the neutral metallic disc by interposing the several plates there mentioned. The interposed plates were fixed to glass or shell-lac pillars, from 4 to 6 inches long, and before the experiments were completely deprived of their electricity.

Diameter of metallic discs 4.46 inches.

Distance asunder . . . 1.65 ...

	Intermediate plate.	Form.	Linear dimensions in inches.	Thickness in inches.	Induced electricity of the neutral disc.
A.	Shell-lac.	Disc.	Diam. { 4.16 4.20 }	0.11	Increased.
a.	"	"	" 1.29	0.26	Increased.
B.	Paraffine*.	"	" 4.63	0.50	Increased.
b.	"	"	" 2.53	0.30	Increased.
β.	"	"	" 2.36	0.11	Increased.
C.	Gutta-percha.	"	" 5.06	0.14	Increased.
c.	"	"	" 2.16	0.14	Increased.
	Mica.	Rectangle.	6.2 by 4.1	0.004	Decreased.
D.	Glass.	Disc.	Diam. 2.20	0.16	Increased.
d.	"	Square.	Side 4.1	0.17	Increased.
δ.	"	Rectangle.	9.60 by 10.25	0.06	Decreased.

The majority of these experiments, therefore, confirm Faraday's statement, that by placing an insulating plate between a charged and a neutral metallic disc, the induced electricity upon the latter is increased. That this increase, however, is not an essential property of the insulator is shown by the two experiments with the mica plate, and the glass plate  $\delta$ , both of which caused a diminution in the induced electricity, although they are by no means inferior to gutta-percha in their insulating capacities. Further, that this increase is not exclusively produced by insulating substances is proved by the following experiments, in which, by employing perfectly conducting intermediate plates fixed to insulating pillars, the same phenomenon is apparent.

Indeterminate plate.	Diameter in inches.	Thickness in inches.	Induced electricity of the neutral disc.
Tinfoil.	1.56	0.002	Increased.
Brass.	0.67	0.25	Increased.
Silver.	1.56	0.12	Increased.
Copper.	4.03	0.03	Decreased.

It is manifest that with metals the difference between the actions of two plates is solely due to their dimensions, so that none of these results can appear strange. When both discs stand alone, the neutral one is acted upon by a stratum of positive electricity 1.65 inch distant. By the interposition of a conducting plate, two electric strata at a smaller distance are super-added, of which the positive stratum lies nearer to the excited disc than the negative. Both strata contain the same quantity of electricity; but the distribution of these quantities in the strata depends not only upon the dimensions and distance between the

\* Commercial paraffine from the Augustinhütte, Bonn.

two discs, but also upon the form and dimensions of the intermediate plate itself. By changing the dimensions of the intermediate plate, its action upon the induced disc will be changed in a twofold manner; the two electricities upon it obtain another distribution and quantity, and the distances at which these strata act upon the induced disc are changed. We can determine two strata, such that their total action upon the induced disc shall be inappreciable, or like in kind to the action of the directly charged disc, or lastly, unlike to this action. The question, therefore, in what manner will a conducting intermediate plate change the induced electricity which a charged disc produces on a neutral one, is completely indeterminate. The question will be a determinate one if the conducting plate is not insulated, for then to the action of a charged disc that of an unlike stratum of electricity is added, which necessarily opposes that action. But to attempt to compare the action of conducting and non-conducting intermediate plates under these circumstances, would be to commit an experimental mistake not less grave, but less excusable, than the mistake of those earliest electricians, who, without insulating the conductors, compared conductors with non-conductors with respect to their capacities of becoming electric by friction.

The simplest form was now given to the induction experiment, and the action of intermediate non-conducting plates examined. Two metallic spheres, 0.87 inch in diameter, were fixed to thin glass rods, and so placed that their central line lay horizontal, and the distance between their nearest points amounted to a little more than 0.6 inch. One sphere was charged with electricity, the other was connected, by means of a wire 18 inches long, with a dry-pile electroscope, and observations were conducted in the manner already described. Every intermediate plate was carefully placed in the space between the two spheres, so that the line joining the centres of the spheres passed perpendicularly through the centre of the plate.

	Intermediate plate.	Induced electricity on the neutral sphere.
A.	Shell-lac.	Decreased.
a.	"	Increased.
b.	Paraffine.	Increased.
β.	"	Increased*.
C.	Gutta-percha.	Decreased.
c.	"	Increased.
D.	Glass.	Increased.
d.	"	Decreased.

\* A disc of paraffine, of similar dimensions to β, gave a decided decrease of the induced electricity; I have not recorded the observations with this disc, because it was considerably thinner in the middle than at the edges.

From this it is manifest that intermediate plates of the same non-conducting substance sometimes decreased, and sometimes increased the induced electricity of the neutral sphere; and further, by comparison with the former experiments, that one and the same non-conducting plate, which increased the induced electricity upon a neutral disc, decreased the same when the neutral body was a sphere. Hence the important theorem:—

*In experiments on induction, the effect of the action of non-conducting intermediate plates depends upon the form and dimensions, not only of those plates, but also of the conductor used in the experiment.* This theorem negatives, in the simplest manner, the assumption that the intermediate plates directly influence the induction of the originally charged conductor. We cannot directly ascertain in what manner the action of insulating plates exerts itself, because the electric condition of these plates cannot be examined, and reference must be had to the analogous action of conducting intermediate plates, in which this examination is possible. The following discs, on insulating pillars, were placed between the spheres.

Intermediate disc.	Diameter in inches.	Thickness in inches.	Induced electricity of the neutral sphere.
Brass.	0·67	0·25	Increased.
"	"	0·04	Decreased.
Silver.	1·56	0·12	Increased.
Tinfoil.	"	0·002	Decreased.
Copper.	4·03	0·03	Decreased.

Here, undoubtedly, both the electricities induced upon the intermediate disc acted. In order to find the general distribution of these electricities, the discs were placed separately at a distance of a quarter of an inch from the surface of the positively charged sphere, in such a manner that the line joining the centres of the sphere and disc was perpendicular to the latter. Several points of the disc were then touched with a pin's head, insulated by shell-lac, and the electric state of the latter examined in an electroscope.

Negative electricity was present upon the whole anterior surface (turned towards the charged sphere) of the thick brass disc, and it diminished from the centre to the circumference; on the cylindrical and posterior surfaces positive electricity was found, which increased from the anterior to the posterior edge, and decreased from the latter towards the centre of the posterior surface. A similar distribution was detected upon the thin brass disc; on the greater discs of silver and tinfoil the negative electricity upon the anterior surface no longer extended to the cir-

circumference; at the edge of this surface a narrow non-electric ring could be detected. Lastly, on the greatest disc, of copper, a broader ring of positive electricity was found on the anterior surface itself. The negative electricity upon this surface decreased from the centre to a distance of about  $1\frac{1}{4}$  inch; at  $1\frac{1}{2}$  inch there was no electricity; and at  $1\frac{3}{4}$  inch there were decided appearances of positive electricity, which increased in density towards the circumference, and from there to the centre of the posterior surface again diminished. This superficial knowledge of the electric distribution upon the disc (which would suffer some change in presence of the neutral sphere), is sufficient to explain the action of the disc as intermediate plate. Whenever a disc is placed normally between two spheres, one of which is directly charged, two equal quantities of unlike kinds of electricity are elicited upon it, of which the quantity, unlike the electricity of the charged sphere, extends at all events over a less surface than the quantity like the electricity of the sphere does. When the disc is very thin, the two electric strata lie on two planes very close to one another, both of which are perpendicular to the line joining the centre of the spheres; hence the unlike-named stratum will act more strongly upon the neutral sphere than the like-named, because it will act at a less distance. For example, let us consider the copper disc placed between the two spheres. Then, acting upon the nearest point of the neutral sphere, we have the whole negative electricity upon a circular surface at a distance of one-third of an inch, and whose diameter is  $2\frac{1}{2}$  inches, together with a smaller quantity of positive electricity upon a circular surface, whose distance from the point in question is only 0.302 inch, but whose diameter is 3.958 inches. At the same time the negative electricity is most dense at the centre where it is nearest to the neutral, whilst the positive electricity is most dense at the circumference where it is furthest from the neutral sphere. The negative electricity therefore exercises a greater influence upon the neutral sphere than the positive; and the induction of the charged sphere must appear diminished, as experiment plainly shows. When the thickness of the disc is great in comparison to its diameter, an opposite effect must ensue. By placing the thick brass disc between the spheres, the whole quantity of negative electricity is confined to a circular surface  $\frac{2}{3}$ ds of an inch in diameter, at a distance of 0.43 of an inch from the nearest point of the neutral sphere; an equal quantity of positive electricity is spread over the cylindrical surface, and a circular surface whose diameter is also  $\frac{2}{3}$ ds of an inch, but whose distance from the nearest point of the sphere is only 0.18 inch. This great difference between the distances manifestly causes the action of the positive electricity upon the

posterior surface to surpass that of the negative electricity upon the anterior surface; sometimes, too, because the former is supported by the action of the whole positive electricity on the cylindrical surface. Here, therefore, the influence of the intermediate plate will be alike in kind to that of the charged sphere, and the induction of the latter will be apparently increased by introducing the plate between the two spheres. It is more difficult to predict the effect upon the observed induction produced by interposing a disc of less extreme dimensions in comparison to the distance between the two spheres, but after observation this effect may be easily explained by the distribution of the electricity upon the plate.

In general broad thin intermediate discs apparently weaken, and thick narrow discs apparently strengthen the inductive effect of a charged sphere upon a neutral one. Inasmuch as no doubt can exist that the action of conducting plates depends solely upon the induced electricities upon these plates, we must conclude that the action of non-conducting intermediate plates ought to be ascribed to the same cause. It is certain that a charged body induces both electricities upon the surfaces of the most perfectly insulating body; and the following simple and remarkable experiment will show that upon every one of the interposed plates hitherto used, these electricities make their appearance during the time of an observation. The disc of shell-lac 7.21 inches in diameter and 0.33 inch thick, or the disc of paraffine 4.63 inches in diameter and  $\frac{1}{2}$  an inch thick, was moved quickly to and fro between the flame of a spirit-lamp and the knob of the conductor of an electric machine slightly charged positively; the anterior surface of the disc approached within one foot of the knob, and the posterior surface within one inch of the flame. By this means the anterior surface became so strongly negatively electric as to impart a strong and durable divergence to the gold leaves of the electroscope. Here, during the moment of passage, the positively charged knob of the conductor imparted negative electricity to the anterior surface of the plate, and positive to the posterior, and the negative electricity alone remained behind because the flame had withdrawn the positive. Consequently, in all the induction experiments above described, each insulating plate was undoubtedly provided with both electricities at the moment when it was interposed between the two discs or spheres; and to explain the different actions of these plates, we have only to assume that these electricities occupied different positions in space upon the thick and upon the thin plates. If, however, the action of the insulating plates depends upon the induced electricities on their surface, it follows that their interposition between the charged and neutral conductors is unessen-



tial, and the plates must also act when they occupy a side position in the neighbourhood of those conductors. By examining the induction upon a conducting disc, we may learn what effect this action must have. A copper disc, 4.03 inches in diameter, was so placed before a positively charged sphere, 0.87 inch in diameter, that the perpendicular let fall from the centre of the sphere upon the plane of the disc intersected the horizontal diameter of the latter at a point 0.77 of an inch from its centre, the distance between the nearest point of the sphere and the disc being 0.26 of an inch. The following are the results of an examination of the disc's horizontal diameter. The negative electricity induced upon the disc's anterior surface extended to a distance of 2.57 inches from that part of the circumference nearest to the sphere; the last 0.86 of an inch extending up to the most distant part of the circumference was occupied by positive electricity, as was the whole diameter of the posterior surface. If such a disc were placed near a positively charged ball during its inductive action upon a neutral one, with its plane parallel to the line joining the centres of the spheres, and its anterior circumference equidistant from both spheres, the negative electricity upon the disc would be nearer to the induced sphere than the positive. This negative electricity, therefore, would exercise a superior influence upon the sphere, and the induction of the originally charged sphere would appear weakened. The same effect would be produced if the conducting disc were placed between the spheres perpendicular to the line joining their centres, but without allowing its plane to be intersected by that line. This last experiment was unintentionally made in all the foregoing ones, in which interposed plates were employed. It was before mentioned, that the induced electricity appeared to be diminished at the moment when a plate was interposed between the two discs or spheres. With some of the non-conducting plates this diminution of the induced electricity was even greater than the subsequent increase of the same, when the plate was completely interposed. Imagine the examined copper disc placed near the two spheres with its surface parallel to the line joining their centres, and with its centre perpendicularly opposite to the centre of that line; the positive electricity on both surfaces of the disc will be much nearer to the induced sphere than the negative electricity, so that the positive electricity will exercise a superior influence on the neutral sphere, and the induction of the directly charged sphere will be apparently increased. I have made this experiment with the non-conducting plates before mentioned. The discs were placed in the described position at the side of two spheres, 0.60 inch distant from one another, and at a distance of 0.26 inch from their surfaces; in this position

I always found that the induced electricity of the neutral sphere was increased. Consequently we can apparently change the induction of a charged body upon a neutral one in any manner we please, without introducing into the space between both, which is occupied by atmospheric air, either a conducting or a non-conducting body.

The preceding research may be considered as demonstrating the following fact. The inductive influence exercised by a charged body upon a neutral one is not changed by bringing any interposed body, conducting or non-conducting, into its neighbourhood. An increase or decrease of the quantity of electricity upon an induced body may be produced by a conducting or a non-conducting interposed body; this increase or decrease arises from two new inductions proceeding from the interposed body itself, whose total action will depend upon the distribution of the electricities upon this body, and consequently also upon its substance, form and dimensions.

Accordingly, there is no essential difference between the actions of conducting and non-conducting bodies; but inasmuch as the distribution of electricity upon them depends also in extreme cases upon their substance, interposed bodies of the same form and magnitude will act differently. This difference of action was particularly perceptible when the interposed body was placed in the space between the charged and neutral bodies, and may be explained by the well-known peculiarities of conductors and insulators. In conducting bodies the distribution of the induced electricities depends solely upon the attracting and repelling forces of these electricities, and of the inducing electricity; in non-conducting bodies the distribution depends not only upon these forces, but also upon the resistance which the substance opposes to the motion of electricity. For example, in a conducting disc, when influenced by a positively charged sphere placed perpendicularly opposite to its centre, the most positively electric places lie in all cases at the circumference; in a non-conducting disc they may be at some distance therefrom. If, as interposed body between a charged and a neutral sphere, we employ a disc whose diameter may be conceived gradually to increase, then the induced positive electricity upon it will sooner cease to exercise a superior influence upon the neutral sphere when the disc is composed of a conducting, than when of a non-conducting substance. Accordingly, with a conducting disc, the action of the electricity, unlike that of the charged sphere, more easily manifests itself, therefore the inductive action of this sphere will be apparently diminished; with non-conducting discs, the electricity, like that of the sphere, acts more easily, consequently the inductive action of the sphere will be increased.

If the conduction on the surfaces of insulators did not make the comparison a very precarious one, it would be interesting, with a view to their classification, to compare the smallest dimensions at which discs of different non-conducting substances perceptibly diminish the influence of a certain body. Glass, mica, and shell-lac conduct on the surface better than in the interior, gutta-percha worse; and even when the greatest care is used in the treatment of the plates, this conduction cannot be retained sufficiently constant as to prevent the same plate at different times from exercising a considerably different action. The examination of a plate, by using it as an interposed plate, is much more delicate than its direct examination by means of an electro-scope; and differences in superficial conduction can be detected thereby, which, by the latter method, remain completely hidden.

*LVIII. Researches into the Alum Manufacture. By JOHN WILSON, Esq., Jun., Student in the Laboratory of St. Thomas's Hospital College, London\*.*

**T**HE history of alum-making is of considerable antiquity. Dioscorides and Pliny describe a substance, alumen, which however does not seem to have had much, if any, resemblance to the alum of the present day. Pliny chiefly speaks of two kinds of alumen, the liquid and the solid (*liquidum spissumque*); the liquid should be limpid and milk-white, and its characteristic is its striking a black colour with the juice of the pomegranate; and he informs us that it is very astringent, and is efficacious when mingled with honey as a cure for ulcers of the mouth. From this character of giving a black with pomegranate juice, it appears to have contained iron as one of its ingredients, which is not unlikely if the salt was native.

There was also a species of alumen called by the Greeks *schistos*, which split up into capillary filaments and liquefied when heated; this substance was prepared for use by heating it till it ceased to turn fluid, or in other words, by expelling its water of crystallization. This description agrees with the characters of some alum shales which become decomposed by exposure and split up, while hair salts shoot from the cavities. Dioscorides states that there are many kinds of alumen, but that most of its varieties may be found in Egypt. The best for medical purposes is very white, astringent, granular, and free from hard concretions, and splits up into capillary crystals.

The date of the discovery of our alum is unknown; previous to the middle of the fifteenth century, all the alum used in

\* Communicated by Dr. Robert Dundas Thomson, F.R.S.

Europe was imported from the East. In the fourteenth century an alum work was in operation at Rocca in Syria. There were also several near Constantinople. In 1459, Bartholomæus Pernix or Perdix, a merchant of Genoa who had frequently been to Rocca, discovered the mineral of alum in the island of Ischia, which is situated at the entrance of the Gulf of Naples. About this time Johannes de Castro, having visited the alum-works at Constantinople, discovered the alum stone at Tolfa. It is said he was led to this observation by the similarity of the trees at Tolfa to those which he had seen growing upon the hills of Turkey; and experiment showed the two minerals to be equally adapted for preparing alum.

At Viterbium the attempts made by the Genoese had great success, and the manufacture increased greatly, especially as Pope Pius the Second strictly forbade the use of oriental alum. But the art did not remain within the bounds of Italy, in the sixteenth century a work having been set up at Alcmaron near Carthagena. In Germany, in the year 1544, several existed; and in the time of Agricola there was an alum-work at Comoton in Bohemia. Bergman says that at this time (16th century) the art had scarcely penetrated into Sweden. The first alum-work in that country was founded in 1637 at Andrarum, where the alum shale is of great depth, being upwards of 360 feet.

In Queen Elizabeth's reign alum-making was introduced into England by Sir Thomas Chaloner of Gisborough in Yorkshire. It is said that he observed the trees in his neighbourhood tinged with a peculiar colour, and suspecting that this was owing to some aluminous mineral, caused a search to be made, the result of which was the discovery of alum shale. It was towards the end of the sixteenth century that his works were commenced; and in a short time so great was their success, that sixteen works of the same kind were soon established in the neighbourhood.

Alum was first made in Scotland in the year 1766, by Messrs. Lightbody and Co. at Hurlet, but their process was found to be unprofitable, and abandoned in 1768-69; and it was not till the year 1797, when works were erected at Hurlet by the late Charles Macintosh of Campsie, and John Wilson of Hurlet, and their partners, that the manufacture of alum was successfully established. In consequence of the success of the Hurlet works, the same Company in the year 1808 commenced a larger work at Campsie, where a shale similar to that at Hurlet is found in abundance.

In the year 1820, the alum made in England and Scotland amounted to from six to seven thousand tons annually; namely four thousand five hundred to five thousand five hundred in England, and fifteen hundred tons in Scotland. At present

about seventeen thousand tons are made annually, of which twelve thousand tons are produced in England, and the rest in Scotland. The processes by which alum is produced from the shale or other material are different, as the composition of these varies.

In the alum-works which were carried on formerly at Smyrna and Solfatara, and in other places where the ore contains a sufficient amount of alkali to form alum without an additional quantity, the process is simple. The rock containing the ready-formed alum is dissolved in a lead vessel and the solution crystallized.

At Tolfa, where the alum-stone contains an excess of alumina, the ore is calcined in order to render insoluble the excess of hydrate of alumina; to effect this, the stone is broken up into small pieces, which are carefully calcined till sulphurous fumes begin to be given off. Whenever this happens, the calcined stone is removed to stone cisterns, where it is moistened with water; in a few days the calcined stones crack and swell, becoming like slacked quicklime. After being thus moistened and exposed for some months, they are thrown into leaden boilers and boiled with water for about twenty-four hours, the water as it evaporates being replaced by the mother-liquor of a former crystallization. When the ore has been thus boiled for a sufficient length of time the fire is extinguished, and the sediment having subsided, the clear liquor is run into wooden reservoirs, where it crystallizes; in these it remains about fourteen days, the mother-liquor being used as described for dissolving the calcined ore.

The alum obtained by this process is what is termed Roman alum, and is considered the purest to be found in commerce. It frequently occurs in cubical crystals, and generally is coated with a reddish-brown powder resembling ochre, but which is said to contain no iron, being put on by the manufacturers to give their alum the peculiar appearance which characterizes the Italian salt.

The alum made in this country is not obtained in this way, as neither potash nor sulphuric acid exist ready-formed in sufficient quantity in any of our shales, so that the processes adopted are necessarily more complicated.

The alum ore is an aluminous shale containing carbonaceous matter, and diffused iron pyrites. By the calcination of this shale the sulphur is oxidized, and unites with the alumina, iron, and other bases, forming sulphates. By digesting the calcined ore in water, and adding a salt of potash or ammonia to the liquor, alum is obtained.

At Hurler the method formerly pursued was to leave the alum

shale exposed to the air in the mine, which has always a temperature of from 60° to 70° (Fahrenheit) for several years. By this treatment the ore becomes gradually covered with a white efflorescence of sulphate of iron and alumina, having an appearance like asbestos. The following are analyses of this hair salt, which I made at Dr. R. D. Thomson's laboratory at St. Thomas's Hospital under his superintendence; and here I take the opportunity of thanking him for the kind assistance he afforded me in these and subsequent analyses. A trace of potash is present.

	1.	2.	3.	4.	5.	Average.	Atoms.
Alumina .....	9·64	9·36	9·04	7·16	6·32	8·30	1·20
Protoxide of iron..	18·00	18·36	17·92	18·68	19·27	18·44	4·09
Lime .....	·24						
Sulphuric acid ...	34·36	33·60	35·20	33·92	34·93	34·40	6·88
Water.....	36·80	37·16	37·04	40·32	40·77	38·41	34·10
	99·04	98·48	99·20	100·08	101·29	99·55	

These analyses show this salt to be composed of 1 atom tersulphate of alumina united to 4 atoms protosulphate of iron and 34 atoms of water, or  $Al^2O^3 3SO^3 + 4FeO SO^3 + 34HO$ .

The decomposed ore was then lixiviated, and the black residue thrown upon the hill, as it was termed, or mass of previously lixiviated shale. This hill, by further oxidation under the influence of the atmosphere, afforded another product of alum. The lixivium was then evaporated, and the copperas allowed to crystallize out. To the solution from these crystals a salt of potash or ammonia was added, when alum was deposited. These crystals, after being washed, were roached in the ordinary manner.

As the demand for alum increased, this process was found too tedious; the fresh or slightly decomposed ore was therefore taken and calcined, thus accelerating the natural process of decomposition. To ascertain the composition of the fresh shale I analysed it, and the results are as follows:—

Specific gravity 2·54.

25 grs. fused with carbonate of soda gave—

		Per cent.
Silica . . .	12·07	48·28
Alumina . . .	6·74	26·96
Peroxide of iron	2·89	11·56 = 10·41 FeO = 8·09 Fe
Carbonate of lime	1·07	2·38 CaO.

100 grs. dried till it ceased to lose weight at 300° F. lost 2·02 per cent. water.

25 grs. deflagrated with pure nitre and carbonate of soda, and the filtered solution acidulated and precipitated by Ba Cl, gave—

		Per cent.
BaO SO <sup>3</sup>	10·76 . . .	5·93 sulphur.
...	10·57 . . .	5·87 ...
...	11·02 . . .	6·08 ...
Average . . . . .		5·94 ...

To determine carbon and hydrogen, the dried shale was burned with chromate of lead, with the proper precautions.

		Per cent.
16·65 grs. gave	2·42 CO <sup>2</sup> . . .	3·96 carbon.
	1·60 HO . . .	1·06 hydrogen.
14·51 grs. gave	2·13 CO <sup>2</sup> . . .	4·00 carbon.
	1·41 HO . . .	1·08 hydrogen.
Average . . . . .	Carbon.	Hydrogen.
	3·98	1·07

The total iron present in the shale is 8·09 per cent., and 5·94 per cent. of sulphur require 5·19 of iron to form bisulphide with it. Deducting 5·19 from 8·09, we have 2·90 per cent. iron, or 3·72 per cent. protoxide uncombined with sulphur. The nitrogen was obtained by combustion with soda lime, and precipitation by bichloride of platinum, 17·83 grs. giving—

Platinum ·76 = ·62 per cent. nitrogen.

I also found potash in small quantity. 200 grs. of shale gave ·78 KCl = ·24 per cent. KO.

The complete analysis will stand thus:—

Silica . . . . .	48·28
Alumina . . . . .	26·96
Protoxide of iron . . . . .	3·72
Lime . . . . .	2·38
Magnesia . . . . .	trace
Potash . . . . .	·24
Bisulphide of iron . . . . .	11·13
Water . . . . .	2·02
Carbon . . . . .	3·98
Hydrogen . . . . .	1·07
Nitrogen . . . . .	·62

100·4

This shale lies in seams which vary in thickness from a few inches to about three feet and a half. It underlies the limestone, and is deposited above the coal. It is very hard as it comes from the mine, but soon decomposes when exposed to the air. Some-

times small particles of the pyrites are visible when it is fractured, but in general this is not the case. There is another seam of alum ore which is called by the miners the Duff bed, lying above the limestone. This mineral, although not so suitable for making alum as the other, is still of great value for that purpose. It has almost the same composition as the ordinary alum ore.

The sketch of a portion of a section of a Hurllet pit shows the relative positions of the minerals mentioned. After the coal has been dug out, leaving pillars at regular intervals to support the roof, the alum shale is taken out, and then the limeblowers, as they are called, proceed to remove the limestone by blasting it with gunpowder.

I find this limestone to have a specific gravity of 2.71, and to be composed of—

	feet.	in.
Ironstone.		4
Duff bed.	1	9
Limestone.	4	6
Alum ore.	1	6
Coal.	5	3

Carbonate of lime . . . . .	88.814
Carbonate of magnesia . . . . .	1.182
Carbonate of iron . . . . .	4.764
Bisulphide of iron . . . . .	.395
Silica . . . . .	3.412
Carbonaceous matter . . . . .	trace
Water . . . . .	1.075
	<hr/>
	99.642

In calcining the ore for alum-making, a flue of brick is constructed upon the ground, and upon this brushwood and coals are laid which are ignited, and alum shale placed above them. When these have become ignited, another layer of ore is deposited upon it, while at the same time waste alum shale is spread over it to prevent the combustion from being too rapid. The presence of carbonaceous matter and sulphur assists the combustion, and renders a further addition of fuel unnecessary. In from twelve to eighteen months the calcination is ended, and the ridges being broken up, the ore is wheeled to the steeps to undergo the process of lixiviation. The composition of the calcined shale is as follows:—



	1.	2.	Average.			
Insoluble.	Silica . . .	35.28	38.31	36.79		
	Alumina . . .	14.48	14.21	14.34		
	Peroxide of iron	21.40	21.67	21.53		
	Lime . . .	3.26	2.56	2.91		
	Magnesia . . .	trace	74.42	76.75	75.57	
Soluble.	Sulphuric acid	8.88	8.86	8.87		
	Alumina . . .	2.92	1.24	2.08		
	Peroxide of iron	1.80	1.56	1.68		
	Lime . . .	.87	1.52	1.19		
	Water . . .	10.72	25.19	11.16	24.34	10.94
		99.61	101.09	100.33		

The ore is thrown into square cisterns of stone having wooden bottoms; mother-liquor is run upon it, and the liquor percolates slowly through the bottom of the steep into cellars beneath. After three or four washes with mother-liquor, pure water is employed. From four to eight washes in all are drawn off; but this varies with the quality of the calcined ore. When the proper number of washes has been taken off, the insoluble residue is removed and used for covering the ridges of calcining ore. This exhausted material consists of—

Silica . . . . .	40.39	} Insoluble	90.52
Alumina . . . . .	17.25		
Peroxide of iron . . . . .	30.66		
Lime . . . . .	2.22		
Sulphuric acid . . . . .	1.92	} Soluble	10.20
Peroxide of iron and alumina . . . . .	.92		
Lime . . . . .	1.00		
Water . . . . .	6.36		
			100.72

The composition of the first four washes from the steeps I have found to be as follows in the gallon:—

	No. 1. Sp. gr. 1188.	No. II. Sp. gr. 1129.	No. III. Sp. gr. 1100.	No. IV. Sp. gr. 1090.
Sulphuric acid	933.05	595.05	456.90	415.35
Peroxide of iron	346.75	118.68	112.42	53.31
Alumina . . .	194.59	149.99	59.42	91.50
Protoxide of iron	73.18	126.92	85.11	138.45
Chlorine . . .	27.44	42.85	38.54	36.60
Magnesia . . .	14.13	14.83	16.06	21.48
Lime . . . . .				
Potash . . . . .	75.67	60.98	28.90	27.05
Grs. per gallon	1664.81	1109.30	797.35	783.74

The potash found in these analyses is derived from the mother-liquor which is thrown upon the raw shale previous to calcination; and as muriate of potash is the salt employed, it accounts also for the chlorine. The shale from which these liquors were ob-

tained must have been different from what I examined, as magnesia and no lime existed in these liquors. The liquors are pumped up from the cellars into wooden reservoirs, whence they are allowed to flow into the evaporating boilers. These last are about 60 feet long, 7 feet broad, and from 4 to 5 deep. They are arched over and are made of fire-bricks. At one end is the furnace, and at the other the chimney. The flame and hot air pass over the liquor, and the draught carries off the steam as it rises. During the evaporation, a brownish or buff-coloured substance falls in considerable quantity, which is commonly called a sub-sulphate of alumina; but a specimen which I analysed I found to contain upwards of 50 per cent. sulphuric acid, 25 of peroxide of iron, and about 3 or 4 of alumina. When the liquor has attained the strength  $65^{\circ}$  to  $70^{\circ}$  of Twaddel's hydrometer (1325 to 1350 spec. grav.), the boiler is opened and the liquor run into the alum-coolers, which are made of stone. The proper quantity of muriate of potash or sulphate of ammonia is added either in solution or dry; if dry, the mixture is agitated by wooden poles to cause the salt to dissolve and be equally diffused. The coolers are left at rest for about a week, at the end of which time the mother-liquors are made to flow into a tank for the purpose, and the first crystals of alum collected and washed with cold water. These crystals are of a brown colour, being contaminated by iron, and contain,—

Sulphuric acid . . . . .	34·33
Alumina . . . . .	6·44
Peroxide of iron . . . . .	4·52
Potash . . . . .	2·33
Ammonia . . . . .	2·81
Chlorine . . . . .	·36
Water . . . . .	49·31

100

The first alum is dissolved in mother-liquor from the second crystallization by means of steam. This operation is performed in a cistern lined with brick about 9 feet deep, to the bottom of which a steam-pipe passes. When the alum has dissolved, the steam is shut off and the deposit allowed to subside. The liquor is run into stone coolers, where the second alum crystallizes. This I found was composed of—

Sulphuric acid . . . . .	36·08
Alumina . . . . .	9·60
Peroxide of iron . . . . .	·88
Ammonia . . . . .	3·83
Potash . . . . .	·73
Chlorine . . . . .	trace
Water . . . . .	48·88

100

The second alum is dissolved for the last time in vessels of lead into which steam is passed: these vessels are called roaching pans. The solution is closely covered over and allowed to settle, and is then run into the roaching-tubs, where it deposits itself in crystals, shooting towards the centre of the tubs. The tubs are simply large wooden casks lined with lead: when the alum has attained a sufficient degree of hardness, the staves are removed, a hole is made near the bottom of the cask to allow the mother-liquor to run off into the cellar for that purpose, and the alum broken up into lumps or ground, as the case may be. The mother-liquor is again evaporated, as in the first process of alum making, when it deposits a crop of first alum. The pure alum contained—

Sulphuric acid	. . .	35·57
Alumina	. . . .	11·40
Peroxide of iron	. . .	trace
Ammonia	. . . .	3·42
Potash	. . . .	·83
Water	. . . .	48·78
		100·

In preparing potash alum, the muriate is the salt generally used; hence small quantities of chlorine are often to be found in the alum. By the process I have described, three parts of shale produce one part of marketable alum.

In 1836, and for several subsequent years, alum was made at Hurler in the following way. Alum shale containing the smallest amount of sulphur was calcined in a reverberatory furnace; it was then ground and sifted, the powder was thrown into lead cisterns and hot dilute sulphuric acid run upon it. A strong action occurred, and the greater part of the shale dissolved. The solution was evaporated and the alkali applied in the usual way; the sulphate of ammonia was at that time made by distilling gas liquor into the mother-liquor of the alum; oxide and sulphide of iron were thrown down, and the solution consisted of sulphate of ammonia.

In Sweden, where wood is the common fuel, and consequently very expensive, the alum shale, which is very carbonaceous, is used for performing the calcination, an operation occupying about a month or six weeks. The late Dr. Thomas Thomson, in his 'Travels in Sweden,' tells us that so great is the quantity of combustible matter in the Swedish alum shale, that he saw it employed for burning limestone. In the same way the shale is used for the evaporation of the alum liquors, the shale being calcined and the evaporation performed simultaneously. Wood is also used in Sweden for the calcination of the alum shale, as

appears from a letter to the late Charles Mackintosh, Esq., published in his memoir, in which the process followed at Haensetter is thus described:—"Their process of alum making is nearly as follows: the schistus, which has something the appearance of our undecomposed ore, lies under a stratum of limestone about 2 feet from the surface of the earth, and upon being quarried is built into kilns in the following manner: first, a layer of fir or rather pine-leaves or branches intermixed with birch; second, a layer of schistus which has been used as fuel in the evaporating or roasting boilers; third, a layer of raw schistus, and so on in rotation as before. After burning, the schistus is put into steeps, which are nearly the same as ours at Hurler; water is poured upon it, the same water being poured upon five steeps before it is fit for use. The liquor is then run into settling cisterns, after remaining some time in which it is next put into the first boilers for evaporation. These boilers, which are of lead, are constructed in the following manner:—Six of them, 4 feet diameter each, are built in one storey, in two rows, with three fires under them; they are made of lead, open above and arched in the bottom, which is of cast iron. From these boilers the liquor is run into wooden coolers which are very small, 3 feet long and  $2\frac{1}{2}$  feet broad, with three wooden partitions in each. The crystallized alum is afterwards washed and roached exactly in the same manner as it is with us. Their roaching-casks are about twice as large as ours, and require to stand for about three weeks. From this it appears that the small quantity of alkali formed from burning the schistus with wood is sufficient for their purpose."

The Kinekulle slate in Dr. Thomson's museum at St. Thomas's Hospital, I found to have the following composition:—

	Specific gravity, 2'42.
Silica . . . . .	52·28
Alumina . . . . .	16·64
Protoxide of iron . . . . .	6·96
Iron } . . . . .	3·61
Sulphur } . . . . .	4·13
Potash . . . . .	7·98
Lime . . . . .	1·53
Magnesia . . . . .	1·10
Water . . . . .	1·40
Carbonaceous matter . . . . .	4·37

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100

The calcined Swedish ore generally has a brown colour, but when it assumes a red appearance it is found to be not so productive in alum.

At Whitby the calcining heaps are from 90 to 100 feet in height. The stratum of aluminous shale is about 29 miles wide. Its specific gravity is about 2.40, and the upper part of the rock is always the most abundant in sulphur. It contains a large amount of bituminous matter, which renders great care necessary to regulate the combustion, as it is apt to burn too rapidly and drive off portions of sulphur. About one ton of alum is produced from 130 tons of calcined shale.

In some alum ores considerable quantities of magnesia exist. When this is the case, Epsom salts are produced in the process of calcination, and being more soluble than alum, remain in solution after it has crystallized. To obtain the Epsom salts, the liquor is evaporated sometimes by waste heat from the alum boilers when the salt crystallizes, often in considerable quantities; one work in Yorkshire producing annually about 800 tons of this salt.

In certain specimens of Hurlet shale magnesia is found, sometimes as much as  $1\frac{1}{2}$  or 2 per cent., but there was no appreciable amount in what I examined. Formerly an efflorescence of nearly pure sulphate of magnesia was to be found in the mines in considerable abundance, but of late years its occurrence has been less frequent; it consists of long, transparent, brittle needles, often more than 2 inches long. A specimen from Hurlet in Dr. Thomson's museum at St. Thomas's Hospital, which he kindly permitted me to analyse, I found consisted of,—

	Per cent.	Atoms.	Epsom salt.	
Sulphuric acid . . . . .	33.17	1	32.52	1
Magnesia . . . . .	15.42	1.07	16.25	1
Protoxide of iron . . . . .	1.28			
Alumina . . . . .	.23			
Water . . . . .	49.91	7.2	51.23	7
	<hr/>		<hr/>	
	100.01		100.	

The formula is  $(MgO FeO) SO^3, 7HO$ .

A specimen from Peru resembling this in appearance, also in Dr. Thomson's museum, contained—

		Atoms.
Sulphuric acid . . . . .	35.96	7.19
Magnesia . . . . .	2.54	1
Alumina . . . . .	15.88	2.2
Water . . . . .	45.80	39.6
	<hr/>	
	100.18	



Alum is also made from clay. For this purpose the purest clay is calcined in a reverberatory furnace at a moderate heat,

This process has the effect of peroxidizing the iron, and rendering the clay more porous. The heat must be regulated carefully, otherwise the clay undergoes a commencement of fusion, in which condition it is nearly insoluble in acid. About 45 per cent. of its weight of sulphuric acid, of specific gravity 1.454, heated by the waste heat of the furnace to about 160° F., is run upon it and digested with it for some months, at the end of which time the mass is lixiviated, concentrated, and the proper quantity of alkali added: the alum precipitates and is purified in the ordinary way.

Concentrated alum or tersulphate of alumina is an article introduced of late years into commerce, for which there is a considerable demand. The objection to its more general use is, that its composition is not always the same. It is prepared by dissolving clay in sulphuric acid, and adding ferrocyanide of sodium to precipitate any iron which the acid has dissolved, as prussian blue. The clear liquid is evaporated till it becomes solid on cooling. The prussian blue is boiled with caustic soda to reconvert it into prussiate of soda, thus avoiding any loss in this respect.

The two analyses given were made upon a sample of concentrated alum sent by Messrs. H. Patinson and Co., of Newcastle-on-Tyne.

The sulphuric acid was precipitated by chloride of barium. 25 grains gave—

I. Sulphate of barytes	27.89 = SO <sup>3</sup> per cent.	38.46
II. ...	27.61 = ...	38.08

25 grains gently heated and then strongly ignited left,—

I. Alumina . . .	4.84 = alumina per cent.	19.36
II. ...	4.55 = ...	18.20

The sulphuric acid per cent. and alumina being deducted, leave,—

I. Water per cent. . . . .	42.18
II. ... . . . .	43.72

	I.	II.	Average.	
Alumina . . .	19.36	18.20	18.78	1
Sulphuric acid . . .	38.46	38.08	38.27	2.6
Water . . . .	42.18	43.72	42.95	13.2
	100	100	100	



or



LIX. *The Polymagnet.* By PROFESSOR TYNDALL, F.R.S. &c.

[With a Plate.]

THE polymagnet was devised for the purpose of exhibiting before a class of pupils as many as possible of the phenomena of electro-magnetism and diamagnetism.

The instrument consists of an arrangement of two horseshoe electro-magnets, a helix of covered copper wire disposed between them, and suitable means of suspension.

A section of one of the electro-magnets and its surrounding spirals is given, fig. 1, Plate IV. *ab, cd* are two cylindrical cores of soft iron, which are united by a cross-piece of the same material, *ef*. Through the cross-piece pass the strong screws *g* and *h* into the cores, and by them the ends *b* and *d* of the cores, which are accurately planed so as to ensure perfect contact with the cross-piece, are attached to the latter. The diameter of the cores is 1.125 inch, and their distance apart, from centre to centre, 4.85 inches; the cross-piece *ef* is drawn in proportion.

Round each core is a helix of copper wire, overspun with cotton, which was afterwards saturated with shell-lac. In winding the helix, two lengths of wire, one covered with red cotton and the other with green, were laid side by side and coiled as a single wire. The diameter of the wire is 0.1 of an inch, and the weight of it which surrounds each limb of the magnet is 12 lbs. For all four limbs, therefore, a weight of 48 lbs. is made use of.

The second electro-magnet is in every respect similar to the one just described.

Fig. 2 is a front view of a helix of covered copper wire, intended to be placed between the two electro-magnets; it has an internal diameter, *ab*, of 1 inch; an external diameter, *cd*, of 8 inches, and measures along its axis 1.15 inch. The diameter of its wire is 0.065 of an inch, and its weight is 6 lbs.; it is wound so as to form a double coil, as in the case of the electro-magnets. The radial strips, and central and surrounding ring seen in the figure, are of brass, and hold the coils of the helix compactly together.

Fig. 3 represents a stout slab of mahogany which supports the apparatus. *ab, cd* are hollows cut in the slab to receive the cross-pieces of the two electro-magnets; from *e* to *f* the slab is cut quite through, the cross-pieces merely resting on the portions between *f* and *b*, *f* and *d*, &c. The small apertures at *x, x'* show where the screws enter which attach the cross-piece to the slab of wood. The central aperture at *g* shows where the pin *g* of the helix, fig. 2, enters, the helix thus occupying the central portion of the board. Right and left are two projections for the reception of two current reversers, which will be described im-

mediately. The apertures 1, 2, 3, 4 are for the reception of pins projecting from a glass case intended to cover the whole apparatus.

When the magnets and central helix are fixed in their places and looked down upon, their appearance is that represented in fig. 4; at *a* and *c* the tops of the cores are seen, the moveable masses of soft iron which belong to them being removed; the two ends of the other electro-magnet bear two such masses, each formed from a parallelopiped 4.5 inches long, 2 inches wide, and 1.25 inch high, having one end bevelled off so as to render it pointed, the other end being suffered to remain flat. The distance between those moveable masses may be varied, or the body to be examined may be suspended either between *surfaces* or *points*, according to the nature of the experiment. The projections of the current reversers upon the horizontal plane are seen right and left.

Simplicity and efficiency being the objects aimed at, a current reverser was devised which fulfils these conditions. A front view of the instrument is given in fig. 5, and its horizontal projection in fig. 6. *Q* is the section of a quadrant of wood, which is capable of being turned by the handle *HP*; *ab* is the section of a strip of brass laid on the periphery of the quadrant; *cd* is a shorter strip similarly laid on; between *b* and *c* is a gap, formed of the wood of the quadrant itself, or of a piece of ivory or glass inlaid; *s* and *s'* are two brass springs\*, which are shown in the figure to rest upon the strips of brass *ab* and *cd*; *M*, *M'* are two clamps secured to the wooden pillars *O* and *O'* by screws *S* which pass up through the latter. The plan below corresponds to the section above. From *b*, fig. 6, the strip of brass crosses to *c'*, and from *c* to *b'*, both being insulated from each other at *R*. Supposing, then, the two clamps *M* and *L* to be connected with the two poles of a galvanic battery, the current entering at *M* would flow along the spring *s* to *b*, thence to *c'*, and finally along the spring *s'* to the clamp *L'*: in like manner the current entering at *L* would attain the clamp *M'*. In this position of things the handle of the instrument leans to the left, as in fig. 5. If the current is to be interrupted, this is secured by setting the handle vertical; for when the handle is in this position, the spring *s'* rests upon the non-conducting surface *bc*, and the circuit is broken. If it be desired to send the current direct from *L* to *L'*, and from *M* to *M'*, this is accomplished by causing the handle to lean to the right; when this is done, both the springs *s*, *s'* rest upon the self-same strip of brass *ab*, and there is direct metallic communication between *L* and *L'*, and between *M* and *M'*. This reverser has been tested practically,

\* Copper, I think, would be better than brass.



and found very convenient. It is very similar to an instrument devised by Prof. Reusch, but simpler and more easily constructed.

Fig. 7 is a plan of the top of the glass case which surrounds the polymagnet. At C a brass tube is cemented to the glass, which is here perforated, and through the tube a rod passes furnished at its lower extremity with a hook, to which is attached a suspending fibre. *ab* is a horizontal brass cylinder capable of being turned on its axis by the milled head at *a*, and thus coiling a suspending fibre around a groove marked at *d*: the cylinder is also capable of sliding right and left, so that the body suspended from the fibre may be moved laterally, and the amount of motion measured on the graduated bar above\*. *a'b'* is another horizontal suspension rod, in every respect similar to the former.

The whole instrument, surrounded by its glass case, is shown in perspective in fig. 8. The magnets are visible, with the moveable masses of soft iron resting upon them; in the centre is seen the helix sketched in fig. 2, and within the helix a bismuth bar supported by several fibres of unspun silk attached to the central rod which passes through the top of the glass case. The manner of suspension of the bismuth will be understood from the drawing, certain practical artifices which suggest themselves when the drawing is attentively inspected, being introduced to facilitate the placing of the axis of the bar along the axis of the surrounding helix. The current reversers are seen *without* the case; two opposite sides of the latter can be opened by the handles *h* and *h'*, so that free and easy access to the interior is always secured.

*Experiments to be made with the Polymagnet.*

1. All the experiments that are usually made with an upright electro-magnet.

2. The various portions of the instrument may with great facility be lifted separately out of the case. One of the electro-magnets being thus removed, a rope can be passed through a ring *r*, introduced for this purpose into the cross-piece, fig. 1: adjacent to the screws *g* and *h* two plates of brass are seen; these are attached to the brass reels of the helices, and by passing a pin through the holes shown in the figure, the helices are prevented from falling when the magnet is turned upside down. Attaching the rope to a hook in the ceiling, or to a strong frame made for the purpose, experiments on the lifting power of the magnet may be made.

3. While one of the magnets is suspended as last described, the other, which is of exactly the same size, can be brought up

\* This arrangement, though very convenient for private research, is not necessary for lecture experiments.

against it, the free ends of the four cores being thus in contact. The same current being sent through both magnets, we have the mutual attraction of two electro-magnets instead of the attraction of an electro-magnet for a mass of soft iron, as supposed in the last experiment. The arrangement just described is indeed precisely that devised by M. Pouillet in the construction of a powerful electro-magnet for the faculty of sciences at Paris. To the cross-piece of the second magnet a ring is also attached, from which weights can be suspended.

4. The cross-pieces may be removed by withdrawing the screws *g* and *h*, and the spirals may be made use of singly with their corresponding bar-magnets. As two wires surround each coil, one of them may be used to exhibit the induced currents developed by the other. The phenomena of the extra-current may also be studied, and the remarkable effect produced by connecting the two ends of one of the wires, or the spark of the extra-current in the other, may be exhibited.

The milled head *a*, fig. 7, can be screwed off, and the rod *ac* pushed downwards into the case; the helix in the centre can also be readily lifted out of its position and removed from the glass case—we will suppose this done. The two electro-magnets alone are now within the case, and the view is uninterrupted, which would not be the case if the helix had been permitted to remain.

The fibre hanging from the groove *d*, fig. 7, can be so arranged that any substance attached to it shall hang between the moveable masses of soft iron which surmount the electro-magnets, and the same arrangement can be made for the fibre suspended from the groove *d'*.

But a body suspended between the moveable masses of soft iron would be hidden by these masses from the audience, and hence to render the motions of the body visible the following expedient was adopted:—fig. 8*a* represents a thin index of ivory about 4 inches long, and shaped as in the figure; from the centre depends the stem *dc*, which is terminated by a tongs-shaped arrangement which can clasp the body to be submitted to experiment; to the right of the index a section of the little ivory pliers, by a plane passing through the stem at right angles to *in*, is given; the stem is slit up to *a*, so as to allow of the pliers being opened to receive the body to be examined, which they then clasp in virtue of the elasticity of the ivory. The stem *de* is of such a length, that when the body is in the centre of the space between the poles, the index *in* is seen above them; and, as the index follows all the motions of the body underneath, these motions are recognized by all who see the index.

5. If an ordinary magnetic bar, sufficiently feeble, be suspended

between one pair of poles, and an ordinary diamagnetic bar between the other pair, on sending the same current round both magnets, the index of the former sets itself parallel to the polar line, while the index of the latter sets itself perpendicular to the polar line, and thus the phænomena of magnetism and diamagnetism address the eye simultaneously.

6. In the same way, if a normal\* magnetic bar be suspended between one pair of poles, and an abnormal magnetic bar between the other, the antithesis of their deportment may be made manifest. The same antithesis is exhibited when we compare a normal diamagnetic bar with an abnormal one.

7. And when between one pair of poles is suspended a normal magnetic bar, and between the other pair an abnormal diamagnetic one, the apparent identity of deportment of both bars is rendered evident at once. The same identity is shown when we compare the abnormal magnetic bar with the normal diamagnetic one.

8. Causing the points to face each other, instead of the flat ends of the poles, and observing the directions given in the paper spoken of, the curious phænomena of rotation on raising or lowering the body from between the points, first observed by M. Plücker, and explained in the paper referred to, may be exhibited.

9. To show that a bar of bismuth, suspended within a helix and acted upon by magnets, presents phænomena exactly analogous to those of soft iron, only always in opposite directions, let the flat helix be replaced between the two electro-magnets. The bar of bismuth used in experiments with the instrument now described is 6 inches long and 0.4 of an inch in diameter. Suspended so as to swing freely within the helix, its ends lie between the moveable masses of iron which rest upon the electro-magnetic cores. Four poles are thus brought simultaneously to bear upon the bar of bismuth, and its action is thereby rendered both prompt and energetic. The two poles to the right of the bar must both be of the same name, and the two to the left of the bar of the opposite quality. If those to the right be both north, those to the left must be both south, and *vice versa*. On sending a current from 10 or 15 cells round the helix, and exciting the magnets by a battery of 4 or 5 cells, the current reversers place the deflections of the bar entirely under the experimenter's control. By changing the direction of the current in the helix

\* For the explanation of normal and abnormal bars see a memoir "On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet," to be published in the forthcoming part of the Philosophical Transactions. The paper will, in due time, be transferred to the pages of the Philosophical Magazine.

by means of its reverser, a change of deflection is produced; the same is effected if the polarity of the magnets be changed by the reverser which belongs to them. For a full description of all these phenomena I must refer the reader to the paper on the nature of the diamagnetic force, already mentioned.

10. To those acquainted with what has been done of late years in diamagnetism, numerous other experiments will suggest themselves. The antithesis of two isomorphous crystals, one magnetic and the other diamagnetic, the general phenomena of magnecrystallic action, and the analogous effects produced by pressure, may all be exhibited.

By placing one of the helices of the electro-magnet upon the other, a coil of double length is obtained, and two such coils may be formed from the four which we have described. For the additional expense of the iron merely, a single electro-magnet, far more powerful than either of the others, because excited by twice the quantity of coil, may be obtained.

I think it would be an improvement if the suspensions were independent of the glass case, so as to permit of the entire removal of the latter. The best way of showing the deflection of the bismuth bar within the central helix to a large audience, is to attach a long, light index to the bar itself, and permit this index to enter a French shade which will protect it sufficiently from currents of air. With this arrangement the motions are strikingly evident, and may be seen by hundreds at once. The instrument above described was constructed by Mr. Becker, of Newman Street, and its cost is about twenty-four pounds. It was not my intention originally to have so much wire round the electro-magnets; and the effects may also be made manifest with a smaller central coil. I have no doubt that with 8 lbs. of wire round each limb of the electro-magnets, and a central coil weighing 4 lbs., the experiments might be exhibited to a large audience with perfect distinctness. A sensible diminution of cost would of course accompany this diminution of material and labour.

#### LX. *Note on the Aberration of Light.*

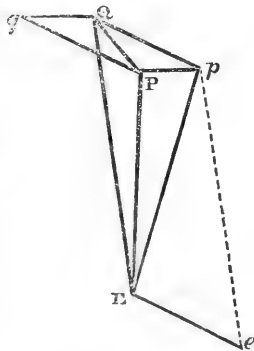
By the Rev. Professor CHALLIS, M.A., F.R.S., F.R.A.S.\*

**T**HE effect of the aberration of light on the apparent place of any body of the solar system, is taken account of by the astronomical calculator on the following principle:—The apparent direction of the body, as affected by aberration, is its real direction at the instant the light by which it is seen started from it. This rule is enunciated by Dr. Young in vol. ii. of his

\* Communicated by the Author.

'Natural Philosophy' (p. 294), but is not accompanied by proof, and I have not met with any proof of it elsewhere. In order to prove the rule completely, it is necessary first to explain the cause to which aberration is due. In vol. iii. (1852) of the *Philosophical Magazine* (p. 53), I have proposed an explanation of aberration which may be briefly stated as follows. In every determination of the apparent place of a celestial object by an optical instrument, the light from the object passes actually or virtually through *two* points rigidly connected with the instrument. One of these is necessarily the optical centre of the object-glass, and the other is a point in the field of view of the telescope, which we may suppose to be coincident with the intersection of two wires. Let, at the instant of an observation,  $O$  be the position of the first point, and  $W$  that of the other. Then the instrument gives by its graduation and by a note of time, the direction of the line joining  $O$  and  $W$  referred to certain fixed directions. If the transmission of light were instantaneous, this direction would coincide with the path of the light, supposing the path to be rectilinear in the short space  $OW$ . But if light occupies a sensible time in passing between the two points, in that time the position of the optical centre of the object-glass has changed, by reason of the earth's motion, from  $O'$  to  $O$ ; so that  $O$  and  $W$  being simultaneous positions of the *direction-points*, the instrument gives  $OW$  for the path of the light, the real path being  $O'W$ . It is found by observation that the difference between these two directions is the whole amount of the aberration of a star. This general explanation of aberration being admitted, the following is the proof of the rule above mentioned.

Let  $e$  and  $p$  be simultaneous positions of the observer and the object, and while light is travelling from  $p$  to the observer, let the observer be carried by the earth's motion from  $e$  to  $E$ . Then, leaving out of consideration any causes, such as atmospheric refraction, which may affect the course of the ray before it enters the telescope, the object is seen by the observer when at  $E$  by a ray which has described the path  $pE$ . Draw  $pQ$  parallel and equal to  $eE$ , and join  $EQ$ . Thus by reason of aberration as above explained, the *instrumental* direction of the



object is EQ. In the time that light takes to pass from  $p$  to E, let the object move from  $p$  to P, so that EP is its real direction at the instant of observation. Consequently the  $\angle QEP$  is the difference between the instrumental and the real directions. Draw Qq parallel and equal to pP, and complete the parallelogram QqPp. Now the earth being conceived to be at rest, the apparent angular motion of the body in the interval of the transit of light from  $p$  to E is the angle subtended at E by the resultant QP of the motion Qq of the body and the motion Qp equal and opposite to that of the earth, viz. the  $\angle QEP$ . Hence EQ, the instrumental direction of the body at the time of observation, coincides with its actual direction at a time preceding the observation by the interval occupied by the passage of the light from the body to the observer. This result establishes the rule it was proposed to demonstrate.

It follows from this theory, that the instrumental direction of a terrestrial object, whose motion in space is parallel and equal to that of the observer, coincides with the actual direction; and that the instrumental direction of a fixed object, as the sun, and of an object having a fixed direction, as a star, differs from the actual direction by the whole amount of instrumental aberration. The theory also shows that the value of the constant of aberration may be in some degree affected by the retardation which the light undergoes in that part of its course between the direction-points O and W, which lies within the substance of the object-glass, or passes through other glasses.

Cambridge Observatory,  
May 11, 1855.

LXI. *On the Periodical Variations of Terrestrial Magnetism.*  
By A. SECCHI, Director of the Observatory of the Collegio Romano\*.

[Concluded from vol. viii. p. 399.]

PART II. § 2. *On the Variations of the other Magnetic Elements.*

THE two magnetic elements, the variations of which remain to be determined, are the intensity and the inclination. But as their direct determination is very difficult, an indirect method is used, which consists in finding the variations of the components of the total force resolved along three coordinate axes taken with reference to the horizon.

The position of the dipping-needle shows the direction of the resultant of all the magnetic forces of the globe; and the direct method of determining the intensity of this force would be to

\* Communicated by Mr. Archibald Smith.

observe the oscillations of the dipping-needle itself. This method has, however, several inconveniences.

The first is, that as the time of an oscillation depends on the intensity of the magnetism of the needle as well as of that of the earth, a change in the one might be attributed to the other, and there are no means of determining in which it has taken place. Another disadvantage is, that as such experiments require a considerable amount of time and no ordinary skill, they may give a great deal of trouble to the observer, and yet leave it uncertain whether the magnetism of the needle and of the earth have remained constant in the interval occupied by the observation.

For these reasons, philosophers, following the steps of Gauss, have abandoned the direct measurement of the total force, and for the same reasons that of the variations of the inclination; and have adopted the following method of investigation.

Conceive the total force resolved along three rectangular co-ordinate axes; one horizontal and parallel to the magnetic meridian, another horizontal and at right angles to the magnetic meridian, the third vertical. For each of these directions there is a particular apparatus for determining the magnitude and the variations of the components; and from these, the direction of the resultant being known, the value and variation of the total force and inclination are easily deduced. The instrument which is used to measure the horizontal component is called the *unifilar magnetometer*. Its office is to give the absolute value of the horizontal force. But the description of the method pursued would lead us too far from our subject\*. It may suffice to say, that these experiments likewise can only be made at distant epochs, and therefore what we have already remarked of vibrations of the dipping-needle is equally applicable to them, viz. that they cannot be used to determine the horary variations, or those of short duration. These last are usually deduced from the variations in the position of a bar placed at right angles to the magnetic meridian. The apparatus used for this is called the *bifilar magnetometer*, a name descriptive of its construction.

It consists of a bar-magnet suspended by two parallel threads in a state of equal tension, which, were a non-magnetic body sus-

\* For the theory of this observation see Gauss's memoir *Intensitas*, &c.; for the practice see Weber's memoirs published in Taylor's *Scientific Memoirs*, and the instructions in the introduction to the *Annuaire Magnétique et Météor.* of Russia. The method is shortly this: the declination needle is made to oscillate, first loaded with certain weights, and then without them. By this means the magnetic moment of the needle, multiplied by the intensity of the terrestrial force, is obtained; next, a bar magnet is placed at different distances from the needle, and from the deviations which it produces, the value of the force divided by the magnetic moment is deduced, and from these the magnetic force may be easily obtained.

pended from them, would remain in the plane of the meridian ; but a magnetized bar being suspended, their line of suspension undergoes a torsion until the bar takes a direction at right angles to the magnetic meridian. The force of torsion exerted by the threads counteracts the magnetism of the earth, and the needle is rendered in a manner astatic, so as to change its position with the slightest variation of the terrestrial force. Observations with this instrument are made, with the same ease as those of the declination, by means of a mirror or collimator attached to the magnet, and observed by a telescope with a graduated scale. One precaution must be taken ; as the force of the magnet varies in intensity with the temperature, it is necessary to take account of this, so as to correct the observations for its influence by known methods of sufficient accuracy.

The vertical force is measured by means of the *balance magnetometer*, invented by Dr. Lloyd. This consists of a bar supported horizontally on knife-edges, which rest upon two agate planes, exactly like the beam of an ordinary balance. This is arranged in the plane perpendicular to the magnetic meridian, and is adjusted by means of small weights, so as to undergo a sensible inclination with the slightest change of the vertical magnetic force. Its motions are observed by means of microscopes attached to the stand of the instrument, and collimating on the intersection of spiders' webs fixed at the ends of the bar, or by observing, as before, the image of a scale reflected by a mirror attached to the bar.

When the variations of the horizontal and vertical force are obtained, the variations of the inclination are deduced from them by known formulæ. The balance magnetometer, as well as the bifilar, is subject to variations depending on the temperature, and therefore it is necessary to determine the coefficients of the correction. It does not, however, appear that its results are in all cases as certain or as exact as those of the bifilar.

#### A. Horizontal Force.

The component which we are now considering is that which is obtained from the bifilar magnetometer, arranged at right angles to the magnetic meridian. The variations may be expressed in the following manner.

*General Laws.*—The bifilar magnetometer is subject to a horary variation of a double period, diurnal and semi-diurnal ; in the semi-diurnal period the magnitude of the variation depends on the geographical latitude, and is zero at the equator ; the phase depends on the angle which the sun makes with the magnetic meridian.

We will demonstrate this by steps. Beginning with the stations of middle latitude, there is this simple law ; the curve of



the bifilar magnetometer is similar to that of the declinometer, but with a retardation of three hours.

*Explanation.*—A glance at Colonel Sabine's figures in the second volume of the Hobarton Observations, plate 1, p. 5, for the declination, and at plate 4, p. 43, for the component of the bifilar magnetometer, will be sufficient to show, that while the minimum of the declination occurs between 20<sup>h</sup> and 21<sup>h</sup>, and the maximum at about 2<sup>h</sup>, the minimum of the bifilar magnetometer occurs about 23<sup>h</sup>, and the maximum between 4<sup>h</sup> and 5<sup>h</sup>. See also the figures in which this celebrated author makes the comparison between Hobarton and Toronto in the first volume of the Hobarton Observations. At p. 34, plate 1, the curves of the declination are shown; and at p. 54, plate 2, figs. 1 and 2, that of the horizontal force; the perfect agreement of the curves will be seen, (though the scales of the abscissæ are different) and the same retardation between Hobarton and Toronto which has been already remarked in the declinations.

Next let us consider the peculiarities of the equatorial observatories. At St. Helena a singular law holds in the horizontal force. It has a single simple period, and the only indication of a secondary period is that the axis of the abscissæ is not divided by the curve into equal parts, but the diurnal part is less extended than the nocturnal. See the St. Helena Observations, p. 30, plate 4, fig. 3.

Here, then, the semi-diurnal period vanishes entirely or nearly so, and cannot be compared with that of the declination.

But we shall presently see, in the theoretical law, the true explanation of this singular fact, and it will be one of the principal proofs of the theory which we are about to expound. In the variation of this component two very decided periods are evident, the diurnal and the annual. The diurnal maximum occurs between 23<sup>h</sup> and 0<sup>h</sup>, and the minimum at 9<sup>h</sup> or 10<sup>h</sup>; but in May and June (the winter months) it occurs later, viz. at 11<sup>h</sup>. The form of the curve shows a rapid increase and an equally rapid decrease. The annual variation is likewise remarkable for its simplicity, being a perfect curve of sines. See p. 28, plate 2, fig. 2. We shall see further on, that at Bombay (lat. 18° 53' N.) the phases approximate to those of St. Helena, except that the secondary periods are more sensible, though not much more so. From the two extreme cases which we have here considered, we may infer what would happen at an intermediate station, like the Cape of Good Hope; that there would be a diurnal and semi-diurnal period, but the latter considerably less developed than at Hobarton and Toronto. Observation confirms this; and fig. 1, p. 40 of the Cape of Good Hope volume shows it at a glance.

The period, then, of the horizontal force is the resultant of

two periods, the one diurnal, the other semi-diurnal; and the value of the semi-diurnal period is a minimum at the equator, and increases with the geographical latitude. The epochs of the changes depend in this case, too, on the hours at which the sun passes the magnetic meridian, and are somewhat advanced in the summer of the hemisphere and retarded in the winter.

If from the middle latitudes we ascend to the Pole, we shall find that the curve of the bifilar appears to be in advance of that of the declinometer: this peculiarity, which seems to complicate the law which we have enunciated, depends entirely on the coefficients with which the diurnal and semi-diurnal period are alternately affected according to the latitude.

The following are the laws relative to the annual variation of the diurnal means. So far as regards the monthly means, we have already remarked that at St. Helena there is evidently an annual period depending on the sun's declination; and in order to display the effects of the solar declination in the other observatories, it would be necessary to repeat the analysis made for the magnetic declination. But unfortunately, really perfect observations are as yet few, and hardly sufficient for the seasons. As far as regards Hobarton, the march appears to be not very unlike that of the declination. Thus during the summer, the diurnal variation has its greatest extension, and becomes successively less in spring and autumn, and is at its minimum in the winter.

So far as regards the absolute value of this component, it is greater than the annual mean in summer, less in spring and autumn, and a minimum in the winter. It would not be difficult to show, from the nature of this curve, that here, too, is to be found the superposition of the two periods depending on the solar declination and on the horary angle, which are added together in summer, and of which one is subtracted from the other in winter. At Toronto the effects are similar in the respective seasons. At St. Helena the absolute maximum of the horizontal force occurs in the months of February, March and April, and the minimum in August and September.

In these variations it is not easy to separate that which is caused by the temperature from that which is strictly the magnetic period,—partly because the temperature exercises an influence on the bars, and if the variations are not accurately corrected there is a danger of error,—and partly, because as the temperature affects the force of all magnets, it may affect that of the earth also.

Besides, as the variation of this component depends both on that of the inclination and on that of the total force, it is not easy by means of the horizontal observations alone to determine

to which cause each phase must be attributed. A few observations, and those subject to some uncertainty, tend to show that at St. Helena the annual variation of the inclination is small, so that we must consider the variations of the horizontal force as depending entirely on those of the total force.

At Makerstoun the values of the horizontal component have their maxima at the solstices, and their minima a little after the equinoxes\*.

In general in this, as in the declination, the months of April and August are marked by the greatest diurnal excursions; this is attributed by Mr. Broun to the extraordinary perturbations, but it probably depends on some other cause. But for the principal details on the epochs of the maxima and minima, we must refer to the original works, this memoir being already too long.

### B. Vertical Component.

This is given by the balance magnetometer arranged at right angles to the magnetic meridian. It follows laws analogous to those of the horizontal force. At St. Helena the curve has a simple period, but with this difference from the curve shown by the bifilar, that the curve of the horizontal force is nearly a curve of cosines (reckoning from noon), and that of the vertical force a curve of sines. But the rudiments of a secondary period are seen in a slight undulation, which it makes at about 10<sup>h</sup>, as also in the intervals of the intersection of the curve with the axis being greater from midnight to noon than from noon to midnight. The maximum occurs between 5<sup>h</sup> and 6<sup>h</sup>, and the minimum at 20<sup>h</sup>. Its amount is smaller from October to March, during which period it is retarded, and greater from April to September, when it is in advance.

At the Cape of Good Hope the march of the vertical force is so similar to that of the declination, that it is susceptible of the same analyses and gives the same results. In these curves the diurnal period always predominates, though somewhat modified in parts. See Sabine, *Obs. Cape*, p. 40, pl. 5. fig. 2.

At Toronto and at Hobarton the usual antagonism displays itself in this component. The double period is developed as much as in the declination, and the hours of minima and maxima are almost identical with those of the declination. The double period displays itself at Hobarton in a somewhat singular manner, producing a maximum towards seven in the evening.

But to follow out all these irregularities would be very tedious; we will therefore abstain, referring the reader to the works already quoted. We will only add, that the explanation of many ano-

\* Makerstoun Observations, 1846, p. 32, where also it is mentioned in a note that this is confirmed at Munich by Dr. Lamont.

malies is to be found in the following remarks, viz. that as the origin of the periods which are superposed depends on the different hours at which the sun passes the magnetic meridian, the semi-diurnal period has necessarily various positions with regard to the diurnal; and in this manner various inflections occur which it would be difficult to account for in any other way. For instance, the variations of the inclination given at the Cape of Good Hope, p. 44, pl. 6, can easily be decomposed by an experienced eye into the usual principal periods, diurnal and semi-diurnal, which gain alternately on each other with varying parameters in the different months.

We may therefore finish this discussion with the following general conclusion: that "the horizontal component, as well as the vertical, may be decomposed into a diurnal and semi-diurnal period, which depend on the declination of the sun and on the geographical latitude."

### C. Inclination and Total Force.

Given the laws of the variation of the two preceding components, that of the resultant or total force may easily be deduced; and the absolute magnetic inclination being known, the variation of the inclination may also be deduced from the variations of the horizontal and vertical force.

*General Law.*—"The phases of the inclination are analogous to those of the declination, but three hours earlier."

*Explanation.*—If the maximum of the declination is at 2<sup>h</sup>, the maximum of the inclination would be at 23<sup>h</sup> or thereabouts. This may be seen in the Makerstoun curves, &c., and also in the Hobarton curves, the explanation of which we will give presently.

As for the other peculiarities, it will be sufficient to remark, without analysing each case in detail, that in general the maxima of the horizontal force coincide with the minima of the inclination.

Colonel Sabine calls attention to the analogy that exists between the variation of the inclination at Hobarton and Toronto, places which are almost antipodal. In these localities the variation of the inclination in both its periods is the same at almost precisely the same hours, with this difference only, that at Hobarton the south pole (the lower) is to be considered, and at Toronto the north (the lower).

The total force at Toronto is subject to two periods, viz. the following:—

Principal maximum at . . .	5 <sup>h</sup>
Principal minimum from . . .	15 <sup>h</sup> to 16 <sup>h</sup>
Secondary maximum from . . .	18 <sup>h</sup> to 20 <sup>h</sup>
Secondary minimum from . . .	22 <sup>h</sup> to 23 <sup>h</sup>

According to this distinguished writer, an analogous double period is wanting at Hobarton, and the total force has a simple progression with the minimum at 20<sup>h</sup> or 21<sup>h</sup>, and the maximum between 5<sup>h</sup> and 6<sup>h</sup>, the intermediate march being continued without interruption. But on carefully examining the curves themselves, given by him at p. 68, pl. 3, it will be seen that the simple period exists only in appearance, and that in certain months the secondary period is very obvious, and rudiments of it exist in all, though to a very small extent. This difference is certainly owing to the great difference of latitude and magnetic force between the two places.

There are not yet a sufficiency of published observations at St. Helena to determine this law; but a copious series of observations at the Cape show a period in the variations of the total force almost complementary to that of the declination. The similarity of the two kinds of curves, which generally differ about three hours in their phases, renders a more complete analysis unnecessary.

#### D. *Complex Period of the Needle.*

The process which we have described for determining the motions of the needle consists in a series of decompositions of the forces, rendered necessary from the mode in which the magnetic bars are supported. The laws of the variations of the components being determined, we may deduce from them what would be the motion of a needle, not on an axis, but suspended by a *single point*, which would be its centre of gravity, and free to obey every magnetic variation in whatever direction it took place. To give an idea of the combined motions which the needle makes in a complete oscillation, we may refer to two figures in particular in Sabine's Plate, Hobarton Observations, vol. i. pl. 3, the first of which belongs to December at Hobarton, the other to June at the same place. The principle upon which these figures are traced is the following:—The point where the two axes intersect represents the mean diurnal position of the needle in declination as well as in inclination. Along the horizontal axis a distance is taken representing the variation of the declination for a given hour, and from the point so obtained, an ordinate is erected representing on the same scale the variation of the inclination for that hour. Thus are obtained the figures to which we have referred.

In Colonel Sabine's Plates, he has given the curves for each month of the year, and they are all extremely instructive, but the two to which we have particularly referred, the march of the curve in the two extreme months of the year, is well shown. It may be seen from these, and still better from the whole series,

that the oscillation of the needle has always a double period, diurnal and nocturnal, but their respective lengths vary with the season. The diurnal period, considerable in summer, is contracted in winter, and the nocturnal period, short and hardly discernible in summer, is greatly developed in winter; in this will be seen the fact, elsewhere noticed, that the absolute nocturnal minimum is greater than the diurnal minimum, and hence we see the cause of the error of those who consider that there is a single period in winter. But two things are particularly to be observed in these curves. 1st. The nocturnal loop is always in diametrical opposition to the point of noon; from this it appears that the phases succeed each other near the lower meridian with the same march as near the upper.

2nd. That the magnitude of each loop in the opposite seasons, diurnal as well as nocturnal, is in a constant proportion to the one diametrically opposed to it, viz. between  $\frac{1}{7}$ th and  $\frac{1}{6}$ th; thus, for example, the loop of the diurnal curve for December becoming the nocturnal loop in June, is diminished to about  $\frac{1}{6}$ th. In like manner the diurnal loop of June, when it becomes the nocturnal loop in December, is diminished to about  $\frac{1}{6}$ th. This constant proportion, which is observed in all the months, must not be overlooked; and physically considered, it must depend on the manner in which the influence of the solar magnetism operates across the earth.

3rd. The appearance of these curves is that which would arise from the superposition of two circular spirals with different moduli, the one having a simple, the other a double period. The curves which are seen in Wheatstone's undulation machine, when two spirals are superposed, the one half the length of the other (in which case the projection of the resultant at right angles to the axis of the spirals forms a kind of *c*), are evidently of the same kind as the present.

Mr. Broun has given analogous curves for Makerstoun, and a glance at these, as in the case of Hobarton, will show the same law, though somewhat more complicated from having grouped too many months together, and from the higher latitude and more frequent disturbances.

Among the points to be remarked in these curves is the following:—"Tracing in them the direction of the magnetic meridian (that is to say, noting the hour at which the sun passes it), it is seen that the greatest velocity of the needle occurs when the sun passes through this plane, and that the centre of the nocturnal loop is to be found in the same line, or very near to it, and that the movements of the needle in inclination are complementary (but with a distance of  $3^h$ ) to those of the declination."

From these facts we conclude that "a perfectly free needle would describe during the day a species of double spiral produced by a compound circular motion having two periods, the one diurnal, and the other semi-diurnal; or of two periods, the one while the sun is above the horizon, and the other while it is below; the excursions of which are in the proportion that the diurnal arcs bear to the nocturnal, and have for principal axis the local magnetic meridian."

Lastly, there is a fundamental characteristic of all the principal elementary periods, which consists in the *maximum and minimum being about 6 hours distant from one another.*

#### E. Total Force.

Colonel Sabine has investigated whether the maxima and minima of the total force vary during the year; from the discussion of the observations at Hobarton and Toronto he has arrived at the conclusion that the total force has its maximum in the months of December and January in both hemispheres, although these correspond to opposite seasons. Such a law corresponds too nearly with the change of the distance of the earth from the sun to admit of our doubting that it depends upon it. We have then that "the disturbing force of the sun increases as its distance from us diminishes, and does not depend on the temperature of the seasons."

The exact determinations of this force hitherto obtained are too few to enable us to get out the rigorous expression of this law, that is, whether it is inversely as the square of the distance; but the fact appears to be established, and will perhaps be brought out by the discussion of other observations, particularly if care be taken to eliminate from them the periodical changes which depend on the seasons, and the secular changes.

#### PART III.

In this part we will briefly discuss the different hypotheses which have been proposed to account for the diurnal magnetic period, and we will take this opportunity of referring to the extraordinary variations.

Justice must be done to the enlightened spirit of modern physicists, who, intent on the study of facts and their laws, care little to construct hypotheses; from this it arises, that whatever has been proposed has been rather by way of conjecture than with any real endeavour to establish a theory. We too, in the same spirit, and merely for the purpose if possible of combining facts, have supposed the sun to act as a great magnet. The explanations hitherto proposed may be reduced either to thermo-electric currents induced by the sun in the different strata of the earth,

or to the electricity developed in the meteorological changes of which the sun is the principal cause. A single reflection seems to exclude these from being *principal* causes of the magnetic diurnal period. The characteristic fact, as we have already noticed, is that the magnetic elements have a double period, diurnal and nocturnal.

Now temperature and the other causes suggested have a simple period, with greater or less modifications it is true, but not so constantly repeated when the sun is below the horizon as the magnetic period is in all climates and in all seasons. This appears to us a law peculiarly characteristic of magnetism, which shows it to be essentially distinct in cause, and to have a separate origin from meteorological phenomena; in the same way that the semi-diurnal lunar period in the tides is a proof of a special action of our satellite on the waters of the ocean, which is universal gravitation. And just as the fact that the tide is more or less retarded after the moon passes the meridian, is not a sufficient objection to destroy belief in this cause, so some irregularity of a like nature observed in the magnetic period will not be sufficient to disprove the reality of the magnetic action, if we believe the proofs adduced to be otherwise sufficient. We remarked at the beginning of this work, that Arago's observations, well discussed, ought to lead to the same conclusions as those of other observers; and now that we have, at last, before us an abstract of these observations, it appears that we were not mistaken, and that they afford a striking proof of the existence of the double period\*.

What has led some to consider the magnetic period as a simple one, has been, seeing that in certain seasons the extreme minima occur at night. The error arises from not distinguishing the absolute from the relative maxima; but these are the true characteristics of the phenomenon, and ought to be looked upon as decisive in the matter.

To this proof in support of the solar magnetic theory, may be added another, already noticed by Col. Sabine, and worked out by us in § 1 of Part II. of this Memoir, viz. the opposite action of the sun according to its declination, the inversion occurring exactly at the epoch of the equinoxes; and here another difference will be seen between the effects of thermal and me-

\* We received the fourth volume of Arago's works, the first of his scientific works, after the present treatise was finished: we see with pleasure that our theories are not contradicted by the observations or the deductions of this celebrated astronomer and physicist, so that, without our knowing it, our opinions are almost coincident with his. We shall see presently how his observations furnish an additional proof of a decennial period in the magnetic changes which we expected to find there. For the double period, see pp. 499, 540.



teological causes, and the magnetic effect of the sun. The former do not reach their extremes for a considerable time after the corresponding astronomical phases, while the latter have an almost exact coincidence with them.

We do not pretend, however, that there are not considerable difficulties in the way of this hypothesis; and although it explains very well certain very singular facts,—as, for example, the interval of six hours between the diurnal maxima and minima, a fact the explanation of which has never, as far as I am aware, been even attempted on any other hypothesis, and which yet is so marked in all the magnetic variations in the mean latitudes; also the singular exception which it suffers at the equator, becoming simple for the horizontal and for the vertical components, and various other points,—yet we must confess that there are some irregularities which our formulæ do not explain. Of this nature is the fact, that at St. Helena, and generally under the equator, the period for the declination of the needle appears to be rather eight hours than twelve, so that it presents sometimes three maxima. Without repeating here what we have said elsewhere in general terms, viz. that these periods may find their explanation in those terms of the formulæ which we have neglected\*, we may say that this fact may simply depend on the configuration and nature of the ground near to the places of observation. Thus, for example, at St. Helena, an island situated in the midst of the Atlantic, and entirely volcanic, the distribution of magnetism must be very different from what it is at a place in the interior of a continent; and we know, in fact, that the isogon lines rapidly change their direction in passing from seas to continents, and this explanation may also apply to equatorial stations near the coast. We know too how much the vicinity of magnetic bodies may influence the diurnal variation of the needle †. This explanation seems to be confirmed by the fact that the diurnal curves derived from the declination at St. Helena resemble much more closely those of other countries than the annual diurnal mean. In fact, the latter is exclusively due to the horary angle, and therefore more strictly dependent on the distribution of the earth's magnetism round the place of

\* We have in every case worked out these formulæ completely, and there have resulted terms which, developed in series, give sines and cosines of triple arcs; from this we may deduce the explanation of a period of the third of a day. But as this development may be made in every periodical function, it does not give a legitimate proof of the truth of the explanation, until we have demonstrated theoretically that these terms may have coefficients of considerable magnitude, a point which I have not yet investigated.

† See the volume of Arago already cited, where he relates the experiments of Barlow on this subject, p. 492.

observation. It may also be said that the small maxima in the morning and evening, which are in truth for the most part only indicated, are only a portion of the regular period cut short midway by the discontinuity introduced by the passage of the sun from above to below the horizon, as we have elsewhere remarked in regard to the periods observed towards evening in high latitudes\*. For these reasons we have urged that a complete explanation of the phenomenon depends on the law of the distribution of magnetism on the globe.

It may not be useless to state here what is habitually observed at Bombay, that being a place situated to the north of the equator, and in a latitude not very dissimilar in amount to St. Helena, the lat. being  $18^{\circ} 53' 30''$  N. From the observations made at this place and reduced by Mr. Montriou, an oscillation results analogous to that of other countries; having an eastern maximum a little before 8<sup>h</sup>, and a minimum between noon and 1 P.M. Besides which, there are two other small oscillations, one near sunrise, the other near sunset; it is evident that this is the nocturnal period interrupted by the interposition of the earth. During the night the needle has a very small oscillation. The horizontal force has a simple period, but with disturbances which indicate the commencement of a secondary period; the vertical force shows a tendency to a like period. When the observations have been continued for a greater number of years, more certain results will be obtained. See Obs. Magn. and Meteor. at the Obs. of Bombay for the year 1847, part 1. p. 493, and Plate 1. The Editor of the Bombay Observations then concludes:—"The presence of the sun seems to produce great magnetic variations in the day-time, and it is otherwise manifest that it is not on account of the heat only of that body; for if this was the case, the curves of the temperature would be similar to the magnetic curves. Besides, the presence of the sun begins to be felt two hours before sunrise, and lasts almost as long after sunset, so that the solar magnetic influence appears to be quite independent of the temperature of the place."

But we are very far from denying that meteorological causes may often affect the needle; we know that every meteorological change is accompanied by a change more or less marked in the vapour of the atmosphere, and therefore by a development of electricity. But when the needle is usually seen to complete its regular oscillation tranquilly in the midst of the most violent storms, and during tempests loaded with electricity, with tremendous thunder and lightning, it may well be asked, *What are the conditions under which electricity must develop itself in*

\* This appears to us to be the true explanation of the phenomenon.

order to affect the needle? That such an action does exist, however, appears to be proved by the fact, that in our climates the needle performs its oscillation with the greatest regularity during calm and serene days, and that on change of weather this regularity is invariably disturbed. We have, in proof of this, a year's observations at Rome: and it would be well to discuss magnetic observations more from the meteorological point of view than has hitherto been done. From the few observations which we have made, it appears that light and passing overcloudings have more effect on the needle than tempests themselves. We have already remarked elsewhere, that at Rome the perturbations exhibit themselves in that particular state of the atmosphere in which there are slightly phosphorescent clouds having at night the appearance of the rudiments of the aurora borealis. This fact was observed by us a second time on the evening of the 27th of July. We were making some observations on stars in the meridian, when towards half-past nine we were interrupted by a slight overclouding coming from the north; a very rare occurrence, since generally with us the sky begins to overcast in the south-west. While we were waiting for it to clear, the cloud appeared slightly luminous at the edges, so that there seemed to be a diffusion of the milky way in unwonted parts of the heavens. Soon after this it cleared, and the observations were continued; but almost immediately the same overclouding recommenced with the same luminous appearance. I then remembered the fact observed before, that a similar state of the atmosphere had been accompanied by magnetic perturbations, and on going to look at the magnetometer I found it more than 20 divisions (about  $7\frac{1}{2}$ ) out of its usual position, and the regular observation made at 9.35 had been marked by the observer as being an extraordinary one for that hour. This was the more striking from the needle having performed its diurnal oscillation with the greatest regularity during the whole of the preceding season. This was, without doubt, a phænomenon of the kind which accompanies the aurora borealis. But it may be asked, was the condensation of vapours the cause, or the effect, of the perturbation? It is generally considered to be most probable that the perturbation is the effect; but is this certain? M. De la Rive has proposed, in his Memoir on the Aurora Borealis\*, a theory which accounts with some felicity for the effects of atmospheric electricity on the needle; but it may be doubted whether this cause is sufficient to explain all the facts to which the author would apply it. An accurate study of the laws to which the extraordinary perturbations of the needle are subject, combined with the study of the aurora borealis, can alone throw light on this question.

\* *Bibl. Univ. Archiv. des Sciences Naturelles*, vol. xxiv. p. 337.

All that we know with certainty on this subject is, like so much besides, due to Colonel Sabine. He has collected the principal results at which he has arrived from the discussion of the Hobarton and Toronto Observations in a memoir inserted in the Philosophical Transactions (March 1852), from which we will give a short extract, as well to complete the exposition of the laws of the magnetic changes, as to obtain some light for guidance in future researches.

A comparison of the Toronto and Hobarton observations establishes, that even the extraordinary perturbations, though occurring at all hours of the day, yet when taken in a mass, have a regular period, which depends on the local time, and have opposite directions in the opposite hemispheres; so that the perturbations which cause an easterly deviation at Toronto, cause a westerly deviation at Hobarton, in conformity with the complete magnetic antagonism at the two stations. This fact is elicited without difficulty from the coincidence of the perturbations observed at the two places in the same day, with a difference in local time corresponding to their difference in longitude. The general result is, that easterly perturbations at Toronto and westerly perturbations at Hobarton, have their minimum in number and magnitude during the day and their maximum during the night. This maximum occurs at Hobarton between 10<sup>h</sup> and 11<sup>h</sup>, and at Toronto at 9<sup>h</sup>. This difference in time, as we have already remarked, occurs in all the other magnetic changes. The minimum occurs at Hobarton between 5 and 6 A.M., and at Toronto between 2 and 3 P.M. The easterly perturbations at Hobarton and the westerly at Toronto, have a distinct period. Their maximum at Toronto is at 5 A.M., and at Hobarton at 6 A.M.; the minimum at Toronto is between 9 and 10 P.M., and at Hobarton at 10 P.M. Taking the perturbations in mass, and laying down the curve representing their mean effect on the curve of the diurnal oscillation of the needle, the following law is elicited:—"The morning perturbations tend to diminish the ordinary excursion of the local period, and the evening ones to augment it."

This law may be enunciated in another way. "The pole which is turned to the sun is by the mean effect of the perturbations moved towards the east from 5 A.M. to 5 P.M. About 6 A.M. and 6 P.M. it passes zero, and the rest of the day is moved to the west. The maximum movement in the morning is at about 7, and in the evening at about 9. In both places a secondary minimum towards the west is observed at noon." In other respects the curves are tolerably regular and of the usual form; but at Hobarton the principal maximum and minimum are less marked than at Toronto; the march of the two curves is in direct opposition at the two stations. These conclusions

agree with those of Mr. Broun at Makerstoun, as may be seen in the results for 1846, p. 87, plate I. The following are the results with respect to the frequency and magnitude of the perturbations in the different months of the year:—"The mean value of a perturbation is a maximum in the equinoctial months, less in the winter months, and a minimum in the summer months." At Hobarton the difference between the summer and the equinoctial months is scarcely perceptible. The proportions of the frequency and of the magnitude of the perturbations in each month, relatively to the sum of those observed in a year, come out a minimum in the winter months, a maximum in the equinoctial months, and intermediate in the summer months. These conclusions, however, may be somewhat varied by the use of different systems of reduction, depending chiefly on the definition of an extraordinary perturbation. This is not the case in the laws of the perturbations taken with reference to the day, because in that case all the methods of reduction bring out the same result. Thus the same result is seen in the curves given by Mr. Broun, whose method of reduction is different from that used by Colonel Sabine.

But a most singular fact, which has been discovered in these researches, is the start which the mean annual values of the perturbations take between the years 1845 and 1846, when they are almost doubled. This being a fact of the greatest importance, Colonel Sabine has endeavoured to place it beyond doubt by the best possible proofs. The following is the table given by him (p. 115):—

Year.	Ratios of the number of perturbations.	Ratios of the aggregate values.
1843	0·60	0·52
1844	0·78	0·78
1845	0·72	0·65
1846	1·20	1·15
1847	1·28	1·42
1848	1·43	1·52

The circumstance of the last three years having ratios almost double of the first three, appears not to be accidental, particularly when we observe that in the two observatories, which are almost antipodal, the same fact appears, and that during all the six years the same instruments were employed. Besides, in the same years the observed diurnal excursions of the declination, of the inclination, and of the total force, have sensibly increased beyond the limit of any probable error. Lastly, the same fact, in regard to the diurnal excursion of the declination, results from the observations of Dr. Lamont at Munich in Bavaria. Further observations may throw much light on this

subject. For the present we can only say with Colonel Sabine, that so general a change in the march of all the magnetic elements demands a proportionate cause; and that as these do not arise from the ordinary effects of the climate, which in these years have exhibited no extraordinary change, it is necessary to seek some other cause.

Colonel Sabine also points out the singular coincidence between the years of the maximum and minimum of these magnetic changes and those of the maximum and minimum number of the solar spots, observed by Schwabe in these years. This number was a minimum in 1833 and 1843, and a maximum in 1828, 1837 and 1848. From these and from other observations, Wolf has deduced a decennial period in the changes of these spots; and it remains to investigate whether a like march can be traced in the older magnetic observations. The publication of Arago's observations has come opportunely for this comparison. From the table at p. 500-501, vol. i. of his *Scientific Works*, the declination-needle appears to have had a minimum excursion in 1823 and 1824. Before that it was greater, and having reached this minimum, it increased continuously until it arrived at a maximum in 1829. These epochs correspond with those derived from the period observed in the solar spots, which was a maximum in 1828 and a minimum in 1823. From the Göttingen observations, we find a maximum in the excursions of the declination-needle in 1836-37. This maximum also coincides with a maximum of the solar spots in Schwabe's table\*. Hence Colonel Sabine thinks it not impossible that changes in the solar atmosphere may extend their influence to the earth in the form of magnetic action.

The truth is, that to consider the whole complexity of magnetic perturbations as a mere meteorological effect, appears to be assigning to them a cause not adequate to the effect. The fact mentioned above, that the maxima of the perturbations at Hobarton succeed each other with the same retardation as the other magnetic phases, is one which cannot be explained either by the retardation of the effect of temperatures, or by the condensation of vapour. We cannot conceive how these should account for the general retardation of one hour. It is then a purely magnetic fact, the explanation of which depends on that of the physical cause of solar and terrestrial magnetism. The same may be said of the greater perturbations at the epochs of the equinoxes, which certainly bear no relation to the state of the atmosphere or to the solar heat. Colonel Sabine makes the

\* Results of the Magnetic Observations made by Gauss at Göttingen, *vide* Taylor's *Scientific Memoirs*, vol. ii. part 1. art. 2. p. 57; see Humboldt's *Cosmos*, vol. iii. p. 292, Sabine's translation.

acute observation, that the coincidence of the solar spots with the maximum of the perturbations demands a cosmical cause, depending on that body\*.

We may be permitted to refer here to the hypothesis of Mairan on the solar atmosphere, and on its relation to the zodiacal light and the aurora borealis, and therefore to the magnetic perturbations. We are far from admitting the theory as proved, since it appears impossible to admit that the solar atmosphere extends so far as half the radius of the orbit of Mercury †, whence it is rather to be inferred that the zodiacal light depends on a nebulous ring circulating round the sun between Venus and the Earth.

But whatever hypothesis be adopted, there are various coincidences which may be deserving of regard. Mairan had, even in his day, remarked the greater frequency of the aurora borealis at the equinoxes ‡, the epochs at which the zodiacal light is most visible. Neither had the relation between the greater frequency of the aurora borealis and the epochs of the greater solar spots escaped him, a relation already remarked by Cassini §.

The paucity of observations at that time permitted suggestions to be made which have been since proved to be groundless; but in general such coincidences are worthy of consideration. Modern observations of the eclipses of the sun, of the protuberances and of the corona, as also of the spots, of the temperature of various parts of the disc, as well as photographic impressions, have placed beyond doubt the existence of the solar atmosphere even beyond the zodiacal light ||. In read-

\* It may be said, that as the spots are apertures in the atmosphere of the sun, they give freer passage to the magnetic action of the solar nucleus.

† See Mairan's Works, p. 199, in which for the months of the year the following numbers are given relative to the frequency of the aurora, beginning with January,—21, 27, 22, 12, 1, 5, 7, 9, 34, 70, 26, 15. *Mairan sur l'Aurore Boreale, suite des Mémoires de l'Ac. des Sciences*, 1731.

‡ *Vide* Humboldt, vol. iii. p. 413, Sabine's edition.

§ *Vide* Humboldt's *Cosmos*, vol. iii. *et seq.*, Sabine's edition.

|| In the past winter I have been engaged in taking the solar disc on plates of glass, in order thus to confirm the discovery made in 1852 of the difference of temperature in different parts of the disc. The impressions obtained were stronger in the centre than at the edges; but I have abstained from publication, because I do not consider this proof as very decisive. In fact, the image cannot have an equal intensity in every part of the field of the telescope, on account of the obliquity of the different pencils of rays which compose it. It is otherwise in the thermo-electric experiments, where the pile remains invariable in regard to the axis of the lens, and therefore this obliquity hardly exists, or is equal for all points of the disc from which rays fall upon it. I think it right to say this, because I find photographic experiments recently brought forward as proofs of the smaller intensity of the light at the edges than at the centre, and I think it probable that they may be subject to the exceptions which I found in my own.

ing Mairan, one cannot help seeing the serious difficulty which he finds in explaining why the auroras have their maximum at the equinoxes, and not at the epochs at which the earth passes through the nodes of the solar atmosphere; but do we truly know the place of the nodes of the zodiacal light? He assumes that they are the same as those of the solar equator, but this is not proved; and if the zodiacal light constitutes a ring, it might well be otherwise. On the magnetic hypothesis, the greater frequency of the aurora at the epochs of the equinoxes would have relation to the position of the poles of the sun with reference to the earth, these poles being in fact more directed towards the earth at the equinoxes, and being more or less oblique to it at other times.

Those who hold the theory of the production of electricity by vapours, may say that these become rarefied in the morning and condensed in the evening; and hence may arise opposite electric states, the fluid passing in the morning from the earth to the atmosphere, and in the evening from the atmosphere to the earth. This may be true; but why should this condensation always take place at nine in the evening? The hygrometric curves of the different months show at all events a variation in the hour of maximum according to the seasons.

A hypothesis, however, can be found which would conciliate the various facts, viz. that atmospheric changes may generate electricity, but that the direction of the current, which of itself would be indeterminate, may be determined by the magnetic action of the sun. But to expand this further into a hypothesis would be at present premature. We will only say that it is not improbable that the earth is subject to the magnetic action of the sun in a manner unknown to us; but now that magnetic phenomena are developing themselves under so many aspects, we may hope that the explanation of these mysterious actions will soon be found. Not only magnetism, but diamagnetism also may cooperate, and still more the induced currents which exist in bodies of every kind. Two things only I wish to notice. First, the value assigned by Gauss to the magnetism of a cubic metre of the earth\*, is such as to make one believe that the whole mass of the earth is really magnetic, and that this force results not only from ferruginous substances, but from the whole globe itself. He proves, in fact, that the eighth part of a cubic metre of the earth has a magnetic moment equal to that which is possessed by a bar of steel 1 lb. in weight and 30 centims. in length, magnetized to saturation. He justly observes, that such a result must surprise physicists, and that it would require 8464

\* General Theory of Terrestrial Magnetism, Taylor's Scientific Memoirs, vol. ii. part 6. art. 5. p. 225. No. 31.



trillions of such bars to represent in space the magnetic force of the earth! The other is, that magnetism may act upon bodies in a manner quite surprising, and of which we are very far from forming an idea before seeing its effects\*. The marvellous experiment performed with Ruhmkorff's apparatus, in which a cube of brass, 2 centims. in the side, rotating with the greatest rapidity is struck motionless, if I may use the expression, by an invisible force at the moment of the completion of the circuit of the great electrical magnet between the poles of which it is situated, and without being drawn to one side or the other, remains there fixed, in spite of the powerful torsion of the wire which tends to cause it to rotate,—and resumes its rapid motion when the current ceases,—proves that non-magnetic bodies in motion may, under the influence of a magnet, give rise to phenomena of the most mysterious nature. An action of this kind must take place between the earth and the sun, and thus perhaps may be explained some of those anomalies which still present no small difficulties to every theory which is proposed.

The object of this memoir having been to coordinate the laws of the diurnal motion of the needle, we leave to another opportunity the discussion of its secular variations, as well as of the lunar action, which appears to Colonel Sabine to have been placed beyond doubt; and perhaps the time is not distant when the diurnal magnetic variations will have been subjected to laws depending only on the most simple relation of distance and angle between two magnets.

#### *Conclusion.*

The matters discussed at length in this memoir may be summed up in the following propositions:—

1. The action of the sun upon the needle is opposite, according as the sun is north or south of the equator.

2. The action of the sun on the declination-needle has a period, in part but not entirely, analogous to that of the temperature and of the annual and diurnal meteorological changes.

\* I owe it to the kindness of R. P. Palladini, Professor in the College at Naples, that I was a witness of this magnificent experiment, performed with the apparatus with which he has provided the physical cabinet of that college. In truth, one cannot but be surprised at seeing a body rotating with such velocity, stop at the instant at which the circuit is completed, as if it had encountered an invisible obstacle. The torsion given to the thread was sufficient to make the cube rotate with such velocity as to appear a cylinder, and yet on completing the circuit it stopped instantly, and when the current was sufficiently strong, it stopped without the angles being directed to the poles of the magnet (as is usual in the case of weak currents). The explanation of this fact is involved in the phenomena of the magnetism of rotation, but I wished to adduce it as an instance of how a force already known may act in certain circumstances in a new and astonishing manner.

3. The periods of the horizontal and vertical components, following the law of the geographical latitude, and occurring at hours wholly different from the variations of temperature, show a different origin from these. Therefore, if the coincidence in time, in the variations of the temperature and declination, have contributed to the belief of the existence between these two of a mutual relation of cause and effect, the study of the other components makes this coincidence disappear, and therefore destroys every foundation of the hypothesis.

4. All the phænomena hitherto known of the diurnal magnetic variations may be explained by supposing that the sun acts upon the earth as a very powerful magnet at a great distance.

LXII. On *Table Spar* from the *Morne Mountains*.

By Dr. M. FORSTER HEDDLE\*.

HAVING by my analysis of the Edinburgh Castle Rock *pectolite* taken *table spar* out of the list of British minerals, I have more than usual pleasure in reintroducing it from the *Morne Mountains*.

Two analyses were made; the first on a specimen sent me by Mr. Greg, the second on one obtained from an Irish dealer, with the following results:—

	On 25 grs.	On 30 grs.
Silica . . . .	51·360	50·434
Oxide of iron . .	·980	·840
Lime . . . .	42·500	43·920
Magnesia . . .	·484	·396
Water . . . .	1·480	1·360
Carbonic acid . .	not det.	2·371
		99·321

The powdered mineral absorbs ·142 per cent. of moisture, and before the blowpipe gives slight and but momentary evidences of the presence of soda.

Both of the above specimens are contaminated by an admixture of carbonate of lime, doubtless derived from the matrix, which is limestone altered by heat.

The *table spar* from this locality bears much resemblance to the *pectolite* found on the Ayrshire coast, consisting of a vein of diverging and interlacing white fibres.

Seeing that *table spar* has been formed by the fusion of lime and silica in a crucible, it is probable that the mineral may have been formed in a similar manner, the granitic heat inducing the combination of a siliceous vein with its calcareous matrix.

\* Communicated by the Author.

LXIII. *On the Benzole Series.*—Part II. *Note on some Derivatives of Xylole.* By ARTHUR H. CHURCH, Esq.\*

WITH xylole, the third member of the benzole series, we have at present but slight acquaintance. Cahours, to whom we owe the discovery of this hydrocarbon, has, I believe, not yet published the details of his investigation. Having had occasion to prepare pure xylole in order to make a determination of its boiling-point†, I imagined that a study of some of its derivatives might prove of interest. The small quantity of pure xylole having a constant boiling-point of  $126^{\circ}\cdot 2$  at my disposal, induced me to employ for those experiments, an account of which I propose to give in the commencement of the present paper, the whole of the crude xylole boiling between  $124^{\circ}$  and  $130^{\circ}$  obtained from two gallons of light coal-naphtha. In following the directions of Cahours for the preparation of this hydrocarbon, I did not obtain from 16 oz. of the oil from wood naphtha more than 1 oz., boiling at from  $126^{\circ}$  to  $128^{\circ}$ .

1. *Preparation of Nitroxylole and Xylidine.*

Crude xylole was dissolved in nitric acid of spec. grav. 1·5, the acid being kept cool; the nitro-compound formed was then separated by the addition of water, and washed until free from nitric acid. Nitroxylole thus obtained is a yellow oily liquid, heavier than water; its odour is less pleasant than that of nitrobenzole; only when impure does it decompose on keeping. By the well-known process of reduction the nitroxylole was converted into xylidine; this base was then purified by repeated crystallization of its combination with oxalic acid; afterwards the xylidine was separated from this salt by distilling it with lime. The xylidine was dissolved in hydrochloric acid, and a considerable quantity of bichloride of platinum solution added. At first a slight precipitate took place; this was redissolved by the addition of a small quantity of water; the whole was then set aside under a bell-jar with oil of vitriol. By fractional crystallizations of the platinum salt I hoped to obtain a xylidine compound, from which, by distillation with caustic soda or potassa, pure xylidine might be procured; for we know in many cases how excellent a method of separation that of fractional crystallization is, and special inquiries by Anderson, C. G. Williams, and others have proved the advantage of this method for the purification of the platinum salts of a series of bases isomeric with that to which xylidine belongs. After twelve hours, the

\* Communicated by the Author.

† Philosophical Magazine, April 1855.

solution to which bichloride of platinum had been added, had deposited a crop of crystals (I.).

This first crop of crystals consisted of slender needles.

I. .7225 grm. gave .2088 grm. platinum :—

Per cent. platinum.	Theory (cumidine).
28.89	28.92

The second crop, of needles similar to the first crop.

II. .5502 grm. gave .16 grm. platinum :—

Per cent. platinum.	Theory (cumidine).
29.08	28.92

The third crop, stellar groups of short yellow needles.

III. .325 grm. gave .098 grm. platinum :—

Per cent. platinum.	Theory (xylidine).
30.15	30.16

The fourth crop, similar to the third.

IV. .3254 grm. gave .099 grm. platinum :—

Per cent. platinum.	Theory (xylidine).
30.39	30.16

The fifth and sixth crops, of short, flattened, orange prisms, gave respectively 30.2 and 30.33 per cent. of platinum. The seventh crop, fine golden-yellow scales.

VII. .2015 grm. gave .0632 grm. platinum :—

Per cent. platinum.	Theory (toluidine).
31.36	31.51

Preliminary examinations with small quantities of a solution similar to that from which were obtained the crops of crystals described above, gave me some idea as to the proper time for the separation of each successive crop. Crops I. and II. (cumidine platinum salt) weighed together less than 2 grms., while the united weight of crops III., IV., V., and VI. (xylidine platinum salt) was not less than 25 grms. Of crop VII. not more than .3 of a gramme was obtained.

22 grms. of xylidine platinum salt were introduced into a Florence flask with an excess of caustic soda, the neck of the flask being bent and connected with a condenser. By distillation a large quantity of a nearly colourless oil was obtained in the receiver. This oil was xylidine,  $C^{16}H^{11}N$ .

Xylidine rapidly attracts oxygen from the air, acquiring a reddish-violet colour and gradually resinifying. It turns dahlia paper green, and slightly restores to reddened litmus paper its original blue tint; the oxalate and sulphate, which were the

only salts I prepared, have an acid reaction. Xylidine boils, according to my determination, at  $213^{\circ}$  to  $214^{\circ}$ \*. The sulphate of xylidine is difficultly soluble in cold water, but crystallizes from hot water in long colourless needles.

## 2. Preparation of Nitro-sulphoxylic and Sulphoxylic Acids.

One portion of the pure xylole boiling at  $126\cdot2$  I treated with fuming nitric acid, &c., and dissolved the nitroxylole thus obtained in fuming sulphuric acid: after three days (the liquid having been heated for one hour at  $100^{\circ}$  C.) the solution was diluted with water, saturated with carbonate of barium and filtered. The filtrate, evaporated nearly to dryness on the water-bath, yielded a lemon-yellow crystalline powder, nitro-sulphoxylate of barium; in quantity, however, too small to admit of an attempt to isolate the acid, or to form from this barium compound other salts by double decomposition. On ignition and treatment of the residue with nitric and sulphuric acids†,—

·42 grm. of the barium salt gave ·1607 grm. of sulphate of barium, corresponding to 22·5 per cent. of barium; calculation as  $C^{16}H^8NO^4Ba2SO^3$  requires 22·94 per cent.

The remainder of the pure hydrocarbon at my disposal (2·5 grms.) was placed in contact with four volumes of Nordhausen sulphuric acid, and set aside for a week: at the expiration of this time, radiating tufts of needles, long and perfectly colourless, filled the xylole, still partly undissolved by the sulphuric acid; these crystals were collected, placed over sulphuric acid, and enclosed in a glass tube which was afterwards sealed; they were pure sulphoxylic acid. This substance has a strongly acid reaction; at first its taste is sour, then bitter; it crystallizes well from xylole, is exceedingly soluble in water and in sulphuric acid, deliquesces rapidly in the air, may be fused *in vacuo* or in xylole vapour without decomposition, but if heated above its fusing-point, acquires a dark colour: the fused acid crystallizes on cooling in fine prisms. A barium salt prepared from this acid gave me 26·93 per cent. of barium; theory as  $C^{16}H^9Ba2SO^3$

\* I had intended to make a combustion of this base, but was prevented by an accident which occurred to the apparatus in which I was heating the xylidine in order to determine a second time its boiling-point. I retained, however, a small portion which had distilled twice at a little below  $213^{\circ}$ , and combined it with sulphuric acid. ·3 grm. of this sulphate gave by precipitation with chloride of barium ·2047 grm. of sulphate of barium, corresponding to 28·11 per cent. of  $SO^1$ ; the formula  $C^{16}H^{12}NSO^4$  requires 28·23 per cent.

† The same process was adopted in all the other determinations of barium given in the present paper.

requires 27·02. The sulphuric acid used in the preparation of this sulpho-acid had acquired a reddish-yellow tint, and gave after neutralization with carbonate of barium, filtration and evaporation, a second portion of sulphonylate of barium. Of this salt 5001 grm. gave 2287 grm. of sulphate of barium, corresponding to 27·01 per cent. of barium; calculation as  $C^{16}H^9Ba_2SO^3$  requires 27·02 per cent.

Sulphonylate of barium crystallizes in pearly scales similar to those of sulphotoluolate of barium; if to its hot concentrated aqueous solution cold water be added, I have occasionally found the salt separate in beautiful iridescent plates. The aqueous solution is not altered by ebullition.

I append the analytical results of those experiments relating to pure toluole (boiling at 103·7), spoken of in my former note on the benzole series. The nitro-sulphotoluolic and sulphotoluolic acids were obtained in the same manner as the corresponding terms of the xylole series. The determinations of sulphate of barium were made by ignition of the barium compounds with the precautions previously mentioned.

#### I. Analysis of toluole.

·4024 grm. of oil gave ·3151 grm. of water and 1·3450 grm. of carbonic acid.

These numbers correspond to the following per-centage composition:—

	Experiment.	Theory ( $C^{14}H^6$ ).
Carbon . . .	91·15	91·3
Hydrogen . . .	8·7	8·7

#### II. Determination of barium in nitro-sulphotoluolate of barium.

·3022 grm. of salt gave ·1289 grm. of sulphate of barium, corresponding to 25 per cent. of barium; theory as  $C^{14}H^6NO^4Ba_2SO^3$  requires 24·41 per cent.\*

#### III. Determination of barium in sulphotoluolate of barium.

·705 grm. of salt gave ·349 grm. of sulphate of barium, corresponding to 29·14 per cent. of barium; theory as  $C^{14}H^7Ba_2SO^3$  requires 29·08 per cent.

May 1855.

\* The excess of barium given by experiment over that required by theory arose from a slight admixture of nitrate of barium with the original salt.

LXIV. *On the Existence of an Electrical Æther through Space.*  
By GEORGE JAMES KNOX, Esq.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

IN the year 1840 you were kind enough to insert in your Magazine a paper, entitled "On the Direction and Mode of Propagation of the Electric Force traversing Interposed Media," in which memoir I endeavoured to prove, from the experiments of Sir H. Davy, that an electric current consists in alternate states of induction and equilibrium of the particles of the medium conveying the current, the *intensity* of the current being proportional to the *rapidity of change* of induction and equilibrium, and consequently that the *mass* of oscillating æther surrounding the particles represents the *quantity*, while the *rapidity* of the oscillations represents the intensity of an electric current.

Your March Number contains some very interesting experiments which were made by Mr. Latimer Clark, on the transmission of currents of electricity of varying intensity through 768 miles of gutta-percha wire, indicating a velocity of propagation of about 1000 miles in a second, which velocity is sensibly *uniform* for *all intensities from 31 cells to 500*; which results, Dr. Faraday remarks, "afford a fine argument in favour of the opinion of those who suppose the electric current to be analogous to the vibrations of air under the action of sonorous bodies."

The experiments of Professor Grove on the electro-chemical polarity of gases (vol. iv. p. 513), where he obtains rings alternately bright and oxidated, showing effects of oxidation and reduction by the same current on the same plate, he considers as "analogous to the phænomena of interference in light; though doubtless, if this be a right view, the very different modes of action of light and electricity would present very numerous phænomenal distinctions."

The idea has lately been presented to my mind, that the oscillations of the electrical æther in combination with the particles of the medium conveying a current, produce undulations, not only in the æthers of light and heat, but also in another æther, which Dr. Draper calls the tithonic æther, which, if experiment proves to be the case, should be more correctly termed the electrical æther.

Dr. Draper, in the year 1847, undertook a series of experiments upon the rays of light emitted by incandescent bodies, from which he concluded that when a platinum wire is heated by the voltaic pile or otherwise, it emits rays of light, which increase in refrangibility proportionally to the increase of heat,

which he explains thus :—“As the luminous effects are undoubtedly owing to a vibratory movement executed by the molecules of the platinum, it seems from the foregoing considerations to follow, that the frequency of those vibrations increases with the temperature.”

Sir David Brewster has observed, that in the spectra produced by the electric light the chemical rays are more numerous than in those produced by the lime light.

The problem then to be solved is,—whether the chemical rays be produced *directly* by the oscillations of the electrical æther in the platinum wire, or *indirectly* by the heat produced.

This question might be resolved by observing the effect produced by voltaic piles of *different* intensities, the *heat* remaining *constant*; and if so, it would afford a strong argument in favour, not only of an oscillatory movement in the electrical æther in combination with the particles of bodies, but also of the existence of such an æther through space.

I have the honour to be, Gentlemen,

Your obedient Servant,

GEORGE JAMES KNOX.

2 Finchley New Road, St. John's Wood,

May 3, 1855.

LXV. *On a New Compound of Gold and Mercury.*

By T. H. HENRY, Esq., F.R.S.\*

**W**HEN gold is treated with mercury in large excess, a definite compound is formed which remains dissolved in the mercury, from which, however, it often separates in a crystalline form, and from which it may be almost entirely separated by mechanical means, such as pressure through chamois-leather. This solid amalgam crystallizes in four-sided prisms, and contains six parts of gold to one of mercury, and fuses on elevating the temperature (Gmelin, vol. iii.). The mercury, however, which has passed through the chamois-leather always contains gold, in proportion varying from a minute trace to 10 grs. in the pound. In the metallurgical processes for extracting gold, it becomes important to estimate the amount of gold remaining in the fluid part of the mercury, and it was during some experiments made with the view of ascertaining the best method of doing so that this new amalgam was discovered.

This substance is best obtained by dissolving gold in mercury in the proportion of 1 part of gold to 1000 of mercury, about 7 grs. to the lb. avoirdupois, squeezing the solution through

\* Communicated by the Author.



chamois-leather, and dissolving the mercury in dilute nitric acid with gentle heat. The compound is left in the form of four-sided prisms of the most brilliant metallic lustre, which may be boiled in nitric acid without decomposition, and exposed to the atmosphere for months without becoming tarnished. On exposure to heat they do not fuse, but afford a sublimate of metallic mercury, amounting in my experiments to rather less than 12 per cent.; the form of the crystals remained unaltered, their lustre was little affected, and the residue consisted of pure gold. This would correspond to a compound of four atoms of gold to one of mercury:—

Au . . .	197 × 4 =	788	or	88·74
Hg . . .	100	100	...	11·26
		888		100·00

### LXVI. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 398.]

Feb. 1, 1855.—Colonel Sabine, Treas. and V.P., in the Chair.

**T**HE following communication was read:—

“Micro-chemical Researches on the Digestion of Starch and Amylaceous Foods.” By Philip Burnard Ayres, M.D. Lond.

After some general historical remarks on the methods hitherto employed in the investigation of the complicated phenomena of the process of digestion, the comparatively small results obtained by chemical analysis of the contents of the stomach, intestinal canal, and of the evacuations, by Tiedemann and Gmelin, Berzelius, and others, the author proceeded to demonstrate the necessity of a minute examination of the contents of the alimentary canal by the microscope, and such chemical tests as we possess for the determination of the changes of such articles of food as exhibit definite structure.

In order that we may ultimately arrive at a complete exposition of the phenomena of digestion, he is of opinion that it will be necessary to examine,—first, the structure of particular kinds of food, then the changes produced in them by cooking, and lastly to trace the changes they undergo at short intervals, through the alimentary canal from the stomach to the rectum. The results of a series of researches of this character on the changes in starch, and starch-containing foods, are presented in this memoir.

The method adopted for the examination of the changes in starch and starch-foods was as follows:—An animal was kept fasting twenty-four hours, and afterwards confined to a diet consisting of the starch or amylaceous food, with water, for five or six days, until the debris of all other kinds of food previously taken were cleared from the alimentary canal. At a determinate time, after a meal, the animal was killed, the abdomen laid open as quickly as possible,

and ligatures placed at short intervals on the intestinal canal, from the pylorus to the rectum. The contents of the stomach and each portion of the intestinal canal included between the ligatures were then carefully examined. This mode of examination sufficed to determine the changes which occur in the food during normal digestion; but other questions as to the particular secretion or secretions by which the changes observed were effected.

The fluids poured into the alimentary canal are five in number,—the saliva, gastric juice, bile, pancreatic juice, and finally, the intestinal mucus.

The influence of the saliva is easily determined, by chewing the particular food subjected to experiment, and keeping the mixture at about 98° Fahr. The combined action of the saliva and gastric juice is seen in the contents of the stomach. To determine the action of the bile, the common bile-duct was tied, and to ascertain the action of the intestinal mucus, it was necessary to ligature the bile and pancreatic ducts. If the digestion of the substance is not effected in the stomach, it is evident that it cannot be attributed to the saliva or gastric juice; if the digestion is still effected in the intestinal canal after ligature of the bile-duct, it cannot be attributed to the action of the saliva, gastric juice or bile; if it still go on after ligature of the bile and pancreatic ducts, the digestive power must of necessity be referred to the action of the intestinal mucus, provided no change has previously taken place in the stomach; but if the food passes unchanged after cutting off the supply of bile and pancreatic juice, but proceeds after ligature of the bile-duct alone, the act of digestion must be referred to the pancreatic juice.

The author first briefly describes the structure of the starches and starch-containing vegetables employed in his experiments; then the changes produced by cooking, and finally enters on a minute description of the changes observed in the experiments he performed on normal digestion, and after cutting off the supply of bile and pancreatic juice.

The correct appreciation of the structure of the starch-granule is of considerable importance in relation to these investigations, and the author believes that he has been able to afford a satisfactory solution of this vexed question. The changes observed during the digestion of starch favour the original opinion of Leuwenhoeck, that the starch-granule consists essentially of an investing membrane or cell-wall, enclosing an amorphous matter, the true starch, which strikes an intense blue colour with iodine; and these changes also support the opinion of Professor Quekett, that the concentric circles seen on the starch-granules of many plants are simple foldings of the investing membrane, leaving it still doubtful, however, whether these concentric circles are not in the starches of some plants composed of linear series of dotted elevations or depressions of the investing membrane.

By these experiments it was determined that the concentric circles remain after the whole of the starch matter, colourable by iodine, was removed, and that even then the characteristic cross and colours

were still seen when the granules were viewed by polarized light, although more feebly than before; this result being probably due to the lessened power of refracting light, after the removal of the starch matter.

After describing the structure of the wheat-grain and flour, the changes occurring in the wheat-starch during the manufacture of bread are given in detail; but the most interesting of the changes produced by cooking are those seen in the boiled or roasted potato and in the boiled pea.

In each of these the act of cooking effects two purposes:—it causes great enlargement and physical change of the starch-granules, and dissolves the intimate adhesion of the starch-cells, which afterwards appear as ovoid or globular, slightly adherent bodies distended by the swollen starch-granules, the outlines of which are indicated by more or less irregular gyrate lines, produced by the mutual compression of the starch-granules within an inelastic cell-membrane.

The starch-granules of the pea possess a much thicker investing membrane than those of the potato, which causes their outlines to remain much more distinct after the removal of the true starch substance during the process of digestion. The other structures seen in the pea are carefully described; the most curious among them being the cells composing the external layer of the testa, which bear so strong a resemblance to columnar epithelium of the intestine, that they might be mistaken for the latter by an inattentive observer.

The substances submitted to experiment were,—1, boiled wheat-starch; 2, wheaten bread; 3, uncooked *tous les mois*; 4, boiled *tous les mois*; 5, boiled potato; 6, uncooked peas; 7, boiled peas; 8, boiled peas after ligature of the bile-duct; 9, boiled potatoes after ligature of the bile and pancreatic ducts. Several subsidiary experiments were made to determine the action of the intestinal mucus, the saliva, and the substance of the pancreas, on starch.

The conclusions at which the author arrives from the experiments are,—

1. That the starch-granule is composed of two parts, chemically and histologically distinct,—a cell-membrane and homogeneous contents. The markings seen on many varieties of starch are referred to folds or markings of the investing membrane.

2. No perceptible change occurs in the starch, whether raw or cooked, during its sojourn in the stomach of quadrupeds or the *ventriculus succenturiatus* and gizzard of birds; all the granules preserve their perfect reaction with iodine and their pristine appearance.

3. The conversion of boiled starch into dextrine and glucose is chiefly effected in the first few inches of the small intestine, but it continues to take place in a less degree throughout the entire intestinal canal.

4. In the digestion of boiled wheat or other starch, or of wheaten bread, the bulk of the mass rapidly diminishes in its passage through

the small and large intestines, so that it ultimately yields only a small quantity of faecal matter. After being deprived of their contents, the membranes of the granules shrink and shrivel up into a minute granular matter, which constitutes the chief bulk of the faecal evacuations after an exclusive diet of starch food.

5. The digestion of raw starch food (peas) in the pigeon or other granivorous birds goes on much more slowly, and progresses pretty equally throughout the entire intestinal canal. The starch-granules, whether free or included in cells, become intersected by radiating or irregular lines or fissures, more or less opaque or granular; they also gradually lose their characteristic reaction with iodine; and this important change, commencing at the surface, progresses towards the centre, until the whole of the starch matter is removed, leaving the starch-membranes often apparently whole, retaining their characteristic markings. The fissured and granular condition of the starch-granules is not due to their trituration in the gizzard, but to the action of the intestinal fluids, since it was often seen in granules enclosed in and protected by perfect starch-cells. In the digestion of raw starch food, a considerable quantity always escapes change, for many starch-cells and granules in the faeces perfectly retain the characteristic reaction with iodine.

6. As the starch remains unchanged in the stomach, its conversion into glucose cannot be attributed to the saliva or gastric juice, unless we suppose these fluids to remain inactive in the stomach, and suddenly to regain their activity in the first part of the small intestine. The author found that the saliva was capable of effecting the conversion of starch into glucose, but that the mixture of saliva and gastric juice in the stomach did not possess that property even after being rendered alkaline by carbonate of soda. It is probable that the converting power of the saliva, as it flows from the mouth, depends not on the true saliva, but on the buccal mucus; for Magendie found that saliva taken from the parotid duct was wholly inactive, while the mixed saliva from the mouth effected the conversion with great facility. Unless, then, the sublingual and submaxillary glands secrete a different fluid from the parotids, it is evident that the activity of the saliva must be attributed to the buccal mucus.

7. The difference between the digestion of boiled and raw starch in dogs is seen in the experiments on the digestion of boiled wheat-starch, boiled *tous les mois*, and bread. In all these, some starch-granules escape the action of heat and water, and remain in nearly their pristine condition. These uncooked starch-granules undergo slow and imperfect changes, being fissured, broken, and more or less altered, but in general retaining their characteristic reaction with iodine.

8. The conversion of starch into glucose is not effected by the bile, since after ligature of the common bile-duct, the changes occur to as great an extent as when the bile passes freely into the intestinal canal.

9. It is not due to the pancreatic juice, inasmuch as after ligature

of the bile and pancreatic ducts in the same animal, the digestion of starch is still effected.

10. The only remaining secretion is the intestinal mucus, which is especially abundant at the upper part of the intestinal canal; and a further proof is afforded of the activity of the intestinal mucus taken from the upper part of the duodenum above the entrance of the pancreatic duct after ligature of this duct and the common bile-duct, by its capability of converting a large quantity of fresh boiled starch into glucose out of the body.

11. In the cooking of starch-containing vegetables, such as potatoes and peas, the adhesion of the starch-cells is dissolved or weakened, so as to render them easily separable and amenable to the action of the intestinal fluids. At the same time the starch-granules undergo a large increase in bulk, distend the cells, and by their mutual compression, their outlines present the appearance of gyrate lines beneath the cell-wall. The cells seldom burst so as to emit their contents, or present any appreciable opening through which the intestinal fluids can directly penetrate. The author cannot positively affirm so much of the starch-membranes, because these are so extremely delicate that fissures might be invisible, but he believes that in a great number the membranes remain entire.

12. If this be the case, the conversion of starch matter into glucose must be effected by the permeation or endosmose of the intestinal fluids through the invisible pores of two membranes, in the digestion of the pea, the potato, and other similar foods, and the glucose must escape through the same membranes by exosmose.

13. Before the conversion of starch into glucose, the amylaceous matter contained in the starch is more dense than the intestinal mucus in immediate contact with the cells, and an inward current or endosmose is established; but after that conversion, the syrupy fluid is less dense than the mucus, and then an outward current or exosmose occurs, by which the glucose escapes from the cells into the intestine and is absorbed. If this be the case, as the details of the experiments tend strongly to prove, a new and important function is assigned to the intestinal mucus.

14. In normal digestion, chyme escapes very slowly from the stomach into the duodenum, in small quantities, as it is detached from the alimentary mass by the muscular movements of the stomach, and this gradual propulsion often occupies several hours after a meal. This slow propulsion is evidently intended to expose the comminuted food fully to the action of the intestinal juices, and produce an intimate mixture with them. The comparatively empty condition of the upper part of the small intestine, even during active digestion, is thus fully explained.

15. If the food be too finely divided or incapable of a second solidification in the stomach, it passes too rapidly into the first part of the small intestine, is insufficiently mixed with the intestinal fluids, and a considerable part escapes digestion. On the other hand, if it enters the small intestine in masses incapable of reduction

by the muscular action of the parts or solution in the fluid, it traverses the intestinal canal unchanged, except at the surface, which is then alone exposed to the action of the intestinal fluids.

16. It is not necessary for the conversion of starch into glucose that the fluids in the duodenum or other parts of the intestinal canal should be alkaline, or even neutral, for in several of the experiments the contents of every part of the alimentary canal had an acid reaction.

17. The greater part of the intestinal mucus is not excrementitious, for little, if any, mucus is perceptible in the fæces in normal digestion, except at their surface, whereas the greater proportion of the contents of the small intestine consists of mucus. A considerable quantity of mucus is seen in the cæcum, but it rapidly diminishes in the colon, and is scarcely detectible in the fæces, except that on the surface, which is probably derived from the mucous membrane of the rectum. The author raises the question, whether one of the chief functions of the cæcum is not to effect the conversion of the intestinal mucus into some other substance capable of re-entering the blood, and performing some ulterior purpose in the animal economy.

18. In normal digestion, the separation of the epithelium of the mucous membrane of the intestine is the exception instead of the rule, as stated by some physiologists. The author questions the theory of the detachment of the epithelium of the villi in each act of absorption, on the grounds that the presence of detached epithelium was unfrequent in the whole course of his experiments; that epithelium is readily detached by manipulation; that the continual reproduction of such a vast amount of cell-tissue must necessarily be accompanied by a vast expenditure of vital force; and finally, that it is not necessary, because fluids readily penetrate epithelial membranes.

19. The passage of a given food through the whole length of the intestinal canal may occupy a comparatively short time, especially when the animal is fasting. In one experiment, where a pigeon refused food until the fæces contained no visible debris of previous food, starch-granules were detected in the fæces within two hours after a meal, and this although the intestine of this animal is extremely narrow and about a yard in length.

20. A remarkable circumstance in the digestion of starch or starch foods is the constant presence of myriads of Vibriones in the lower part of the intestinal canal. They are generally first observed in the lower part of the small intestine, as minute brilliant points, just visible with a power of 600 diameters, in active movement. They increase in numbers towards the cæcum, in which a large number of fully-developed vibriones are constantly seen. These minute organisms increase in size and length in the colon and rectum, and their fissiparous mode of propagation, first described by the author in the '*Quarterly Journal of Microscopical Science,*' may be distinctly traced by examining the contents of these portions of the intestine.

February 15, 1855.—Thomas Bell, Esq., V.P., in the Chair.

The following communication was read:—

“An Account of some recent Researches near Cairo, undertaken with the view of throwing light upon the Geological History of the Alluvial Land of Egypt.”—Part First. By Leonard Horner, Esq., F.R.S.S.L. & E., F.G.S.

The author commences by observing, that although it be highly improbable that we can ever form an appropriate estimate in years of the age even of the most modern strata, we are not cut off from all hope of being able to assign an amount in years to the duration of some of the great geological changes which, in past ages, the present surface of the earth has undergone, by causes that are still in operation; especially by a careful study of the formation of the deltas of great rivers, and of the action of the latter on the rocks and soils they traverse in their course. If in a country in which a certain alteration in the land has occurred, we know that such alteration has taken place in part within historical time, and if the entire change under consideration presents throughout a tolerable uniformity of character, we may be justified in holding the portion that has taken place within the historical period to afford a measure of the time occupied in the production of the antecedent part of the same change.

Egypt supplies us with the earliest evidence of the existence of the human race recorded in works of art; in its monuments we find the dawn of the historical period and of civilization; and that land alone, of all parts of the world as yet known to us, offers an instance of a great geological change that has been in progress throughout the whole of the historical period, in its annual inundations and the sediment these deposit to form the alluvial land in the valley of the Nile; and there is good reason for believing that the change had been going on with the same uniformity for ages prior to that period when our reckoning of historical time begins. To investigate the formation of the alluvial land in the valley of the Nile in Upper and Lower Egypt is therefore an object of the highest interest to the geologist and the historian.

The author being impressed with the conviction that this geological problem could only be solved by having shafts and borings made in the alluvial soil to the greatest practicable depths, determined to have some such experiments made; as the results might lead the way to other researches on a greater scale. The ground on which he hoped to be able to form a chronometric scale by which the total depth of sediment reached might be measured, was the same as that on which the French engineers in 1800 had proceeded, viz. the accumulation of Nile sediment around monuments of a known age. If that depth of sediment be divided by the number of centuries that have elapsed since the date of the erection of the monument, we obtain a scale of the secular increase of which the base of the monument is zero, assuming that the average increase from century to century has been uniform within an area of some extent.

Then if the excavation be continued below the base stone, and the sediment passed through exhibits similar characters as to composition with that above the base line of the monument, it would be fair to apply the same graduation below the zero-point of the scale as above it, and, if we reached so far, we should be able to estimate the time that has elapsed since the first layer of sediment was deposited on the rock forming the channel over which the water spread when it first flowed northward from its sources in the interior of Africa, subject however to correction for causes that might make a difference in the rate of increase between the earlier and later periods.

The author submitted his scheme to the President and Council of the Royal Society, who encouraged his proceeding by acceding to his request of a grant from the Donation Fund at their disposal, towards the expenses of the researches.

The author introduces his subject by a sketch of the physical geography and geology of Egypt, a description of the annual inundations, and of the sediment deposited from the water of the Nile.

Egypt is separated from Nubia by a low hilly region, about fifty miles broad from north to south, chiefly composed of granitic rocks, but associated with two kinds of sandstone, the one belonging to the cretaceous series, the other of the newer tertiary age. The valley of Upper Egypt is bounded by two ranges of hills running northward, the Arabian range on the right, the Libyan on the left of the river, both alike composed of sandstones and limestone. The cretaceous sandstone extends from the granitic rocks forming the first cataract at Assouan for about eighty-five miles, where it is covered by a limestone which has the characters of the upper chalk of Europe. This chalk continues on both sides of the valley for about 130 miles, when it is covered in its turn by a tertiary nummulite limestone, and of which the further prolongation northward of both ranges is composed; this nummulite limestone can be well studied in the extensive quarries of Gebel Mokattam above Cairo.

The author briefly describes the operations of the private association of English, French, and Austrian Engineers in 1846-47, commonly called the French Brigade, for the purpose of determining the disputed question of the relative levels of the Red Sea and Mediterranean. The French engineers, at the beginning of the present century, had come to the conclusion that the Red Sea was about 30 feet above the Mediterranean, but the observations of Mr. Robert Stephenson, the English engineer, at Suez, of M. Negretti, the Austrian, at Tineh near the ancient Pelusium, and the levellings of Messrs. Talabot, Bourdaloue and their assistants, between the two seas, have proved that the low-water mark of ordinary tides at Suez and Tineh is very nearly on the same level, the difference being, that at Suez it is rather more than one inch lower.

At the island of Philæ, about five miles above Assouan, may properly be placed the first entrance of the Nile into Egypt. It is here about two miles broad, but is soon after divided into several branches by the rocks that rise up in its bed to form the rapids, commonly called the First Cataract, which have a descent of about 85 feet in a



distance of five miles. Here the river is contracted to about a third of a mile. Assouan is about 300 feet above Cairo, and the distance between the two places being 556 miles, the average fall of the river is little more than half a foot in a mile, 0·54, and Assouan being 365 feet above the Mediterranean, and 696 miles distant from it, the average fall of the Nile from the foot of the First Cataract to the sea is 0·525 in a mile. Low Nile at Cairo is 43 feet  $6\frac{1}{2}$  inches (as measured by the French Brigade in 1847) above low-water mark in the Mediterranean, and the distance being 149 miles, the average fall is little more than  $3\frac{1}{2}$  inches in a mile. The author cites a report of Mr. Rennie to the British Association in 1834, showing that the fall of the Thames, between Chertsey and Teddington Lock, is nearly  $17\frac{1}{2}$  inches in a mile.

The commencement of the annual inundation is about the summer solstice. The rise is scarcely perceptible for six or eight days, it then becomes more rapid, and about the middle of August has usually reached one-half of the greatest amount; it attains its maximum towards the end of September, remains pretty stationary for about fourteen days, and then begins to fall, at first at a more rapid rate than that with which it rose, but after it has fallen one-half the decrease is very gradual, and it goes on sinking until the end of May. The rise continues about 90 days, the falling lasts 250. In 1846, Mougel Bey, the French engineer of the Barrage near Cairo, found the maximum rise 7·20 metres, or 30 feet 10 inches.

When the inundations commence, the Nile is of a reddish colour, and is loaded with sand and mud. From the fall between the Second Cataract at Wadi Halfa and the First, a distance of 214 miles, being not more on an average than 9 inches in a mile, very little coarse gravel can be transported by the river into Egypt. The greater portion of the heavier detritus falls down in the higher parts of Upper Egypt, and from the very gentle slope of the Delta, only a small amount of the solid matter suspended in the water can reach the sea; still, however, the sea has been observed to be turbid at a distance of forty miles from the mouths of the Nile.

The author then proceeds to describe the recent researches. His first and indispensable step was to procure the aid of a competent person to conduct his projected operations, and he was fortunate in obtaining the active and intelligent aid of an Armenian gentleman, educated and long resident in England, Hekekyan Bey, a civil engineer, who had occupied some important positions in the service of the Viceroy Mehemet Ali, especially as chief of the Polytechnic School in Cairo. But nothing could be done without the consent of the then Viceroy Abbas Pacha, more especially as the spot the author had selected for his first operations was in a garden of the Pacha. By the active intervention of Her Majesty's Consul-General in Egypt, the Honourable Charles Augustus Murray, not only was the Viceroy's consent obtained, but his Highness was pleased to direct his ministers to place at the disposal of Hekekyan Bey what-

ever was necessary to conduct the operations in the most complete manner; and, with truly royal munificence, ordered that the whole expense of them should be defrayed by his Treasury. The author never contemplated having the means of making these researches on more than a very limited scale, but he had now the prospect, and it has since been realized, of their being conducted on a very enlarged plan.

It has been often a subject of regret that experiments of this nature, of which the French had set an example half a century ago, had not been followed up. On this subject the author remarks, that the operations are of a nature that scarcely any individual traveller could undertake; for they require a large body of men, some practised in the art of surveying, and as they can only be carried on continuously after the inundation waters have subsided for some time, and therefore at a season of the year when the heat is excessive, those only inured to the climate could stand the work.

The place selected for commencing the operations was at the Obelisk of Heliopolis, about six miles below Cairo, the oldest known; erected, according to Lepsius, 2300 years before Christ. The author having given Hekekyan Bey full and minute directions as to the manner in which the researches were to be carried on, the observations to be made, the plans and reports to be drawn up, and the specimens of soils sunk through to be selected, the operations commenced in June 1851. Sixty men were employed under the direction of Hekekyan Bey, assisted by an officer of Artillery, and some young engineers from the Polytechnic School in Cairo.

Nine pits or excavations were sunk at different distances around the obelisk, each down to the level of the filtration water from the Nile at that season, and as much under the surface of that water as was practicable. The most important of these was one close to the obelisk. The upper surface of the pedestal on which the obelisk rests, was reached at the depth of 5 feet 6 inches below the surface of the ground, Nile mud being accumulated to that height; the pedestal was 6 feet 10 inches in height, and it was found resting on two limestone flags, the upper 16 inches, the lower 15 inches in thickness, and this foundation was laid upon pure quartzose sand. This last was penetrated to the depth of 3 feet  $2\frac{1}{2}$  inches below the lower layer of limestone.

The author gives a section and description of each of the nine excavations. But before doing so, he states that he obtained twenty-eight specimens of soils sunk through in different parts of the Nile Valley, eleven of which were carefully analysed at the Royal College of Chemistry, under the superintendence of Dr. Hofmann. A collection of specimens, duplicates of which are in the possession of Hekekyan Bey, serve as a standard for the description in his reports of the soils passed through, to avoid the necessity of sending specimens of identical alluvia. These samples were carefully compared with the specimens analysed, and were found to resemble them closely in external characters.

The results of the analyses are given by the author, and the average of eight specimens of Nile mud gives the following composition in 100 parts:—

Silica.....	54·585
Sesquioxide of iron .....	20·215
Sesquioxide of alumina .....	6·418
Alumina .....	5·237
Carbonate of lime .....	3·717
Sulphate of lime .....	0·245
Lime .....	1·912
Magnesia .....	0·762
Potassa .....	0·473
Soda .....	0·553
Organic matter.....	5·701
	<hr/>
	99·818

In order to ascertain the amount of solid matter held in suspension in the water of the Nile near Cairo, the author described to Dr. Abbott, a resident in that city, the method he had followed in 1832 to determine the amount of solid matter suspended in the water of the Rhine at Bonn, and requested him to undertake the experiment on the same plan, which he did, and the result gave 110·6 grains in an imperial gallon. The residuum sent was analysed at the Royal College of Chemistry, and yielded very nearly the same result as to composition as the above average analysis of the Nile sediment.

On examining the descriptions of the soils sunk through in the nine excavations at Heliopolis, it appears that they consist of two principal kinds, viz. earths and sands. The earths vary in colour, but are so nearly allied, passing by such insensible shades into each other, and having so great a resemblance to the modern Nile sediment, that they may all be classed as Nile mud. The sands are almost entirely pure quartz, similar to those of the adjoining deserts.

In the same horizontal plane, even in this limited space of half a square mile, there is a very considerable difference in the nature of the soil, and in none of the excavations was there an instance of lamination in the deposit.

When it is considered how small is the amount of sediment left annually by the inundations in any one place, it is very difficult to conceive, in the author's opinion, how there should be in any one spot so great a thickness as  $12\frac{1}{2}$  feet of one kind of sediment, as is the case in one of the excavations, without any lamination or other sign of successive deposition, and still more inconceivable that in pits within a very short distance of each other different kinds of soil should be found at the same levels. Other causes than the tranquil deposit from inundation water must have been at work in the formation of this portion of the alluvial land. The layers of sand were most likely blown across the valley from the desert.

The author deems it advisable to abstain from general remarks,

and from all inferences as to the secular increase of the alluvial deposits, until he has had an opportunity of laying before the Society an account of the far more extensive researches made in the district of Memphis in 1852, and during the last year in a series of pits sunk in a line across the valley of the Nile, extending from the Libyan to the Arabian Chain, in the parallel of Heliopolis.

In the various excavations that have been made in the prosecution of this inquiry, many objects of art of historical interest have been discovered; but as these do not come within the province of the Royal Society, the author proposes to give an account of them in a memoir to be laid before another learned body.

March 1, 1855.—Charles Wheatstone, Esq., V.P., in the Chair.

The following communication was read:—

“On the Structure, Functions, and Homology of the Manducatory Organs in the Class Rotifera.” By Philip Henry Gosse, A.L.S.

In this paper the author institutes an examination of the manducatory organs in the class Rotifera, in order to show that the various forms which they assume can all be reduced to a common type. He further proposes to inquire what are the real homologues of these organs in the other classes of animals, and what light we can gather, from their structure, on the question of the zoological rank of the Rotifera.

After an investigation of the bibliography of the class from Ehrenberg to the present time, in which the vagueness and inexactitude of our knowledge of these organs is shown, the author takes up, one by one, the various phases which they assume throughout the whole class; commencing with *Brachionus*, in which they appear in the highest state of development. Their form in this genus is therefore taken as the standard of comparison.

The hemispherical bulb, which is so conspicuous in *B. amphicerus*, lying across the breast, and containing organs which work vigorously against each other, has long been recognized as an organ of manducation: it has been called the gizzard; but the author proposes to distinguish it by the term *mastax*. It is a trilobate muscular sac, with walls varying much in thickness, receiving at the anterior extremity the *buccal funnel*, and on the dorsal side giving exit to the *oesophagus*.

Within this sac are placed two geniculate organs (the *mallei*), and a third on which they work (the *incus*). Each *malleus* consists of two parts (the *manubrium* and the *uncus*), united by a hinge-joint. The *manubrium* is a piece of irregular form, consisting of *carinæ* of solid matter, enclosing three areas, which are filled with a more membranous substance. The *uncus* consists of several slender pieces, more or less parallel, arranged like the teeth of a comb, or like the fingers of a hand.

The *incus* consists of two *rami*, which are articulated by a common base to the extremity of a thin rod (the *fulcrum*), in such a way that they can open and close by proper muscles. The fingers of each *uncus* rest upon the corresponding *ramus*, to which they are

attached by an elastic ligament. The *mallei* are moved to and fro by distinct muscles, which the author describes in detail; and by the action of these they approach and recede alternately; the rami opening and shutting simultaneously, with a movement derived partly from the action of the *mallei*, and partly from their own proper muscles.

All these organs have great solidity and density; and, from the action of certain menstrea upon them, appear to be of calcareous origin.

The writer proceeds to describe the accessory organs. The ciliated disc has an infundibuliform centre, which commonly merges into a tube before it enters the *mastax*. The particles of food that float in the water, or swimming animalcules, are whirled by the ciliary vortex into this tube; and, being carried into the *mastax*, are lodged upon the *rami*, between the two *unci*. These conjointly work upon the food, which passes on towards the tips of the *rami*, and enter the *oesophagus*, which opens immediately beneath them.

From this normal condition, the author traces the manducatory organs through various modifications, in the genera *Euchlanis*, *Notommata aurita*, *N. clavulata*, *Anuræa*, *N. petromyzon*, *N. lacinulata*, *Furcularia*, *N. gibba*, *Synchæta*, *Polyarthra*, *Diglena*, *Eosphora*, *Albertia*, *F. marina*, *Asplanchna*, *Mastigocerca*, *Monocerca*, and *Scaridium*. Some of these display peculiarities and aberrations highly curious. Notwithstanding the anomalies and variations which occur, however, the same type of structure is seen in all; and the modifications in general may be considered as successive degenerations of the *mallei*, and augmentations of the *incus*.

The form of the manducatory organs, which occurs in *Triarthra*, *Pompholyx*, *Pterodina*, *Æcistes*, *Limnias*, *Melicerta*, *Conochilus*, *Megalotrocha*, *Lacinularia*, and *Tubicolaria*, is next examined. The organs are shown to be essentially the same as in the former type, but somewhat disguised by the excessive dilatation of the *mallei*, and by the soldering of the *unci* and the *rami* together, into two masses, each of which approaches in figure to the quadrant of a sphere.

Attention is then directed to what has been called (but by a misapprehension) the "stirrup-shaped" armature of the genera *Rotifer*, *Philodina*, *Actinurus*, &c. Here, however, the organs are proved to have no essential diversity from the common type; their analogy with those last described being abundantly manifest, though they are still further disguised by the obsolescence of the *manubria*.

*Floscularia* and *Stephanoceros*, the most elegant, but the most aberrant forms of Rotifera, close the series. The *mastax*, in these genera, is wanting; and in the former genus the *incus* and the *manubria* are reduced to extreme evanescence, though the two-fingered *unci* show, in their structure, relative position and action, the true analogy of these organs.

Having thus shown that there is but one model of structure, however modified or disguised, in the manducatory organs of the Rotifera, the author proceeds to the question of their homology. He

argues on several grounds that they have no true affinity with the gastric teeth of the Crustacea, though he states his conviction that the Rotifera belong to the great Arthropodous division of animals.

It is with the Insecta that the author seeks to ally these minute creatures; and, by a course of argument founded on the peculiarities of structure already detailed, he maintains the following identifications:—that the *mastax* is a true *mouth*; that the *mallei* are *mandibles*; the *manubria* possibly representing the *cheeks*, into which they are articulated; that the *rami* of the *incus* are *maxillæ*; and that the *fulcrum* represents the *cardines* soldered together.

While the author maintains the connexion of Rotifera with Insecta, through these organs in their highest development, he suggests their affinity with Polyzoa, by the same organs at the opposite extremity of the scale, since the oval muscular bulbs in *Bowerbankia*, which approach and recede in their action on food, seem to represent the quadriglobular masses of *Limnias* and *Rotifer*, further degenerated.

If this affinity be correctly indicated, the interesting fact is apparent, that the Polyzoa present the point where the two great parallel divisions, Mollusca and Articulata, unite in their course towards the true Polypi.

March 8, 1855.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

The following paper was read:—

“On the Perihelia and Nodes of the Planets.” By Edward J. Cooper, F.R.S.

Prefatory to my volume on Cometic Orbits, published in 1852, I invited the attention of astronomers to the several points of resemblance between the planetary orbits and those of periodic comets. Among these it was shown, that of the heliocentric longitudes of perihelia and ascending nodes of the then known planets and periodic comets, two-thirds were situated in the heliocentric semicircle between  $315^\circ$  and  $135^\circ$ . The planets stood thus in quadrants—

L. P.'s between	$45^\circ$ and $135^\circ$ =	9	}	16.	
	$135$ and $225 = 4$	}			7
	$225$ and $315 = 3$				
	$315$ and $45 =$	}			7
♁ between	$45$ and $135 =$	13	}	14.	
	$135$ and $225 = 4$	}			8
	$225$ and $315 = 4$				
	$315$ and $45 =$	}			1

Here the L. P.'s appeared as 16 to 7, and the ascending nodes as 14 to 8. Two additional asteroids were subsequently discovered leaving the L. P.'s as 16 to 9, and the ascending nodes as 15 to 9.

Again, in 1853, I sent a note upon the same subject to the Royal Astronomical Society of London. At that time a considerable addition had been made to the asteroids, and the total number of planets had risen from 25 to 35. Following the same distribution of the

perihelia and ascending nodes as in my previously published work, the result was—

L. P.'s between	$45^{\circ}$ and $135^{\circ}$ =	13	}	24.	
	$135^{\circ}$ and $225^{\circ}$ = 5	}			11
	$225^{\circ}$ and $315^{\circ}$ = 6				
	$315^{\circ}$ and $45^{\circ}$ =	11			
$\Omega$ between	$45^{\circ}$ and $135^{\circ}$ =	19	}	21.	
	$135^{\circ}$ and $225^{\circ}$ = 9	}			13
	$225^{\circ}$ and $315^{\circ}$ = 4				
	$315^{\circ}$ and $45^{\circ}$ =	2			

The suspicion of some yet undiscovered law became strengthened by this further investigation; and it occurred to me to ascertain if any other heliocentric semicircles would mark the effect of such law more clearly. Let me be permitted to extract the concluding passage from the note as it is printed in the Royal Astronomical Society's Notices:—

“But if, instead of the semicircles  $315^{\circ}$  to  $135^{\circ}$  and  $135^{\circ}$  to  $315^{\circ}$ , we adopt those from  $45^{\circ}$  to  $225^{\circ}$  and  $225^{\circ}$  to  $45^{\circ}$ , we see that of the ascending nodes of thirty-four planets, twenty-eight are found in the first semicircle and only six in the second. Again, the semicircles that contain the greatest number of L. P.'s of planets are between  $0^{\circ}$  and  $180^{\circ}$ , or  $10^{\circ}$  and  $190^{\circ}$ . That which contains the greatest number of nodes is between  $35^{\circ}$  and  $215^{\circ}$ . In the first case there are twenty-six, and in the latter twenty-nine. The quadrant containing the largest number of L. P.'s of planets is that between  $11^{\circ}$  and  $101^{\circ}$ , of which there are sixteen. That containing the largest of nodes is from  $35\frac{1}{2}^{\circ}$  to  $125\frac{1}{2}^{\circ}$ , of which there are twenty.”

At the present moment (January 1855) we have orbits, more or less accurate, of forty-one planets. It cannot be altogether uninteresting to pursue once more the traces of a law still unknown, if it have existence. Our position now stands thus—

L. P.'s between	$45^{\circ}$ and $135^{\circ}$ =	16	}	29.	
	$135^{\circ}$ and $225^{\circ}$ = 6	}			12
	$225^{\circ}$ and $315^{\circ}$ = 6				
	$315^{\circ}$ and $45^{\circ}$ =	13			
$\Omega$ between	$45^{\circ}$ and $135^{\circ}$ =	19	}	24.	
	$135^{\circ}$ and $225^{\circ}$ = 11	}			16
	$225^{\circ}$ and $315^{\circ}$ = 5				
	$315^{\circ}$ and $45^{\circ}$ =	5			

But, be it remembered, that in 1853 of the then known planets the greatest number of L. P.'s were found to be situated in the heliocentric semicircles  $0^{\circ}$  to  $180^{\circ}$  or  $10^{\circ}$  to  $190^{\circ}$ . At present we shall find the perihelia of thirty out of the forty-one planets in either of these semicircles. The greatest number of nodes were then (1853) between  $35^{\circ}$  and  $315^{\circ}$  = 29; and  $45^{\circ}$  and  $225^{\circ}$  = 28. At present, of forty planets there are thirty nodes in either of these heliocentric semicircles. These facts are at least very singular. I may tabulate them—

Of forty-one planets, L. P.'s between $0^{\circ}$ and $180^{\circ}=30$	
	10 and $190^{\circ}=30$
Of forty planets $\oslash$ between	35 and $215^{\circ}=30$
	45 and $225^{\circ}=30$
and between $354^{\circ}$ to $355^{\circ}$ and $174^{\circ}$ to $175^{\circ}=31$	

We here perceive that there are thirty L. P.'s situated in the heliocentric semicircle between  $0^{\circ}$  to  $10^{\circ}$  and  $180^{\circ}$  to  $190^{\circ}$ . It is also the fact, that there are thirty ascending nodes between  $357^{\circ}$  to  $7^{\circ}$  and  $177^{\circ}$  to  $187^{\circ}$ , which may be called the same semicircle as that in which the thirty L. P.'s are found.

The quadrant containing the greatest number of L. P.'s of the forty-one planets, is that between  $10^{\circ}$  and  $100^{\circ}=20$ .

Those containing the greatest number of ascending nodes, are

between $36^{\circ}$ to $43^{\circ}$ and $126^{\circ}$ to $133^{\circ}=20$
and between $62^{\circ}$ to $66^{\circ}$ and $152^{\circ}$ to $156^{\circ}=20$ .

Surely there must be an undiscovered cause determining the orbits in this way. Having laid these facts before my first assistant Mr. Graham, he computed the degree of probability of such a law, arguing thus:—"Were the nodes and perihelia indifferent to all heliocentric longitudes, it would of course be an equal chance in the case of a planet whose orbit had not been determined, in which semicircle either would be found; and the *a priori* probability that, of the forty-one known L. P.'s, thirty would be in one semicircle, is about  $\frac{1}{96}$ ; and that of the forty ascending nodes, thirty-one would be in one semicircle, is about  $\frac{1}{4021}$ . Thus the probability that there is some influence causing a tendency to one semicircle, ascertained from the facts before us, is very strong: for, for the L. P.'s, the odds are about 660 to 1, and for the ascending nodes about 4430 to 1 in favour of such a supposition." But after all it may be an accidental coincidence; as, consistently with the laws of planetary motion, such a congregation of perihelia or nodes may occur at periods exceedingly remote. The further consideration of this subject must be left to analysts, of leisure and inclination to pursue it.

## LXVII. *Intelligence and Miscellaneous Articles.*

### RESEARCHES UPON THE MAGNETIC POWER OF OXYGEN.

BY M. E. BECQUEREL.

**I**N a memoir read on the 21st of May, 1849, at the Academy of Sciences, relating to the action of magnets upon all bodies, I announced that oxygen is a magnetic body capable of being attracted by magnets, and that atmospheric air also possesses this property in consequence of the amount of oxygen contained in it.

The process employed to measure the action exerted by a magnet upon gases, as compared with that produced upon a body which is taken for unity, consisted in placing little sticks of wax, sulphur, glass, charcoal, &c. *in vacuo*, and in different gases, so as to ascertain the magnetic power of these gases from the difference in the effects observed under these two circumstances; it is susceptible of



great precision. Amongst other results obtained, I may refer to the fact that the relation of the attraction exerted by a magnet upon oxygen, to the repulsion which takes place with the same volume of water, is in proportion to the density of the gas, and that it may be represented by 0.18 at a temperature of 53° 6 F.

Since the publication of these researches, Faraday\* has given for the comparative action exerted upon oxygen and water, ascertained by another method, a number similar to mine. M. Matteucci† has also indicated a method by which he has succeeded in arriving at a number which differs but little; but M. Plücker‡ has arrived at different results, by means of a balloon successively filled with oxygen and empty, in contact with the armatures of an electro-magnet, determining the weights necessary for the interruption of this contact. If we reflect that the earth is surrounded by a mass of air equivalent in weight to a stratum of mercury of 76 centimetres in thickness, it is easy to understand that such a mass, subject to incessant variations of temperature and pressure, must influence some of the phenomena dependent upon terrestrial magnetism. Thus, if we calculate the magnetic power of this gaseous mass, we shall find that it is equivalent to an immense iron plate of a little more than one-tenth of a millim. in thickness, which would cover the whole surface of the globe. I have thought, therefore, that it would not be uninteresting to examine afresh the action exerted upon oxygen, air, and gases at different temperatures and at different pressures, so as to determine their specific magnetism for different magnetic intensities.

As in my first researches I have referred all the determinations to distilled water, and have determined by means of my delicate balances the attractions and repulsions effected by a powerful electro-magnet, of rather less power than that formerly employed; I decided upon this method, because M. Plücker, having adopted an analogous process, arrived at numbers differing from those given by me; but I avoided all contact between the bodies submitted to the action of the magnets and the armatures, and I have reduced all the determinations to the same temperature and pressure.

In my previous experiments, I supposed in each case that the intensity of the action exerted upon the substances examined varied as the square of the intensity of the electrical current circulating round the electro-magnet; but in these new researches, the action exerted upon the bodies submitted to experiment being complicated by that exercised upon the envelope, its adjuncts, &c., I preferred the direct determination by experiment of the action exerted upon each body at different magnetic intensities, varying the number of couples employed from ten to sixty, and determining the intensity of the current in each case by means of a tangent galvanometer; it was then possible by a very simple interpolation formula to find the magnetic action exerted upon each substance, and at different determinate and constant intensities.

\* *Bibliothèque Universelle de Genève*, 1853, p. 112.

† *Comptes Rendus*, vol. xxxvi. p. 317.

‡ *Annales de Physique et de Chimie*, vol. xxxiv. p. 342.

The results obtained lead to consequences of which the following are the principal:—

1. With an electro-magnet, of which the iron was 7 centimetres in diameter (instead of 11 centimetres, as in the previous experiments), the repulsive action exerted upon bismuth, water, &c. does not vary in proportion to the square of the intensity of the current which circulates round the electro-magnet, except just at the intensity corresponding with 15 to 20 Bunsen's couples; with a larger number of couples rising to 60, the relation between the action measured by the balance and the square of the intensity diminishes in proportion as this intensity becomes greater. The actions exerted upon different substances at different magnetic intensities lead, with some, to a specific magnetism, which varies with the magnetic intensity.

2. Between the limits of intensity of currents of from 10 to 60 elements, the specific magnetism of oxygen compared with that of water does not vary sensibly  $\frac{1}{20}$ th of its value. It is in the direct ratio of the density of the gas.

3. At 32° F., and a pressure of 0<sup>m</sup>.76, the specific magnetism of oxygen, compared with that of water, is on the average +0.1823.

4. The action exercised by atmospheric air is measured by  $\frac{21}{100}$ ths of the action of oxygen under the same circumstances of temperature and pressure.

5. The following numbers have been obtained to represent the specific magnetism by volume of some solid and gaseous substances at 32° F. and a pressure of 0<sup>m</sup>.76, the intensity of the current being comprised between 30 and 60 Bunsen's couples.

Substances.	Specific magnetisms.	Substances.	Specific magnetisms.
Water .....	-1	Water .....	-1
Oxygen .....	+0.1823	Copper (galvanic deposit)...	-1.41
Deutoxide of nitrogen .....	+0.0498	Copper (pure) .....	-1.68
Air .....	+0.0383	Silver (pure) .....	-2.32
Chlorine .....	-0.0046	Nativegold (massweighing) } .....	-2.41
Ammoniacal gas } according	-0.0020	4813.5) .....	
Sulphurous acid } to aqueous	-0.0005	Gold (pure) .....	-3.47
		Bismuth .....	-22.67

6. This method of experiment, which has enabled me to check that which I employed in my first investigations, but which nevertheless is less delicate than the latter, did not allow me to determine the diminution of the magnetic attraction which oxygen seems to undergo when its temperature is raised (the density remaining equal)\*.—*Comptes Rendus*, April 16, 1854, p. 910.

\* From the abstract of M. E. Becquerel's researches here presented to us, the reader would be led to infer two things:—first, that the differential method of experiment is M. E. Becquerel's method; and secondly, that the discovery of the magnetic properties of oxygen is M. E. Becquerel's discovery. Both of these inferences would be contrary to fact. Nearly four years before M. E. Becquerel made use of the differential method, it was applied with success by Mr. Faraday (see Experimental Researches,

## ON THE ALTERATION OF THE VOLUME OF SOME SUBSTANCES BY HEATING AND FUSION. BY HERMANN KOPP.

Previous investigations have unmistakably shown that there is a certain connexion between the specific gravity and the chemical constitution of bodies. This applies particularly to fluid bodies, as with these the influence of temperature upon the volume of the substance could be easily ascertained. Certain rules also exist with respect to solid bodies, when their specific volumes are compared.

The author now publishes a series of experiments, the object of which was to ascertain the volume of one substance in a solid and fluid state; they also include the determination of the change of volume undergone by some substances when heated or fused. He gives these results as facts which may serve hereafter for the explanation of the question, when similar observations have been made upon a great number of bodies.

The author describes his method of investigation as follows:—For the determination of the expansion of solid bodies which are to be fused in the course of the experiment, a fluid is required the expansion of which is known. The methods of employing such a fluid are two,—namely, 1, *thermometric*, in which the operation is performed in a thermometer-like apparatus, enclosing a solid body together with a fluid of known expansion, in which case the expansion of the solid body is found by deducting that of the fluid from the whole; and 2, *gravimetric*, in which the specific gravity of the body is compared in the fluid and solid states. The author prefers the former method. The fluids employed were *water* for phosphorus, wax, stearine and stearic acid; *sulphuric acid* for sulphur; *olive oil* for chloride of calcium, phosphate of soda, hyposulphite of soda, and the readily fusible metallic alloys; and *oil of turpentine* for ice.

The results of the author's experiments are as follows:—It is rarely the case that a solid body exhibits the same coefficient of expansion when approaching its melting-point that it does when at some distance from it; the coefficient of expansion usually increases rapidly towards the melting-point. Amongst the substances here investigated, it is only with phosphorus, and according to other observers, ice, that the degree of expansion does not increase distinctly towards the melting-point.

The increase of volume exhibited by a melted body when compared with the same body at a lower temperature, arises mostly from the large expansion which the substance undergoes on approaching the melting-point whilst still in the solid state, and from its sudden expansion at the moment of fusion. In some bodies only one of these expansions is of particular importance: thus, for invol. iii. p. 59, &c.); and nearly *two years* before the paper referred to by M. Becquerel was communicated to the Academy of Sciences, Mr. Faraday discovered the magnetic properties of oxygen. (See a letter addressed on this and kindred subjects to Mr. Richard Taylor, *Phil. Mag.* vol. xxxi. p. 401.) In a note bearing date the 28th of November, 1850, Mr. Faraday states his own claims with reference to this subject. (See *Exp. Res.* vol. iii. p. 219.)—ED. PHIL. MAG.

stance, with phosphorus the volume only increases at the moment of fusion, whilst with wax the increase at this moment is but small, but there is great expansion at the temperatures very closely approaching the melting-point.

Phosphorus expands uniformly up to its melting-point ( $111^{\circ}$  F.), when its volume compared with that at  $32^{\circ}$  F. = 1.017; it then, at the moment of fusion, exhibits a sudden augmentation of volume amounting to 3.4 per cent., so that at  $111^{\circ}$  F. its volume is = 1.052.

Rhombic sulphur expands unequally, increasing rapidly in the vicinity of its melting-point ( $239^{\circ}$  F.); if its volume be taken equal 1 at  $32^{\circ}$  F., it is = 1.010 at  $122^{\circ}$  F., at  $212^{\circ}$  F. = 1.037, and at  $239^{\circ}$  F. = 1.096; at the moment of fusion it increases 5 per cent., and then amounts to 1.150.

White wax increases in its expansion very rapidly as it approaches the melting-point ( $147^{\circ}$  F.), but very little (about 0.4 per cent.) at the very moment of fusion; its volume compared with that at  $32^{\circ}$  F. is 1.068 at  $122^{\circ}$  F., 1.128 at  $140^{\circ}$  F., 1.161 at  $147^{\circ}$  F., and becomes 1.166 when fused.

Pure stearic acid expands less than wax before melting, but its volume increases about 11.0 per cent. at the moment of fusion; its volume at  $32^{\circ}$  F. being regarded as unity, it is 1.038 at  $122^{\circ}$  F., 1.054 at  $140^{\circ}$  F., 1.079 at  $158^{\circ}$  F., and becomes by fusion at the latter point 1.198. Impure stearic acid, melting at  $136^{\circ}$  F., expands rather more strongly on approaching its melting-point than pure acids at the same temperature.

Stearine, employed in the first modification, exhibits an increasing expansion up to the temperature at which it passes to the second modification; at this temperature it shows a considerable change of volume (amounting to  $2\frac{1}{4}$  per cent.), but above this the volume increases rapidly in the vicinity of the melting-point, and at the moment of fusion there is an expansion of about 5.0 per cent. Stearine, with a melting-point of  $140^{\circ}$  F., shows a volume of 1.031 at  $122^{\circ}$  F., whilst still in the first modification; on passing to the second it becomes 1.008, and the volume then increases to the melting-point, when it is 1.076, becoming 1.129 when fused.

Water at the moment of freezing expands nearly 10 per cent. in volume. 1.1 volume of ice gave 1 volume of water at  $32^{\circ}$  F., which contracted when heated up to  $39^{\circ}$  F., at which temperature it occupied 0.99988; when heated further, it expands so that its volume at  $212^{\circ}$  F. amounts to 1.043.

Hydrated salts, on the contrary, expand at the moment of fusion. Chloride of calcium,  $\text{Ca Cl} + 6\text{HO}$ , which expands from  $32^{\circ}$  F. to  $68^{\circ}$  F. in the proportion of 1 to 1.007, reaches 1.020 at  $84^{\circ}$  F.; at this temperature it melts, and its volume increases 9.6 per cent., so that in the fluid state it is 1.118. Phosphate of soda,  $2\text{NaO}, \text{HO}, \text{PO}^3 + 24\text{HO}$ , expands from  $32^{\circ}$  to  $68^{\circ}$  F. in the proportion 1 to 1.001; at  $95^{\circ}$  F. it reaches 1.005; at the moment of fusion at this temperature its volume increases 5.1 per cent., so that in the fluid state it is 1.056. Hyposulphite of soda,  $\text{NaO}, \text{S}^2\text{O}^2 + 5\text{HO}$ , expands from  $32^{\circ}$  to  $68^{\circ}$  F. from 1 vol. to 1.002; at  $104^{\circ}$  F. its volume is 1.005; and at  $113^{\circ}$  F., its melting-point, 1.007; at the moment of

melting it increases 5·1 per cent., so that its volume at 113° F. in the fluid state is 1·058.

Rose's fusible alloy (composed of 2 parts of bismuth, 1 of lead, and 1 of tin) expands between 32° and 138° F. in the proportion 1 to 1·0027; when further heated, it contracts, so that its volume at 179°·6 F. is again equal to its volume at 32° F., and at 203° F. amounts only to 0·9947; it then expands again on fusion between 203° and 208° F. by 1·55 per cent., its volume at the latter temperature being 1·0101.—*Annalen der Chemie*, vol. xciii. p. 129.

RECENT APPOINTMENTS.

Prof. Graham's elevation to the post of Master of the Mint left vacant the office of Assayer to that establishment; we understand that the staff has been completed by the nomination of Dr. Hofmann, Chemist to the Museum of Practical Geology, to the vacant office. We have not heard whether Dr. Hofmann proposes to continue his duties in Jermyn Street. While there are so very few state occupations for men of science, it is scarcely desirable that the best of these should be gathered into single hands.—*Athenæum*, May 26.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1855.

*Chiswick*.—April 1. Clear and frosty: overcast. 2. Frosty and foggy: fine: 3. Cloudy: rain. 4. Cloudy and cold: clear and frosty. 5. Slight haze: very fine. 6. Fine. 7. Overcast: very fine. 8. Cloudy and cold: slight rain. 9. Cloudy. 10. Boisterous. 11. Cloudy and boisterous: rain at night. 12. Clear: very fine. 13. Overcast: slight rain: cloudy: fine. 14. Foggy: fine. 15. Overcast. 16. Foggy: cloudless and exceedingly fine. 17. Fine: clear. 18. Fine. 19. Slight haze: fine. 20, 21. Very fine: nights frosty. 22. Cold dry easterly wind. 23. Fine: sharp frost at night. 24. Clear and dry: overcast. 25. Densely overcast: cloudy and cold. 26. Overcast and cold: fine: overcast. 27. Dry haze: fine. 28. Hazy: fine: overcast. 29. Cloudy and cold. 30. Uniformly overcast: cloudy at night.

Mean temperature of the month ..... 46°·08

Mean temperature of April 1854 ..... 47·53

Mean temperature of April for the last twenty-nine years ... 47·17

Average amount of rain in April ..... 1·59 inch.

*Boston*.—April 1. Fine. 2. Cloudy. 3. Cloudy: rain P.M. 4. Cloudy. 5—7. Fine. 8. Cloudy: rain A.M. and P.M. 9. Rain: rain A.M.: stormy. 10. Fine: stormy. 11. Cloudy: rain P.M. 12. Fine. 13. Cloudy. 14. Fine. 15. Cloudy. 16—20. Fine. 21, 22. Cloudy. 23, 24. Fine. 25—30. Cloudy.

*Sandwich Manse, Orkney*.—April 1. Clear A.M. and P.M. 2. Bright A.M.: hazy P.M. 3. Bright A.M.: clear P.M. 4. Clear A.M.: cloudy P.M. 5. Bright A.M.: drops P.M. 6. Rain A.M.: clear, aurora P.M. 7. Showers A.M.: sleet-showers P.M. 8. Snow-showers A.M. and P.M. 9. Rain A.M.: showers P.M. 10. Showers A.M.: rain, clear P.M. 11. Cloudy A.M. and P.M. 12. Rain A.M.: clear, aurora P.M. 13. Bright, solar halo, A.M.: rain, aurora, P.M. 14, 15. Bright A.M.: rain P.M. 16. Sleet-showers A.M.: showers P.M. 17. Cloudy A.M.: cloudy, aurora P.M. 18. Rain A.M.: cloudy P.M. 19. Rain, bright A.M.: showers P.M. 20. Hail-showers A.M.: cloudy P.M. 21. Bright A.M.: clear, fine P.M. 22. Clear A.M.: clear, fine P.M. 23. Bright A.M.: cloudy P.M. 24. Hazy A.M.: cloudy P.M. 25. Damp A.M.: cloudy P.M. 26. Cloudy A.M.: clear P.M. 27. Cloudy A.M.: clear, fine P.M. 28. Bright A.M.: clear, fine, aurora P.M. 29. Clear A.M.: clear, fine P.M. 30. Clear A.M.: damp P.M.

Mean temperature of April for twenty-eight previous years . 43°·48

Mean temperature of this month ..... 43·20

Mean temperature of April 1854 ..... 44·68

Average quantity of rain in April for fourteen previous years 1·83 inch.

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

Days of Month.	Barometer.				Thermometer.				Wind.			Rain.		
	Chiswick.		Orkney, Sandwick.		Chiswick.		Orkney, Sandwick.		Chiswick.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
	Max.	Min.	9 1/2 a.m.	8 1/2 p.m.	Max.	Min.	9 1/2 a.m.	8 1/2 p.m.	1 p.m.					
1855.														
April.														
1.	30'382	30'221	30'00	30'15	48	22	36	36 1/2	ne.	n.	sse.	.....	.....	.....
2.	30'173	29'97	29'86	29'85	46	27	36	42 1/2	se.	s.	sse.	.....	.....	.....
3.	29'723	29'566	29'48	29'64	51	39	38	42	s.	ese.	ssw.	.....	.....	.....
4.	29'868	29'631	29'40	29'79	51	23	41 1/2	42 1/2	ne.	nne.	w.	.....	.....	.....
5.	30'051	30'013	29'65	29'73	57	33	46	47 1/2	ne.	w.	w.	.....	.....	.....
6.	30'135	30'119	29'67	29'72	64	40	52	48	nw.	ws.	w.	.....	.....	.....
7.	30'120	29'994	29'73	29'80	60	32	49	44	nw.	nw.	w.	.....	.....	.....
8.	30'050	29'909	29'63	29'53	53	38	45	39 1/2	nw.	nw.	w.	.....	.....	.....
9.	29'811	29'404	29'35	29'61	58	39	47	46	w.	sw.	w.	.....	.....	.....
10.	29'301	29'253	28'80	28'52	54	40	43 1/2	41	w.	w.	wnw.	.....	.....	.....
11.	29'556	29'423	29'08	29'32	57	42	47	43	w.	w.	nw.	.....	.....	.....
12.	29'572	29'512	29'09	29'03	60	46	48	42	sw.	wnw.	nw.	.....	.....	.....
13.	29'607	29'340	29'20	29'49	57	39	52	44	s.	s.	e.	.....	.....	.....
14.	30'002	29'844	29'47	29'57	62	40	47 1/2	43	w.	wnw.	nw.	.....	.....	.....
15.	30'201	30'091	29'61	29'61	65	41	54 1/2	45 1/2	w.	w.	e.	.....	.....	.....
16.	30'267	30'219	29'79	29'82	71	41	53	45 1/2	w.	w.	sw.	.....	.....	.....
17.	30'343	30'322	29'97	30'32	65	32	48	40	e.	ne.	s.	.....	.....	.....
18.	30'342	30'224	29'98	30'00	66	23	43	46	ne.	se.	w.	.....	.....	.....
19.	30'129	30'005	29'75	29'86	67	36	43 1/2	46 1/2	ne.	se.	w.	.....	.....	.....
20.	30'339	30'162	29'77	30'09	67	27	47 1/2	40 1/2	n.	nw.	nw.	.....	.....	.....
21.	30'485	30'474	30'11	30'45	54	28	44	45	e.	h.	w.	.....	.....	.....
22.	30'473	30'327	30'17	30'40	52	28	47	47	e.	ene.	s.	.....	.....	.....
23.	30'503	30'422	30'14	30'43	60	22	43 1/2	49 1/2	ne.	nw.	w.	.....	.....	.....
24.	30'321	30'174	29'92	30'30	69	40	47	48	n.	nw.	nw.	.....	.....	.....
25.	30'161	30'134	29'80	30'29	51	27	44	47	ne.	nne.	ne.	.....	.....	.....
26.	30'230	30'212	29'87	30'24	57	31	43	45 1/2	n.	ne.	se.	.....	.....	.....
27.	30'222	30'091	29'83	30'12	60	28	46	49	n.	nw.	wnw.	.....	.....	.....
28.	30'063	30'046	29'74	30'18	57	36	48	49	ne.	ene.	nw.	.....	.....	.....
29.	30'200	30'182	29'86	30'30	54	41	46	49	n.	ne.	sw.	.....	.....	.....
30.	30'222	30'162	29'92	30'32	53	38	42	47	n.	ne.	n.	.....	.....	.....
Mean.	30'095	29'991	29'6	29'872	58'20	33'96	45'6	45'05				0'26	0'23	2'89

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LXVIII. *On the Optical Properties developed in Transparent Bodies by the action of Magnetism.* By M. VERDET.

[With a Plate.]

PART I.\*

I PROPOSE in this first investigation to measure anew the rotations of the plane of polarization which are observed when the direction of the luminous ray is parallel to that of the magnetic forces, and to examine the laws according to which these rotations depend on the distance and on the force of the magnetic centres which act on the transparent bodies.

Various difficulties present themselves on commencing this investigation. First, in the usual conditions of the experiments, *e. g.* when the transparent body to be examined is placed between the branches of the electro-magnet of M. Ruhmkorff, the various points of the body cannot be regarded as subject to equal magnetic influences. Consequently the optical properties developed by magnetism ought to vary from one point to another, and observation can only exhibit the result of a number of unequal actions. A body of finite dimensions cannot be compared to a body of infinitely small dimensions, which is the reverse of what takes place in ordinary optical researches; hence the elementary law of the phenomena, that is to say, the only law which it is of consequence to determine, cannot be immediately deduced from these observations.

Secondly, the optical properties developed in an elementary slice of the transparent substance depend themselves on a very complex combination of circumstances. The electro-magnet is, in fact, a system of magnetic centres whose distribution is not exactly known; the properties acquired by an elementary plate of the substance depend evidently on the distance of these centres,

\* Abridged from the *Annales de Chimie et de Physique*, 3rd series, vol. xli. *Phil. Mag.* S. 4. No. 62. *Suppl.* Vol. 9.

and on the quantities of free magnetism which are there accumulated; and the law to be determined is that which would be observed if the magnetic action emanated from a single centre whose distance and force could be made to vary. As, in order to exhibit the phenomena on a scale sufficiently large for observation, we are forced to place the transparent substance very near the electro-magnet, we cannot assume that the action is analogous to that of one or of two poles, according as the electro-magnet has one or two branches.

We are ignorant also, in consequence of the complicated form of the apparatus, of the position of the poles in the electro-magnet. To remove the difficulties which arise from the inequality of the optical action of different plates of the same transparent body, I thought it would be sufficient to use an artifice employed by several authors, especially Mr. Faraday and M. Plücker, in the study of diamagnetic attractions and repulsions.

If we adapt to the two extremities of an electro-magnet two large armatures of soft iron, which present, opposite each other, two wide vertical faces, the space comprised between these vertical faces becomes, in Faraday's words, *a magnetic field of equal intensity*; *i. e.* a molecule of magnetic fluid\* placed anywhere in this space, excepting in the vicinity of its extremities, is submitted to a system of actions whose resultant varies very little in magnitude and direction. In fact, the two terminal surfaces of the armatures are charged with the greatest part of the free magnetic fluids (as experiment shows), and these fluids are distributed almost uniformly, although with a tendency to accumulate towards the edges.

If we now conceive a magnetic molecule placed in the centre of this space, it would be submitted to a system of forces whose resultant would have a certain magnitude, and whose direction would be, on account of the symmetry, the right line which unites the centres of the two terminal surfaces, *i. e.* the right line generally called the line of the poles. If we conceive the magnetic molecule to deviate from this position by receding from one of the armatures, it would approach the other; by approaching the edge of one armature it would recede from the opposite edge. Consequently, according as certain actions became more intense or more inclined towards the line of the poles, others would become less intense or less inclined. We can conceive, then, that in a certain region there could be established a compensation almost complete, in such a manner that, as before men-

\* It is scarcely necessary to say, that when I employ the expressions magnetic fluid and other similar ones, it is only for the sake of representing the phenomena more conveniently, and without committing myself to any particular theory of magnetism.



tioned, the resultant would remain almost constant in magnitude and direction. It is for experiment to show what case satisfies this condition.

The accuracy of these observations is evidently in no respect subordinate to the particular law of magnetic action, which is the law of the square of the distance. Consequently, without knowing the law according to which the optical properties of transparent substances develop themselves under the influence of magnetism, since we know that this law involves a variation with the distance, we may presume that, in placing a transparent substance between two armatures similar to those already mentioned, compensations of the same kind will be established for the different points of the substance, and the several infinitely small plates of which we can conceive it formed, will all acquire optical properties sensibly identical. Experiment completely confirms this supposition.

I have succeeded, in fact, without difficulty in fulfilling the conditions in question by furnishing one of Ruhmkorff's electro-magnets with suitable armatures. This electro-magnet was formed of two cylinders of soft iron, AB, A'B', 0<sup>m</sup>.20 long and 0<sup>m</sup>.075 in diameter, with a narrow canal bored in the direction of their axis to allow passage to the light; each cylinder was surrounded with about 250 metres of copper wire of 2.5 millims. diameter, and joined by the pieces of iron P and P' (Plate V. fig. 1)\*; these pieces are made to slide at pleasure along the piece of iron RS, and can be fixed in any position by means of the screws V, V'. On the two extremities B and A' of the horizontal branches I screwed two cylinders of soft iron, F, F', 0<sup>m</sup>.05 in height, and 0<sup>m</sup>.14 in diameter, bored along their axes by a narrow canal; and I found that when the distance between the terminal faces of these armatures was neither too great nor too small, when it amounted, for instance, to 50 or 90 millims., a transparent substance placed in the intermediate space acquired the same optical properties, whatever was its situation, provided it was not extremely near one or the other of the armatures. In fact, allowing a pencil of solar light to pass through the apparatus, I placed across the course of the pencil a parallelepiped of heavy glass, and having excited the magnetism of the electro-magnet, turned the birefracting prism which served as analyser until I perceived the violet tint known to physicists under the name of the *sensible tint* or *tint of passage*; I then displaced the parallelepiped of heavy glass parallel to itself,

\* Fig. 1 being an elevation of the apparatus, the two cylinders of soft iron are of course concealed by the copper wire which covers them; the internal canal is merely indicated by the lines, and the axis of the canal by an interrupted dotted line MN.

in order that the light might always pass through the same thickness; as long as I did not bring it in contact with one or the other armature, the *tint of passage* did not suffer any modification. The experiment succeeded as well with the other substances which I have used in my researches.

Thus, in my apparatus, the space comprised between the two terminal armatures was so constituted, that a piece of transparent substance placed in any point of this space, excepting in the vicinity of the edges, was everywhere affected in the same manner. Hence the optical properties of the fragment were the same in all points of its mass, and consequently they were the same (excepting the magnitude of the rotation) as those of an infinitely small element. At the same time this space was what Faraday terms a magnetic field of equal intensity. In fact, by employing a method which will be indicated further on, I found that the resultant of the action which the electro-magnet would exert on a molecule of magnetic fluid situated in any point of this space, was sensibly constant in magnitude and direction; as long as the molecule was not very near the limits, *e. g.* in a series of experiments where the distances of the terminal faces of the armatures were successively 50, 60, and 90 millims., I measured the resultant before defined at the centre C of the intermediate space, fig. 1, and found the numbers 134.12, 116.33, and 86.17. I measured it at the point D situated on the axis, and 15 millims. distant from one of the armatures, and found the numbers 133.87, 116.75, and 86.00: I measured it at a point E 25 millims. from the axis and in the plane, and found the numbers 133.5, 116.00, and 85.5.

The differences of the values corresponding to the same position of the armatures are sufficiently small to allow the conclusion, that, in the interior of a region whose dimensions do not exceed those of the transparent bodies submitted to experiment, the resultant of the actions which would be exercised on a molecule of free magnetic fluid does not vary one-hundredth part of its value, which fraction about represents the degree of accuracy attainable in these experiments.

For the sake of brevity, the resultant before defined will be hereafter called *the magnetic action in a given point*.

This simultaneous constancy of the magnetic action and of the optical properties would naturally lead to a simple conjecture, which experiment has entirely confirmed, and whose confirmation removes the second difficulty which I before noticed. We might, in fact, ask if the magnitude of the rotatory power developed in a transparent substance did not depend solely upon the magnitude of the magnetic action considered in the space occupied by the substance. In fact, these two quantities are simultaneously

constant and variable; besides, all the external actions of a magnet, its magnetizing action, its inductive action, its action on the element of a current, depend solely upon the action which it would exercise upon a molecule of magnetic fluid. It is then probable, that the action, in virtue of which it develops the rotatory power in transparent substances, depends solely upon the same quantity. If it be so, we need no longer trouble ourselves about the distribution of the free magnetism of the electro-magnet; no longer inquire separately into the influence of the variations of the intensity of magnetization, and the influence of the variations of distance. We shall measure, on the one hand, the optical action of the transparent substance, and on the other, a magnitude which at the same time represents in every point the effect of the variations of distance and the effect of the variations of intensity; we shall then have taken account at once of the influence of the two causes, and we can determine the elementary law of the phenomena without any of those arbitrary and inexact hypotheses to which we should have been necessarily conducted, if, for instance, we had wished to deduce the law relative to the influence of the distance in experiments where it would have been necessary to vary the distance of the transparent body from the extremity of one of the branches of the electro-magnet.

Experiment has, as we shall see, confirmed this supposition; and to obtain the elementary law which was sought, it has been sufficient to measure simultaneously the optical phenomena and the magnetic action.

The measurement of the optical phenomena has only been an application of methods known to all physicists, employed in the form which appeared to me most convenient for the peculiar conditions of my experiments.

A pencil of solar light, reflected by a heliostat into the dark chamber where I experimented, was polarized by passing through a Nichol's prism fixed at the entrance of the canal which passed through the electro-magnet in the direction of its axis. At the other extremity of the same canal the pencil of light encountered a very narrow diaphragm, and the portion which emerged fell upon the analysing apparatus placed at some distance. This apparatus, constructed by M. Brunner, was composed of a small telescope having an analysing prism placed before the object-glass, and capable of turning about its axis. The rotation of the telescope and of the analyser could be measured almost to a minute by the aid of a system of two concentric circles, of which the first, fixed to the support of the apparatus, was divided into degrees and thirds of a degree; and the other, moveable with the telescope, carried two opposed verniers giving the minute;

a screw allowed the telescope to be turned very slowly. The precision which it has been possible to give to the experiments has shown that none of those precautions were superfluous. The telescope was also capable of two rotatory movements, a vertical and a horizontal one, so that it was always possible to bring the axis into the direction of the pencil of light. The analyser was sometimes a birefracting prism of Iceland spar achromatized for the ordinary ray, sometimes one of Rochon's prisms. The telescope being adjusted so as to perceive clearly the image on the diaphragm, two images could be seen through the spar prism, and four through the Rochon prism, that is to say, two principal images, and two secondary images due to the imperfection of the construction. Having conveniently chosen the distance of the telescope and the diameter of the diaphragm, it could be so arranged that only the image whose variations were to be examined remained in the field of vision, an indispensable condition for the exactitude of the experiment. I took a diaphragm 3 millims. in diameter, and placed the lunette at a distance of 0<sup>m</sup>.80.

I used, besides, two methods which have given me results perfectly accordant; sometimes I employed homogeneous light, and determined the position of the plane of polarization by observing the extinction of the extraordinary image; sometimes I have employed white light, and have had recourse to the observation of the tint of passage.

In experimenting on homogeneous light, I neither made use of red glass, which would have given me too small deviations\*, nor of the monochromatic lamp, which would have furnished too feeble a light. I employed a solution of sulphate of copper in carbonate of ammonia, which, when its thickness amounts to some centimetres, only allows passage to the violet rays near to the ray G. If it be requisite that the light employed should have a sufficient intensity, the use of solar light is indispensable. Rotations of the plane of polarization almost twice as great as when operating with the tint of passage, are obtained by this mode of proceeding; but the estimation of the extinction is not always made with much exactitude, and depends in a singular manner upon the sensitiveness of the eye. I determined the two positions of the analyser which caused the image of the diaphragm to disappear by two movements in opposite directions; but as between the two disappearances it was necessary to illuminate the graduation in order to read off the position of the

\* We know, in fact, that the rotation of the plane of polarization due to the magnetic action varies with the length of an undulation, almost in the same manner as the rotation produced by quartz and by organic liquids. It is hence the smallest possible for red rays.

analyser, and since this circumstance might modify the sensibility of the eye, I did not take the mean of these two observations as corresponding to the position of the plane of polarization. I made at least four observations, oftener six or eight; these observations differed usually by ten minutes, but often the differences were as much as thirty minutes.

The observation of the tint of passage has given me generally more accuracy than the observation of the extinction of homogeneous light, and I have employed it more frequently. In fact, although the tint of passage results from the extinction of the most intense rays of the spectrum, *i. e.* of the mean yellow rays whose plane of polarization is much less deflected than the plane of polarization of the indigo rays, yet this smallness of deflection is more than compensated by the exactness with which the eye appreciates the variations of colour in the neighbourhood of the tint of passage.

Again, I had to employ solar light in this case, in consequence of the conditions peculiar to the phenomena which I studied. The rotation of the planes of polarization of the different colours being always very small, their dispersion was very small also, and consequently when the analyser had a position convenient for completely extinguishing the mean yellow rays, it extinguished also in a great measure the other rays of the spectrum, so that the tint of passage was only produced by a very small part of the incident light. If, then, this light had not been extremely intense, it would have been impossible to perceive the tint of passage, and the eye would have only perceived so feeble a minimum of light that all coloration would have escaped detection. It is superfluous to add, that it would have been necessary to operate quite otherwise in order to measure large rotations, such as those produced by great thicknesses of quartz or organic liquids. In this case the use of solar light would have only dazzled the eye and rendered all observation of the tint of passage quite inexact. I always determined the azimuth of the tint of passage four times; twice setting out from the red, and twice from the violet. There was generally never more than five minutes' difference between these four observations. The mean could therefore be regarded as correct to two or three minutes.

As to the estimation of the magnetic action, it was only after numerous unfruitful attempts that I found a satisfactory process. The first idea was to cause a steel needle, strongly tempered, and magnetized to saturation, to oscillate in the intermediate space between the two armatures. The square of the number of oscillations in a given time would have served as a measure of the magnetic action.

This process would have been sufficiently exact if the actions

to be measured had been feeble and incapable of altering the magnetic state of the needle; but the powerful electro-magnets necessary in my experiments would have considerably affected the magnetism of the needle, and the observations would not have been in any way comparable with one another. Neither the tempering of the needle, nor the magnetization to saturation, would have been a sufficient guarantee; since the magnetic state of any needle whatever undergoes, as is well known, a temporary change when it is brought near a strong magnet.

I tried, without much success, to use the action exerted by the electro-magnet on an unmagnetized body, magnetic or non-magnetic. Under the influence of an electro-magnet, a bar of a magnetic substance acquires a temporary magnetization; and if the body has no coercive force, it is admitted that this magnetization is proportional to the magnetic action. It follows hence, that the action exerted by the electro-magnet on the bar is proportional to the square of the magnetic action, and it only remains to measure it by known methods, *i. e.* by torsion or by oscillation. If the body under treatment be diamagnetic, although we do not know exactly in what manner the phenomena occur, it appears beyond doubt that a sort of polarity or temporary magnetization is developed, and consequently we admit that the action exerted by the electro-magnet is again proportional to the square of the magnetic action. Unfortunately in both cases the law in question is only an approximative one, sufficiently exact when it is required, for instance, to correct the effect due to the small variations on the power of an electro-magnet, but which cannot be the basis of a satisfactory process, destined to measure magnetic actions which vary between rather extended limits. In magnetic substances there exists always a sensible coercive force incompatible with so simple a law; in diamagnetic bodies no indications of the existence of a coercive force have yet been observed; but the only experiments we possess, in particular those of M. Edmond Becquerel, merely give an approximative law. I could therefore do nothing with this process, although I must admit that it might be useful in many cases: it might, for instance, be employed in confirming the constancy of the magnetic action in a given space, for this research is independent of the exact form of the law which represents the action of an electro-magnet on a magnetic or diamagnetic bar. In my experiments, on the contrary, the knowledge of this law would have been indispensable.

I thought of measuring the action of an electro-magnet on a small solenoid suspended between its branches. By using the bifilar mode of suspension employed by M. Wilhelm Weber in his electro-dynamic researches, this method would have offered

no great difficulties. The arrangement of my apparatus, however, did not allow me to apply it; besides that, it would have been less simple and convenient than the following, which I ultimately employed.

This method is founded on a remarkable consequence that can be deduced from the laws of induction established by MM. Neumann and W. Weber. In his first memoir on the mathematical theory of induced currents\*, M. Neumann has given a formula which represents the electromotive power developed by a magnetic pole in a closed conductor when displaced in any manner whatever. If the magnetic pole be regarded as the summit of a cone having for its base the closed conductor, the electromotive force developed by an infinitely small displacement of the current is proportional to the infinitely small variation in the angular opening of the cone, and consequently the sum of the electromotive forces developed by a finite displacement is proportional to the difference between the initial and final values of this angular opening†. I shall call this sum the *total electromotive force*. From this theorem the following conclusion may be deduced:—“If in a space where the magnetic action is constant in magnitude and direction, a circular conductor is placed so that its plane be parallel with the direction of the magnetic action, and if it be made to turn 90 degrees round an axis perpendicular to this direction, the total electromotive force developed is exactly proportional to the magnitude of the magnetic action.”

This conclusion would be evident if the magnetic action were simply due to one or two very distant poles. In order to demonstrate it on the general case, let us consider a plane conductor C, fig. 2, and a magnetic pole M, and let us suppose that the conductor suffers any displacement whatever which causes it to pass from the position C to the position C'. Let us call  $\mu$  the quantity of magnetism accumulated at the point M,  $d^2\omega$  the area of an infinitely small element  $o$  of the plane space surrounded by the conductor,  $r$  and  $r'$  the two successive distances  $oM$ , and  $o'M$  from this element to the point M,  $\phi$  and  $\phi'$  the angles made by the right lines  $oM$  and  $o'M$  with the normal to the conductor; the total electromotive force will be expressed by

$$\mu \left( \int \frac{d^2\omega}{r^2} \cos \phi - \int \frac{d^2\omega}{r'^2} \cos \phi' \right).$$

\* Memoirs of the Academy of Berlin for the year 1845.

† M. Neumann has not directly demonstrated by experiment the principles of his theory; he has deduced them from the law of Lenz. But we may regard the experiments of M. Weber, of M. Kirchhoff, and of M. Riccardo Felici, as having established the exactitude of the formulæ relative to the case of closed conductors, the only case with which we have here to occupy ourselves.

If the closed conductor be submitted to the action of any number of magnetic poles, the electromotive forces respectively developed by these different poles will be united, and their sum represented by

$$F = \Sigma \mu \left( \int \frac{d^2 \omega}{r^2} \cos \phi - \int \frac{d^2 \omega}{r'^2} \cos \phi' \right).$$

This equation can evidently be put in this form,

$$F = \int d^2 \omega \left( \Sigma \frac{\mu \cos \phi}{r^2} - \Sigma \frac{\mu \cos \phi'}{r'^2} \right).$$

Or if we call  $R$  and  $R'$  the resultants of the actions which the magnetic poles would exercise on the unit of magnetic fluid placed in  $o$  and in  $o'$ ,  $x$  and  $x'$  the angles made by these resultants with the normals to the conductor, we shall have

$$R \cos x = \Sigma \frac{\mu \cos \phi}{r^2}, \quad R' \cos x' = \Sigma \frac{\mu^2 \cos \phi'}{r'^2}.$$

If, as is supposed, the magnetic action is the same at all points of the conductor and of the space which it traverses when displaced, the two resultants  $R$  and  $R'$  would be constant throughout the extent of the conductor, and equal to one another. Calling  $\omega$  the total area of the conductor, we should have

$$F = \omega R (\cos a - \cos a').$$

If  $a = 0$ ,  $a' = 90^\circ$ , we have

$$F = \omega R ;$$

*i. e.* if the plane of the closed conductor is at first perpendicular and then parallel to the magnetic action, the total electromotive force is proportional to the area of the conductor and this magnetic action itself.

During the displacement of the conductor, the induced current is at each instant proportional to the electromotive power developed in the conductor, and consequently a quantity of electricity proportional to this force passes through any section whatever of the wire. Hence it follows, that the total quantity of electricity which passes through any section of the wire during the whole duration of the movement is proportional to the total electromotor force. It is then proportional to the magnetic action in the case just considered. Now this total quantity of electricity is precisely the only thing relative to the induced current which we can measure by the aid of the galvanometer : it is often described, but falsely, under the name of intensity of the induced current ; we shall designate it simply by the expression *induced current* ; thus the proposition demonstrated above could be thus enunciated, under the conditions already defined, the induced current is proportional to the magnetic action.



Consequently, if we conceive a system of circular conductors constituting a helix whose dimensions do not exceed those of the space where the magnetic action is constant, a rotation of 90 degrees round an axis perpendicular to the direction of the magnetic action will develop an induced current proportional to the magnetic action. If the movement is made very rapidly, the induced current can be measured without difficulty by the galvanometer, and the magnetic action will thus be determined\*.

I constructed, therefore, a small bobbin capable of turning about one of its diameters; and in order to give to the phenomena all possible intensity, the dimensions of wire around the bobbin were such that its resistance was almost equal to that of the wire of the galvanometer, of which we shall afterwards speak†. To this end, 23 metres of copper wire covered with silk, 0.5 millim. in diameter, were rolled so as to form a bobbin 28 millims. in external diameter, 12 millims. in internal diameter, and 15 millims. high. This bobbin was mounted upon a copper support, represented in fig. 3, which was fixed upon the electromagnet in the middle between its two branches. By means of the button B, the bobbin C could be turned 90 degrees round the dotted line FG; and in consequence of the arrangement of the apparatus, the axis of rotation was perpendicular to the line of the poles, *i. e.* to the direction of the magnetic action. The transparent bodies submitted to experiment were placed on the piece of copper L, fixed on the support above the bobbin. The stem A could be raised or lowered by means of a rackwork regulated by the screw D, the transparent body or the bobbin could be brought at pleasure to the same point. The stem A could be retained in a fixed position by the screw H.

If the dimensions of the bobbin are given, by varying the diameter of the wire, we vary in inverse ratio the number of layers which can be arranged in the height and length of wire of which each layer is formed. Calling then  $d$  the diameter, we should have

$$n = \frac{k}{d};$$

\* M. Weber has founded upon the same principles a very remarkable method for comparing the two components of terrestrial magnetic action, in order to determine the value of the inclination. Mr. Faraday made use of analogous processes in studying the distribution of the magnetic forces round a magnet.

† This condition is easily deduced from the law of Ohm. In fact, let  $L$  be the resistance of the galvanometer,  $\lambda$  that of a layer of bobbin wires contained in the same plane,  $n$  the number of these layers, of the electromotor force, developed in one layer; the induced current would be expressed

by  $\frac{nf}{n\lambda + L}$ .

and since the resistance of a layer of wires is proportional to the length, and in the inverse ratio of the square of the diameter, we should have

$$\lambda = \frac{h}{d^3}.$$

On the other hand, since the sum of the electromotor forces developed in a layer is proportional to the length of the wire, we may set  $f = \frac{g}{d}$ . By substituting these values, the preceding expression becomes

$$\frac{kgd^2}{kh + Ld^4},$$

and its maximum is given by the relation

$$kh - Ld^4 = 0.$$

As, however,  $\frac{kh}{d^4}$  expresses the resistance of the bobbin, we see that this resistance ought to be equal to that of the galvanometer.

The extremities of the wire of the helix were connected with those of a galvanometer constructed according to the system of M. Wilhelm Weber. It is known that the object of the arrangements adopted by this physicist, is to allow the deviations of the needle to be reduced to a very small amplitude, the smallness of these deviations being compensated by exactitude of measurement. To this end, the needle of the galvanometer is suspended by means of a frame of copper to a vertical mirror placed above the frame of the galvanometer, and itself suspended at the lower extremity of a silk thread without torsion, which supports the whole apparatus. At some distance from the mirror, and almost in the same horizontal plane, is placed a scale divided into millimetres, above which is a small telescope; by means of the telescope the image of the scale reflected in the mirror is read off, and by conveniently removing the scale and the telescope, it may be arranged that the image of the division on the scale immediately under the axis of the lunette, coincides with the vertical cross-wire when the magnetized needle is in the position of equilibrium. The axis of the telescope is thus normal to the mirror; as it is also perpendicular to the divided rule, it is easy to see, that if the needle and the mirror, which are invariably connected, are deflected through any angle, the image of the scale seen in the telescope will suffer a displacement equal to the tangent of double the angle through which the mirror is to be turned, this tangent being drawn to a circle whose radius is equal to the distance from the mirror to the scale. Hence it is the same if the axis of the telescope be not perfectly normal to the mirror, provided

the angle through which the whole system is turned be sufficiently small. By increasing the distance from the scale to the mirror, the precision of measurement is in some degree indefinitely increased, provided the telescope have a magnifying power sufficient to show the divisions clearly. In my experiments the most convenient distance appeared to me to be that of  $1^m\cdot25$ ; an angular displacement of 80 seconds corresponded to each division of the graduated scale; and since a quarter of a division could be easily estimated, the accuracy of the measurement reached twenty seconds.

In my instrument, the magnetized needle was a large bar of steel,  $0^m\cdot3$  long by  $0^m\cdot015$  in diameter; the wire of the galvanometer was 100 metres long and 1 millim. in diameter, and was rolled on an oval frame of yellow copper,  $0^m\cdot15$  in length and  $0^m\cdot10$  in width. The metallic mirror was a square of  $0^m\cdot04$  each side, and the cocoon thread without torsion was  $0^m\cdot35$  long. In the interior of the frame of brass was another frame of red copper, of the same form but 1 centimetre thick, intended to damp the oscillations of the needle by the counter-action of the induced currents which the movement of the needle develops in its mass. In the construction of this apparatus I employed M. Ruhmkorff, who acquitted himself with his usual ability.

If along the wire of such a galvanometer a current of very short duration is passed, this current communicates to the needle an impulse proportional to the integral of the actions which it exercises during the successive instants of its duration, and consequently proportional to the total quantity of electricity which it causes to pass through any section whatever of the wire; this quantity is, as was seen above, the measure of the magnetic action exerted at the point where the helix of induction is situated. If no resistance was offered to the movement of the needle, it would perform oscillations entirely comparable to those of a pendulum *in vacuo*, and the sine of the semi-deviation would exactly measure the initial impulse. In reality the needle suffers various resistances, among which the most important is that which proceeds from the reaction of the induced currents developed in the copper frame and in the galvanometric wire itself; and the amplitude of its oscillations decreases so rapidly, that the previous relation does not take place. But if the decrease in the oscillations occurs in geometrical progression, and if the initial deviation do not exceed a certain limit, it may be demonstrated, according to M. Weber, that the initial displacement of the image of the scale is proportional to the initial impulse.

I assured myself by repeated trials that these conditions were satisfied in my galvanometer. I took, in consequence, as a measure of the induced current and of the magnetic action, the dis-

placement of the image of the scale observed in the small telescope. By suppressing the elliptic frame of red copper, we should increase without doubt the amplitude of the elongations, but we should fall into the inconvenience which makes the use of galvanometers with a single needle, such as the sine and tangent boussoles, so restricted. The needle deviated by the impulse of a current, only rests again at the expiration of some minutes; the least accidental cause would communicate to it a motion which would be equally long in disappearing, so that the successive observations would necessarily be separated by a considerable interval. On the contrary, in M. Weber's apparatus, the influence of the induced currents in the copper frame weakens the oscillations of the needle, and fixes it in its position of equilibrium with a promptitude surprising to those who observe it for the first time. The effect of small accidental oscillations is destroyed almost immediately, and nothing hinders the observations from succeeding each other at very close intervals\*.

It is now easy to perceive how each experiment was made. I commenced always by making two or three observations on the

\* This advantage is rendered much more sensible by means of an ingenious modification suggested by M. Ruhmkorff. This consists in cutting hollow the magnetized bar so as to diminish considerably its moment of inertia without sensibly altering its magnetic moment. It follows evidently, that the damping of the oscillations ought to be much more rapid; the formulæ developed in the preceding note give the mathematical expression of this difference, and experiment shows very evidently that it is correct. M. Ruhmkorff had joined two different magnetized bars to the galvanometer which he had constructed for my experiments; the one was solid, and the other hollow; both were magnetized to saturation, and had the same external dimensions. With the solid bar, the amplitudes of the oscillations decreased according to a geometric progression whose ratio was equal to 0.646; with the hollow bar the geometric progression had for ratio 0.477. The following are the two series of observations relative to the two bars:  $n$  is the number of the order of each elongation observed, first on one and then on the other side of the position of equilibrium;  $a$  the amplitude of the elongation;  $r$  the ratio of one elongation to the preceding.

Solid Bar.			Hollow Bar.		
$n$ .	$a$ .	$r$ .	$n$ .	$a$ .	$r$ .
1 +	179.0		1 +	180.0	
2 -	116.0	0.648	2 -	83.5	0.475
3 +	75.0	0.646	3 +	41.25	0.482
4 -	48.5	0.646	4 -	19.5	0.473
5 +	31.75	0.654			
6 -	20.25	0.638			
Mean . . .		0.646	Mean . . .		0.477

Thus the movement of the hollow bar is damped in three oscillations as much as the movement of the solid bar in five oscillations. As, besides, the oscillations of the hollow bar are more rapid than those of the solid bar, the advantage of the hollow bar is made again much more sensible.

induced current by the rotation of the small bobbin ; then raising, by means of a movement of the rack A, fig. 3, the transparent body into the place previously occupied by the bobbin, I turned the analyser so as to determine the azimuth of the tint of passage if I worked with white light, or the azimuth of extinction if I worked with homogeneous light, reversing the direction of the current, but taking care not to break the circuit\*. I determined anew the same azimuth. The difference of the two observations evidently gave the double of the rotation of the plane of polarization, if the magnetic action had not perceptibly varied during the experiment. In order to assure myself, I measured the magnetic action immediately after the determination of the second azimuth, and only regarded those experiments as good where the two measurements of magnetic action did not differ in their mean value by more than a hundredth of that value. It is this mean value which I have set in the tables to be given further on. I perceived, like observers who have preceded me, that the magnetism of the electro-magnet takes a certain time to develop itself and attain its maximum. It is not then necessary to commence the experiments immediately after closing the voltaic pile ; it is only after some time that the magnetism has taken a value which does not vary perceptibly during a complete experiment.

The galvanometer ought to be placed at a great distance from the electro-magnet, in order that it may not be affected by it ; I was obliged to have an assistant to move the little bobbin while I observed the motions of the magnetized bar. I have not had recourse to any particular mechanical arrangement, experience having shown me, that, notwithstanding the little irregularities which movements effected by means of the hand always involve, the results of many consecutive observations agreed perfectly, provided the movement was sufficiently rapid. If by chance the movement was too slow, or if it was performed at different times, I perceived it by the movement of the needle, and I recommenced the experiment.

I have experimented solely on three substances,—the heavy glass of Faraday, common flint, and bisulphide of carbon ; but these three substances differ so much from one another, that a law which suits them equally may be regarded as general. I had at my disposal two specimens of heavy glass ; the first, which M. De la Rive was good enough to lend me, was a parallelepiped with square base, 40 millims. long by 13 millims. on the side, polished on its two bases and on two of the lateral faces ; the second, which belonged to the collection of the Ecole Normale

\* This precaution is useful, so as not to cause variations of intensity in the current.

supérieure, was a rectangular parallelepiped polished on its six faces, whose edges were respectively equal to 37.2 millims., 26.0 millims., and 12.5 millims. I describe the two samples as No. 1 and No. 2. The specimen of flint was a parallelepiped of square base 43.3 millims. long by 14.5 millims. on the side, two of its bases, and a couple of its lateral faces, being polished. These three pieces were not entirely free from defect; but by elevating or lowering the support on which they were placed, and by making them slide on this support, the ray of light could always be made to traverse a space where the defect had no perceptible influence, so that by a convenient position of the analyser the incident light could be completely extinguished when the electro-magnet was not magnetized. The bisulphide of carbon was contained in small glass cylinders, closed by plates of ordinary glass; one of these cylinders was 44 millims., and another 31 millims. long. I satisfied myself beforehand that the rotation due to the terminal plates of glass was entirely imperceptible.

The whole of the experiments establish a very simple law,—  
 “The rotation of the plane of polarization is proportional to the magnetic action.” The following Tables, which only contain a part of my experiments, give the proof of this law. In these tables F is the magnetic action measured by the deviation directly observed in the galvanometer, R the double of the rotation, and Q the value of the quotient  $\frac{R}{F}$  when we suppose R expressed in minutes. In each table the words *white light* or *homogeneous light* indicate the manner in which the optical observation is made; the thickness of the substance traversed by the luminous rays is also indicated.

*Experiments on heavy Glass, No. 1.*

I.*			II.†		
White light.		Thickness, 40 millims.	Homogeneous indigo light. Thickness, 40 millims.		
F.	R.	Q.	F.	R.	Q.
143.37	9 13 45	3.86	157.5	16 36	6.32
115.00	7 28 30	3.90	119.0	13 13	6.66
112.37	7 17 45	3.89	109.62	11 44	6.42
87.75	5 46 45	3.95	Mean . . . 6.46		
63.62	3 55 45	3.71			
Mean . . .		3.86			

\* The hundredths of division and the seconds that we shall see in these tables result from the calculation of the mean.

† In these two experiments, the distance from the divided rule to the mirror was not the same, so that the magnetic actions given in the Tables I. and II. are not measured with the same unity.

*Experiments on heavy Glass, No. 2.*

III.			IV.		
White light. Thickness, 37·2 millims.			White light. Thickness, 26 millims.		
F.	R.	Q.	F.	R.	Q.
148·25	6° 05' 15"	2·80	143·81	4° 31' "	1·88
116·37	5 28	2·82	109·62	3 30 45	1·92
107·00	5 9 30	2·89	85·37	2 48	1·97
92·87	4 26	2·84			
89·37	4 20	2·91			
83·50	4 4 20	2·93			
59·37	2 57 15	2·98			
Mean . . .		2·88			1·93

*Experiments with common Flint.*

V.		
White light. Thickness, 43·3 millims.		
F.	R.	Q.
148·00	4° 40' 45"	1·90
123·81	4 2 30	1·96
92·75	2 57 15	1·91
Mean . . . . .		1·92

*Experiments with Bisulphide of Carbon.*

VI.			VII.		
White light. Thickness, 44 millims.			White light. Thickness, 31 millims.		
F.	R.	Q.	F.	R.	Q.
150·37	6° 16' 15"	2·50	149·62	4° 19' 30"	1·73
112·87	4 37 30	2·46	113·5	3 23	1·78
94·19	3 55	2·49	93·5	2 34 45	1·65
69·00	2 54	2·52			
Mean . . .		2·49			1·72

VIII.

Homogeneous indigo light. Thickness, 44 millims.

F.	R.	Q.
148·5	10° 47' "	4·37
124·5	9 29 30	4·57
94·4	7 7 30	4·53
Mean . . . . .		4·49

We see from these tables that the magnetic action and the rotation may vary in the relation of 1 to 3, and still remain proportional. It is further to be remarked, that in each series of experiments the magnetic action is made to vary in two ways; by varying sometimes the intensity of the current, sometimes the distance of the branches of the electro-magnet. From 4 to 20 Bunsen's elements were employed, and the distance between the armatures was varied from 50 to 90 millims., so that the distance from each of the armatures to the middle of the transparent substance might vary in the relation of 1 to 2. In order to show that in both cases the law was the same, and also to give an idea of the agreement in the individual observations, of which the preceding tables only contain the mean, I shall give here the experiments I. and VI. in detail.

*Experiment No. I.*

Heavy glass, No. 1; thickness, 40 millims. White light was employed. Distance between the armatures, 60 millims. 20 Bunsen's elements.

The magnetic action was measured; the mean of two consecutive observations was . . .	143·25
Azimuth of the tint of passage . . . . .	182° 28' 45"
Azimuth of the tint of passage after reversing the current . . . . .	191° 42' 30"
Second determination of the magnetic action	143·5

Double rotation, 9° 13' 45"; magnetic action, 143·37; ratio, 3·86.

*Experiment No. II.*

Distance between the armatures, 80 millims. 20 Bunsen's elements.

Magnetic action . . . . .	115
Azimuth of the tint of passage . . . . .	183° 21'
Azimuth after inverting the current . . . . .	190° 49' 30"
Magnetic action . . . . .	115

Double rotation, 7° 28' 30"; mean magnetic action, 115; ratio, 3·90.

*Experiment No. III.*

Distance between the armatures, 60 millims. 10 Bunsen's elements.

Magnetic action . . . . .	112·75
Azimuth of the tint of passage . . . . .	183° 24' 45"
Azimuth after inverting the current . . . . .	190° 42' 30"
Magnetic action . . . . .	112

Double rotation, 7° 17' 45"; mean magnetic action, 112·37; ratio, 3·89.



*Experiment No. IV.*

Distance between the armatures, 80 millims. 10 Bunsen's elements.

Magnetic action . . . . .	87·5
Azimuth of the tint of passage . . . . .	190° 0'
Azimuth after inverting the current . . . . .	184° 13' 15"
Magnetic action . . . . .	88·0

Double rotation, 5° 46' 45"; mean magnetic action, 87·75; ratio, 3·95.

*Experiment No. V.*

Distance between the armatures, 90 millims. 10 Bunsen's elements.

Magnetic action . . . . .	64·0
Azimuth of the tint of passage . . . . .	189° 0'
Azimuth after inverting the current . . . . .	185° 4' 15"
Magnetic action . . . . .	63·25

Double rotation, 3° 55' 45"; mean magnetic action, 63·62; ratio, 3·71.

*Experiment No. VI.*

Bisulphide of carbon; thickness, 44 millims. White light was used. Distance between the armatures, 62 millims. 20 Bunsen's elements.

Magnetic action . . . . .	151·0
Azimuth of the tint of passage . . . . .	183° 53' 45"
Azimuth after inverting the current . . . . .	190° 10'
Magnetic action . . . . .	149·75

Double rotation, 6° 16' 15"; mean magnetic action, 150·37; ratio, 2·50.

*Experiment No. VII.*

Distance between the armatures, 90 millims. 20 Bunsen's elements.

Magnetic action . . . . .	113
Azimuth of the tint of passage . . . . .	184° 39' 15"
Azimuth after inverting the current . . . . .	189° 16' 45"
Magnetic action . . . . .	112·75

Double rotation, 4° 37' 30"; mean magnetic action, 112·87; ratio, 2·46.

*Experiment No. VIII.*

Distance between the armatures, 62 millims.	6 Bunsen's elements.
Magnetic action . . . . .	94·12
Azimuth of the tint of passage . . . . .	185° 0' 45"
Azimuth after inverting the current . . . . .	188° 55' 45"
Magnetic action . . . . .	94·25
Double rotation, 3° 55' ; mean magnetic action, 94·19 ;	ratio, 2·49.

*Experiment No. IX.*

Distance between the armatures, 90 millims.	6 Bunsen's elements.
Magnetic action . . . . .	68·5
Azimuth of the tint of passage . . . . .	185° 29' 30"
Azimuth after inverting the current . . . . .	188° 23' 30"
Magnetic action . . . . .	69·5
Double rotation, 2° 54' ; mean magnetic action, 69·0 ;	ratio, 2·52.

Each of these observations ought to be exact, as I have before said, to two or three minutes, and consequently the measure of the rotations does not allow a greater error than five or six minutes; the probable error of the measurement of magnetic action is half a division of a scale. Although these errors are so small, we may rest satisfied that they are sufficient to account for the differences which exist between the several values of the ratio of the rotation to the magnetic action determined in the same series of observations.

It is important to notice, that the proportionality of the rotation and of the magnetic action is confirmed with the same exactitude whether the distance from the magnetic centres to the transparent substance is changed, or whether the quantity of free magnetism accumulated in these various centres suffers a variation. This law of proportionality is demonstrated by our experiments for transparent bodies of finite dimensions, all of whose parts are equally affected by the electro-magnet; it is consequently true for all the infinitely small plates of which we can conceive the transparent body to be composed. It follows, hence, that the formula can be stated in the following manner:—*The rotatory power developed by the action of a magnetic centre in an infinitely thin plate of a monorefracting substance varies proportionally to the magnetic action, i. e. directly as the quantity of magnetism accumulated in this centre, and inversely as the square of the distance.*

M. Wiedemann has demonstrated that the rotatory power developed by the direct action of electric currents is proportional to the intensity of these currents. He has made no experiments on the influence exercised by the relative situation of the electric currents and of the transparent bodies; but if my results are compared with those of M. Wiedemann, if further, the general identity of the properties of magnets and of systems of closed currents be taken into account, it will appear evident that the rotatory power developed by a system of closed currents in an infinitely thin plate of a transparent substance ought to be proportional to the action which the system would exercise on a molecule of magnetic fluid.

I find a complete contradiction to the law announced by M. Bertin, according to which the rotation of the plane of polarization due to the influence of a single magnetic pole will decrease in geometric progression when the distance from the transparent substance to the pole increases in arithmetical progression. The explanation of this disagreement is not difficult to give. M. Bertin considers as pole, the terminal surface of soft iron of one of the branches of Ruhmkorff's electro-magnet. Now this surface could not be regarded as pole, at least if to this expression be attributed its precise signification; it is a system of magnetic centres distributed over a great surface, and whose action cannot be assimilated to that of a single centre. We cannot then look for the elementary law which makes the rotation of the plane of polarization depend on the distance from the transparent substance to this polar surface; we can only find an empirical formula, which ought to change when the electro-magnet is changed, or even when we simply change the terminal armatures of the same electro-magnet. But if the law which I have established in this memoir be true, the empirical formula which represents the decrease of the rotations at different distances from the polar surface ought also to represent the decrease of the magnetic action, since these two quantities are always proportional; consequently, in the apparatus of M. Bertin, the magnetic actions ought to decrease in geometric progression when the distances from the polar surface increase in arithmetical progression.

Direct experiments have completely confirmed this conclusion. The apparatus which I used, and which belongs to the Ecole Normale supérieure, was that which M. Bertin also used in his researches. I took away one of the branches of the electro-magnet, and at the extremity of the branch that I retained, I screwed instead of the large armature of my experiments, the small hexagonal armature which M. Ruhmkorff generally uses in his apparatus, and which M. Bertin himself used. I determined by the method before indicated, the magnitude of the

magnetic action at different distances from the terminal surface of this armature\*, and observed a very slow decrease, which may fitly be represented by a decreasing geometrical progression, although it may be in reality a little less rapid.

This may be seen in the following Table:—

Distance from the polar surface. mm	Magnetic action †.	Ratio of each magnetic action to the preceding.
20	192.25	
30	146.00	0.76
40	113.75	0.78
50	91.00	0.80
60	73.75	0.81
70	61.25	0.83
	Mean . . .	0.796

I repeated the experiment after an interval of two months, during which time the electro-magnet had been very frequently put in use, and I obtained again the same law, as the table shows.

Distance to the polar surface. mm	Magnetic action †.	Relation of each magnetic action to the preceding.
25	129.0	
35	97.0	0.75
45	76.0	0.78
55	60.9	0.80
65	49.7	0.81
	Mean . . .	0.785

Thus, by the effect of time and of use, the electro-magnet did not appear to be modified, so that the law of its action on an external point was sensibly changed. We may then be permitted to believe, that when M. Bertin made his experiments in 1847 and 1848, if he had measured the magnetic actions exercised at different distances, he would have obtained results perfectly similar to the preceding; he would therefore have been able to represent these magnetic actions by a decreasing geometric pro-

\* This method only gives the value of the magnetic action exactly when this action is constant in the space where the inductive bobbin moves; but if the values of the magnetic action at different points of this space are slightly different, it is clear that the methods determine practically the mean value.

† The magnetic actions inserted in this column have been determined by the method of alternatives, in order to eliminate the influence of small variations of the intensity of the current. The pile was composed of 10 Bunsen's elements.

‡ See preceding note.

gression, whose ratio would have been very little different from the mean of the two preceding determinations, *i. e.* of 0.790. Now we find in the memoir of M. Bertin\* five series of experiments relative to the decrease in the rotations observed at different distances from a single branch of the electro-magnet. The two first are relative to Faraday's heavy glass, and may be represented by two decreasing geometric progressions whose ratios are respectively 0.78329 and 0.78330 for 10 millims. of increase of distance; the third is relative to a flint prepared by M. Mathiessen, and is represented by a decreasing geometrical progression whose ratio is 0.78233; the fourth is relative to the bisulphide of carbon, and is represented by a decreasing geometric progression whose ratio is 0.78329; lastly, the fifth relates to the heavy glass of Faraday, and is represented by a decreasing geometrical progression whose ratio is 0.78329. The mean of these five geometrical progressions is 0.78318, and differs very little from 0.790. Thus the experiments of M. Bertin agree entirely with the law which they seem to contradict.

In order to show well the influence which the form of the terminal surfaces of the electro-magnet exercises upon the law of the decrease of magnetic action, I repeated the same series of experiments by screwing one of my large armatures to the extremity of the branch of the electro-magnet which I employed. I obtained a law of decrease much more slow than in the preceding case, which may again be represented by a decreasing geometrical progression. On the contrary, when I replaced the large armature by a cone of soft iron 24 millims. high, with a diameter at the base of 45 millims., the decrease was much more rapid, and entirely different from that which would have indicated a decreasing geometrical progression. The following Tables contain the results of these experiments:—

*Large armature.*

Distance to the polar surface.	Magnetic action.	Ratio of each magnetic action to the preceding.
mm 22	77.00	
32	73.75	0.96
42	67.87	0.92
52	61.75	0.91
62	55.50	0.90
	Mean . . .	0.925

\* *Annales de Chimie et de Physique*, 3 sér. vol. xxiii. pp. 22, 23 and 27.

*Conical armature.*

Distance from the summit of the cone.	Magnetic action.	Ratio of each magnetic action to the preceding.
$\frac{\text{mm}}{25}$	137.0	
35	98.0	0.71
45	74.5	0.76
55	61.75	0.83

Lastly, the experiments No. III. and No. IV., and the experiments No. VI. and No. VII., furnish a confirmation which ought not to be neglected. In the experiments No. III. and No. IV., I measured the rotations produced by two different thicknesses of the same piece of heavy glass; if these measurements corresponded to equal magnetic actions, the rotations ought to be proportional to the thicknesses, in virtue of the identity of the action of all the plates of the substance. In reality the magnetic actions were not the same in the two experiments; but it is clear, that if the experiments had been well made, the ratios of the rotations to the magnetic actions ought to be proportional to the thicknesses. Now these relations are respectively equal to 2.88 and 1.92; by dividing them by the corresponding thicknesses 37.2 and 26, we obtain the quotient 0.077 and 0.074, that is to say, numbers whose differences do not exceed the errors of experiment. The experiments No. VI. and No. VII. relative to the bisulphide of carbon, lead to the same conclusion. The ratios of the rotations to the magnetic actions in the two experiments are 2.49 and 1.72; by dividing them by the corresponding thicknesses 44 and 31, the quotients 0.056 and 0.055\* are obtained.

## PART II†.

In publishing his discovery of the rotation of the plane of polarization produced by the influence of magnetism, Mr. Faraday stated that the phenomenon manifested itself with the greatest intensity when the direction of the ray of light was parallel to the direction of the magnetic forces, and that it disappeared when these two directions were perpendicular to one

\* It would not have been possible directly to compare the rotations produced by two different thicknesses under the influence of the same magnetic action, at least in the case of heavy glass. The solar light in traversing the glass heats it perceptibly, and the glass acquires a perceptible birefracting power in every direction perpendicular to that of the ray of light. Any observation with a thickness perpendicular to the first thickness studied is therefore impossible, as long as this birefracting power has not disappeared, and this disappearance often requires more than an hour.

† From the *Annales de Chimie et de Physique*, vol. xliii. for January 1855.

another ; but he said nothing of the manner in which the passage from one of these extremes to the other is effected. Subsequent observers have confirmed these two results, but have added nothing to them. M. Bertin made some experiments by placing a piece of flint or heavy glass upon the base of a cylindrical electro-magnet at different distances from the axis ; in this manner he observed that the rotations of the plane of polarization varied with the position of the transparent substance, but he discovered no law, and it is, in fact, evident that with the complex circumstances under which he experimented, the elementary law of these phenomena could not be discovered. M. Pouillet, M. Edmond Becquerel, and M. Wiedemann in all their experiments have directed the rays of light parallel to the action of the magnetic force.

In this second part of my investigation I propose to complete our knowledge of the phenomenon discovered by Mr. Faraday, by determining, in a general manner, what takes place when the angle formed by the direction of the rays of light with the direction of magnetic action varies from  $0^{\circ}$  to  $90^{\circ}$ . It will not be necessary to insist upon the inherent interest of this research ; it is evident that it must precede all discussion of the theories which have been, or may be proposed to explain these phenomena.

The apparatus usually employed is not well suited to experiments of this kind ; in them the rays of light must necessarily have the same direction as that of magnetic action. Ruhmkorff's apparatus is an example of this ; it was, in fact, constructed with a view to give the greatest possible intensity to the phenomena when the directions above mentioned were coincident. Nevertheless, at first I endeavoured to make use of Ruhmkorff's apparatus by giving a variable direction to the luminous rays by means of two successive reflexions from plane mirrors parallel to one another. A few experiments were sufficient to show that this modification of the apparatus was not adapted to give results worthy of confidence. On this account I experimented in a manner similar to Mr. Faraday, by placing the transparent substance so as to cause the rays of light to pass a little on one side, or a little above the polar extremities of the electro-magnet : as in my former experiments, however, the apparatus was arranged so that the space occupied by the transparent body was a *field of uniform magnetic intensity*.

For this purpose I procured a strong horse-shoe electro-magnet composed of two cylinders of soft iron, each 0.20 metre high and 0.075 metre in diameter, and each surrounded by 250 metres of copper wire 2.5 millimetres in diameter ; the two were fixed to the extremities of an iron bar AB 0<sup>m</sup>.35 long, 0<sup>m</sup>.07 broad, and

0<sup>m</sup>·015 thick, fig. 4, Plate V\*. The apparatus was mounted upon a copper pedestal which rested upon four screws V, V', V'', V''', so that it could turn around a vertical axis passing through the middle of the transverse bar AB; the vernier C, moving upon the surface of the graduated horizontal circle DE, indicated the horizontal displacement of the system. On the upper horizontal faces of the vertical branches two armatures of soft iron were fixed, whose disposition will be easily understood from the figure. Two prismatic pieces F and F' glided along two grooves G and G', and could be fixed in any desired position by means of the screws U and U'. At their extremities these two pieces carried two plates of soft iron HK and H'K', each 0<sup>m</sup>·016 long, 0<sup>m</sup>·04 broad, and 0<sup>m</sup>·005 thick. The two edges of these plates were exactly parallel, and, when they were withdrawn from each other to a convenient distance, it was easy to show by the method indicated in the preceding memoir, that the magnetic action was sensibly the same at all points of the rectangular space HK H'K', as well as at a small distance above and below the same; between the same limits the optic action was also invariable. In the middle of the space between the two branches of the electro-magnet was a vertical copper bar which reached almost to the level of the upper surfaces of the armatures, and carried at its extremity a horizontal plate L, graduated at the circumference. Above this plate was a second plate O, which turned on a vertical axis coincident with the axis of rotation of the electro-magnet, and was provided with a vernier which indicated tenths of a degree. Upon this second plate, and resting on a small ledge R, the transparent substance was placed. A vertical rod fixed to the side of the transverse bar AB carried a pointer S at the same height as the graduation upon the plate L. Two other vertical rods, fixed to the foot of the apparatus and independent of the electro-magnet, carried two blackened screens 15 centimetres in diameter. At the centre of one of these screens was a birefracting prism T, which polarized the incident light; in the centre of the other screen an aperture 3 millimetres in diameter was pierced, merely to allow a narrow cylindrical pencil of rays to pass through†. The analysing apparatus, which consisted of the small *lunette* used in my former investigations, was completely independent, and placed at some distance.

The experiments were conducted as follows:—A ray of solar light, which was reflected by a heliostat and polarized in passing through the birefracting prism T, was made to traverse the trans-

\* The figure represents a view looking down upon the apparatus. The ray of light MN and the direction of magnetic action PQ make an angle of 30°.

† The apparatus was constructed by M. Ruhmkorff.



parent substance, and afterwards analysed beyond the screen. The direction of magnetic action was first brought into parallelism with the direction of the rays of light, by making the zero of the vernier C coincide with the zero of the graduated scale beneath it; the transparent substance was next placed so that the faces of entrance and exit were perpendicular to the same direction: this was done by making the zero of the vernier on the plate O coincide with the zero of the corresponding graduation; and lastly, the optic phænomenon was observed. Afterwards, the electro-magnet was turned through any angle whatever; this of course caused the transparent substance to turn through an exactly equal angle, but it was brought back to its primitive position by an opposite and exactly equal rotation of the plate O; the luminous rays, however, preserved an invariable direction, and consequently they always traversed the same thickness of transparent substance. The apparatus being thus disposed, the optical phænomenon was observed, care being taken to repeat each experiment twice by turning the electro-magnet successively towards the right and towards the left; afterwards, the mean of these two results was taken in order to eliminate the possible influence of imperfections in the construction and arrangement of the several parts of the apparatus. With small angular displacements of the electro-magnet, these two results were not sensibly different from one another; when the angular displacement attained or exceeded 45 degrees, the difference between the two results was greater than the usual errors of observation.

In the first place, it was easy to corroborate the fact that the optical phænomenon is, in all cases, simply a rotation of the plane of polarization. By using homogeneous light, and first placing the analyser so as completely to extinguish one of the two images of the diaphragm, I have witnessed the reappearance of this image by the influence of magnetism; but whatever might have been the relative situations of the electro-magnet and of the transparent substance, I was always able to extinguish this new image by a convenient rotation of the analyser. By using white light I have seen the image reappear with variable colours, and by changing the position of the analyser, I have always found the tints to succeed each other in the order peculiar to these phænomena of rotation, whatever might have been the arrangement of the apparatus. In common with my predecessors, I have always observed that no effect was produced when the magnetic action was perpendicular to the direction of the rays of light.

Thus these phænomena manifest themselves in a less complicated manner than might have been expected; when the direction of the rays of polarized light is parallel to that of magnetic

action, there is a certain rotation of the plane of polarization produced; when these two rotations are perpendicular to one another, the rotation is zero; when the angle enclosed by both directions varies from  $0^\circ$  to  $90^\circ$ , the rotation decreases in a continuous manner. This general fact once established, all that remains is to determine, by exact measurements, the law of the decrease of the rotations. The apparatus and optical methods described in my preceding memoir remain, therefore, entirely applicable. For reasons stated in another place, I have always preferred the use of white light and the observation of the tint of passage, to the use of homogeneous light.

The measurement of the phenomena have again led me to a very simple law. *The rotation of the plane of polarization is proportional to the cosine of the angle enclosed between the direction of the luminous rays and that of magnetic action*; consequently it is proportional to the component of magnetic action parallel to the direction of the luminous rays.

I have experimented on the same substances as in my former researches, *i. e.* on heavy glass, flint, and bisulphide of carbon. The bisulphide of carbon was always enclosed in the same small vessels described in the first part of this memoir; the specimen of flint was the same as that I had already used, but besides the two specimens of heavy glass, of which I have before given the dimensions, I was enabled, through the kindness of Mr. Faraday, to add a third of somewhat smaller size.

In the following tables I shall merely record the results of two experiments, both of which were made by the method of observing the tint of passage; in these tables  $\alpha$  represents the angle enclosed by the rays of light and the direction of magnetic action,  $R$  and  $R'$  the *complete*\* rotations corresponding, respectively, to positions on the right and on the left of the luminous rays;  $M$  the mean of these rotations;  $Q$  the ratio of this mean, expressed in minutes, to the cosine of the angle  $\alpha$ ;  $M_1$  the calculated values of  $M$ , supposing the law of the cosine to be exact, and adopting, for the ratio of the rotation to the cosine of the angle  $\alpha$ , the mean of the values of  $Q$ ; and  $\Delta$  the difference between  $M$  and  $M'$ †.

\* I repeat that by this term, the difference between the two azimuths of the tint of passage, corresponding to two opposite directions of the electric current, is intended. As in my former researches, each of these azimuths was determined from four separate observations made at intervals of five or six minutes at most.

† In order to avoid the effect of variations in the electric current, I have adopted the method of alternatives; the rotation corresponding to  $\alpha=0$  being always taken as a term of comparison; the values in the table are those which would have been observed had the rotation corresponding to  $\alpha=0$  remained constant. I have calculated them by means of a proportion,

Experiments on Heavy Glass. Thickness 40 millims.

$\alpha$ .	R.	R'.	M.	Q.	M <sub>1</sub> .	$\Delta$ .
0	° ' "	° ' "	8 55 " 45	535.75	8 55 " 45	0 0 " 0
15	8 26 30	8 32 0	8 29 15	527.25	8 37 50	-0 8 35
30	7 38 0	7 42 0	7 40 0	531.25	7 44 0	-0 4 0
45	6 17 45	6 22 15	6 20 0	537.50	6 18 45	+0 1 15
60	4 22 30	4 35 0	4 28 45	537.50	4 27 45	+0 1 0
75	2 9 30	2 29 30	2 19 30	539.00	2 18 45	+0 0 45
Mean.	.....	.....	.....	535.65		

Experiments on Bisulphide of Carbon. Thickness 44 millims.

$\alpha$ .	R.	R'.	M.	Q.	M <sub>1</sub> .	$\Delta$ .
0	° ' "	° ' "	5 58 " 0	358.0	5 56 " 0	+0 2 " 0
15	5 44 0	5 47 30	5 45 45	357.25	5 44 0	+0 1 45
30	5 5 2	5 10 30	5 7 45	355.25	5 8 15	-0 0 30
45	4 62 0	4 16 0	4 9 0	352.0	4 11 45	-0 2 45
60	2 52 0	3 5 30	2 58 45	357.50	2 58 0	+0 0 45
Mean.	.....	.....	.....	356.00		

The law just established explains a circumstance which I had occasion to notice in my former researches; when the ray of light is parallel to the direction of magnetic action, the adjustment of the apparatus may be deranged to a sensible extent, and consequently the angle  $\alpha$  made to vary from 3 to 4 degrees, without altering the rotation of the plane of polarization to any appreciable extent.

If we adopt the theoretical notions of Fresnel with respect to the rotation of the plane of polarization, we must imagine the polarized ray, falling perpendicularly upon the transparent substance which is submitted to the influence of magnetism, transformed into two rays circularly polarized, and in opposite directions, and propagated with unequal velocities. If the velocities of propagation be represented by  $v$  and  $v'$ , it follows from the law above enunciated, that the expression  $\frac{1}{v} - \frac{1}{v'}$  varies proportionally to the cosine of the angle enclosed between the direction of the luminous rays and that of magnetic action.

I shall confine myself to this theoretical remark, and for the present abstain from any further reflections on the law I have just established.

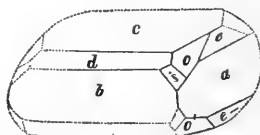
LXIX. *On the Crystalline Form of Leucophane.*

By R. P. GREG, Esq.\*

**M**INERALOGISTS have not hitherto satisfactorily determined the crystalline form of *leucophane*, from the want probably of good crystals. In the last edition (fourth) of Dana's 'Mineralogy' it is described as *prismatic*, with cleavages in three directions inclined to each other at angles of  $53\frac{1}{2}^{\circ}$  and  $36\frac{1}{2}^{\circ}$ ; while in Brooke and Miller's late edition of 'Phillips,' *leucophane* is stated as belonging to the *anorthic* system.

As this mineral, from its chemical composition, locality, and associated minerals, forms an interesting and important species, I propose briefly to describe a large crystal in my possession, and which I believe is the only tolerably perfect crystal of *leucophane* yet obtained. The accompanying figure is drawn so as to represent the most perfect portion of the crystal, the dotted lines indicating those faces or edges which are imperfect or broken in the crystal itself.

	Greg.	Miller.
$a : c$	$\begin{matrix} 90 & 0 \end{matrix}$	$\left\{ \begin{matrix} 89 & 54 \\ 89 & 28 \end{matrix} \right\}$
$c : b$	$\begin{matrix} 90 & 0 \end{matrix}$	$\left\{ \begin{matrix} 89 & 51 \\ 89 & 11 \end{matrix} \right\}$
$a : b$	$\left\{ \begin{matrix} 90 & 0 \\ 93 & 0 \end{matrix} \right\}$	$\begin{matrix} 91 & 3 \end{matrix}$
$c : d$	$\begin{matrix} 118 & 30 \end{matrix}$	$\left\{ \begin{matrix} 119 & 7 \\ 117 & 57 \end{matrix} \right\}$
$c : e$	$\begin{matrix} 117 & 0 \end{matrix}$	$\left\{ \begin{matrix} 117 & 38 \\ 117 & 57 \end{matrix} \right\}$
$a : d$	$\begin{matrix} 90 & 0 \end{matrix}$	
$b : o$	$\begin{matrix} 101 & 30 \end{matrix}$	$\begin{matrix} 100 & 9 \end{matrix}$
$c : o$	$\begin{matrix} 140 & 30 \end{matrix}$	
$a : o$	$\begin{matrix} 126 & 30 \end{matrix}$	
$a : e$	$\begin{matrix} 151 & 0 \end{matrix}$	$\begin{matrix} 151 & 50 \end{matrix}$
$b : d$	$\begin{matrix} 154 & 0 \end{matrix}$	$\begin{matrix} 153 & 48 \end{matrix}$
$e : o$	$\begin{matrix} 153 & 0 \end{matrix}$	
$d : o$	$\begin{matrix} 122 & 0 \end{matrix}$	
$d : i$	$\begin{matrix} 147 & 0 \end{matrix}$	
$o : i$	$\begin{matrix} 157 & 0 \end{matrix}$	
$b : i$	$\begin{matrix} 124 & 0 \end{matrix}$	
$a : i$	$\begin{matrix} 128 & 30 \end{matrix}$	
$c : i$	$\begin{matrix} 126 & 0 \end{matrix}$	



The measurements given by Professor Miller were made with a reflective goniometer constructed for measuring the angles of large crystals; those given by myself were taken with the common goniometer, for applying which the crystal is well adapted. From the nature of the crystal it is not easy to determine all its forms,

\* Communicated by the Author.

or ascertain precisely to what system they lead; but the discovery by Descloiseaux and Brooke, that leucophane shows distinctly two systems of rings\* when examined by polarized light in a direction perpendicular to the face of principal cleavage (*c*), renders the reading of this crystal a matter of less uncertainty; and I am inclined to consider that leucophane belongs to the prismatic system, having a right rectangular prism for its primitive form. This view supposes that  $ab=90^\circ$ , as well as  $cb$  and  $ac$ .

I have observed three cleavages of leucophane. One parallel to *c* is the most readily obtained; a second is apparently parallel to *i*; and the third, *t*, truncates the edge between *a* and *b*, at an angle of about  $135^\circ$  with each of those faces. The three cleavage planes are in one zone,  $ci$  being  $= 126^\circ 25'$ , and  $ct=90^\circ$ .

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LXX. *On an Analysis of the Potash and Soda Felspars of one of the Dublin Mountains.* By FRANCIS M. JENNINGS, M.R.I.A., F.G.S.†

AT the January meeting of the Geological Society, Dublin, 1855, a discussion took place on a paper, by the Rev. Joseph Galbraith, Professor of Natural and Experimental Philosophy in the University of Dublin, entitled "The Composition of the Felspars of the Granite of the Dublin and Wicklow Mountains," which appeared in the January Number of the Philosophical Magazine.

I regret much that the reverend essayist was not present, as all those who spoke, except myself, appeared to agree in the opinion expressed in the paper, namely that the granite of the Dublin and Wicklow Mountains was a potash granite, the analyses of the felspars by the author appearing conclusive to them.

I expressed doubts, not as to the correctness of the analyses, but as to the opinions of those who, from the analyses of the felspars, drew such inferences. It appeared to me a great defect in a paper drawing such important conclusions, that portions of the specimens which were analysed were not exhibited to the meeting, for it to judge if they were selected with that required discretion and care that would justify him in drawing from them the conclusions he had come to.

To ascertain the relative proportions of potash and soda of the felspar of those mountains, and thence to infer the composition of the granite, I consider it necessary that the specimens to be analysed should be selected so as to represent as closely as pos-

\* According to M. Descloiseaux, exactly resembling those exhibited by topaz.

† Communicated by the Author; having been read at the meeting of the Geological Society of Dublin, May 9, 1855.

sible, in lithological character, the mass of rock or mountains the composition of which is to be inferred from such analyses; and if they do not fulfil these conditions, I consider them worthless as far as inferring from them the constitution of the mass of the mountain.

As the opinion of those members who spoke were all against me, I therefore resolved, as soon as possible, to test by analysis, from one of those localities given by the essayist, the correctness of my views; and learning that the "Three Rock Mountain" represented in general lithological characters most nearly the granite of the Dublin and Wicklow Mountains (being too much occupied myself), I got a skilful professional chemist of high character in London, Mr. Dugald Campbell, of Quality Court, Chancery Lane, to analyse for potash and soda a portion of the granite of the "Three Rock Mountain," and took care that the specimen should be as similar as possible in general character to the granite of those mountains. Mr. Walter Wilson, of the Geological Survey, who is thoroughly conversant with the district, undertook very kindly to procure for me as fair an average specimen as he could obtain, and which accompanies this paper, *minus* that portion used for analysis.

From this analysis it will be perceived that a great difference exists between it and the analysis of the specimens of felspar from the same mountain by Professor Galbraith, as far as regards potash and soda.

*Analysis by Professor Galbraith.*

	Per cent.	Atomic quotients.	Proportionals.
Si O <sup>3</sup> . . . . .	65.40	1.444	4.19
Al <sup>2</sup> O <sup>3</sup> . . . . .	17.71	0.344	1.00
MgO . . . . .	1.77	0.088	} 0.420 1.22
CaO . . . . .	trace	0.000	
KO . . . . .	10.68	0.227	
NaO . . . . .	3.26	0.105	
Loss by ignition . . . .	0.69		
	<u>99.51</u>		

Specific gravity = 2.562.

*Analysis of the amount of Potash and Soda by Mr. Dugald Campbell.*

KO . . . . .	3.23 per cent.
NaO . . . . .	3.75 . . .

There is in this analysis of the felspar about 4 per cent. of mica, which of course increases the relative amount of potash belonging to the felspar.

Now assuming, what I have no doubt to be the case, that both the analyses are correct, how is the difference to be accounted for? Either that there is in the felspar of the mountains a great diversity of constitution, or that the felspar selected by Professor Galbraith was not similar to that composing the general mass of the mountain.

To enable him and the Society to judge as to the specimens from which the analysis was made by Mr. Campbell, it accompanies this paper; and any part not exceeding one-half is at the disposal of the Professor if he wishes to examine it.

I have since received the following letter:—

“London, May 10, 1855.

“Estimation of the potash and soda in a specimen of granite received from F. M. Jennings, Esq., of Cork, on March 16, 1855.

“In my first experiments, before reducing the mineral to a fine powder, I only removed the larger pieces of mica, and the residue gave,—

Potash . . . .	3.23 per cent.
Soda . . . .	3.75 ...

“In a second set of experiments just finished the mica was carefully picked out, scarcely a trace being left, and the residue gave,—

Potash . . . .	3.02 per cent.
Soda . . . .	3.98 ...

DUGALD CAMPBELL.”

“Francis M. Jennings.”

LXXI. *On the Porism of the In-and-circumscribed Triangle, and on an Irrational Transformation of two Ternary Quadratic Forms each into itself.* By ARTHUR CAYLEY, Esq.\*

**T**HERE is an irrational transformation of two ternary quadratic forms each into itself, based upon the solution of the following geometrical problem,

Given that the line

$$lx + my + nz = 0$$

meets the conic

$$(a, b, c, f, g, h)(x, y, z)^2 = 0$$

in the point  $(x_1, y_1, z_1)$ ; to find the other point of intersection.

The solution is exceedingly simple. Take  $(x_2, y_2, z_2)$  for the coordinates of the other point of intersection, we must have

\* Communicated by the Author.

identically with respect to  $x, y, z$ ,

$$(a \dots \chi(x, y, z))^2 < (\mathfrak{A} \dots \chi(l, m, n))^2 - k(lx + my + nz)^2 \\ = (a \dots \chi(x_1, y_1, z_1) \chi(x, y, z)) < (a \dots \chi(x_2, y_2, z_2) \chi(x, y, z))$$

to a constant factor *près*.

Assume successively  $x, y, z = \mathfrak{A}, \mathfrak{H}, \mathfrak{G}; \mathfrak{H}, \mathfrak{B}, \mathfrak{F}; \mathfrak{C}, \mathfrak{F}, \mathfrak{C}$ ; it follows that

$$x_2 : y_2 : z_2 = y_1 z_1 \{ \mathfrak{A} (\mathfrak{A} \dots \chi(l, m, n))^2 - (\mathfrak{A}l + \mathfrak{H}m + \mathfrak{G}n)^2 \} \\ : z_1 x_1 \{ \mathfrak{B} (\mathfrak{A} \dots \chi(l, m, n))^2 - (\mathfrak{H}l + \mathfrak{B}m + \mathfrak{F}n)^2 \} \\ : x_1 y_1 \{ \mathfrak{C} (\mathfrak{A} \dots \chi(l, m, n))^2 - (\mathfrak{G}l + \mathfrak{F}m + \mathfrak{C}n)^2 \};$$

or, what is the same thing,

$$x_2 : y_2 : z_2 = y_1 z_1 (bn^2 + cm^2 - 2fmn) \\ : z_1 x_1 (cl^2 + an^2 - 2gnl) \\ : x_1 y_1 (am^2 + bn^2 - 2hlm).$$

It is not necessary for the present purpose, but it may be as well to give the corresponding solution of the problem.

Given that one of the tangents through the point  $(\xi, \eta, \zeta)$  to the conic

$$(a, b, c, f, g, h) \chi(x, y, z)^2 = 0$$

is the line  $l_1 x + m_1 y + n_1 z = 0$ ; to find the equation to the other tangent.

Let  $l_2 x + m_2 y + n_2 z = 0$  be the other tangent, then

$$(a \dots \chi(\xi, \eta, \zeta))^2 (a \dots \chi(x, y, z))^2 - (a \dots \chi(\xi, \eta, \zeta) \chi(x, y, z))^2 \\ = (l_1 x + m_1 y + n_1 z) (l_2 x + m_2 y + n_2 z)$$

to a constant factor *près*. Assume successively  $y=0, z=0; z=0, x=0; x=0, y=0$ , we have

$$l_2 : m_2 : n_2 = m_1 n_1 \{ a (a \dots \chi(\xi, \eta, \zeta))^2 - (a\xi + h\eta + g\zeta)^2 \} \\ n_1 l_1 \{ b (a \dots \chi(\xi, \eta, \zeta))^2 - (h\xi + b\eta + f\zeta)^2 \} \\ l_1 m_1 \{ c (a \dots \chi(\xi, \eta, \zeta))^2 - (g\xi + f\eta + c\zeta)^2 \};$$

or, as they may be more simply written,

$$l_2 : m_2 : n_2 = m_1 n_1 (\mathfrak{B}\xi^2 + \mathfrak{C}\eta^2 + 2\mathfrak{F}\eta\xi) \\ n_1 l_1 (\mathfrak{C}\xi^2 + \mathfrak{A}\eta^2 - 2\mathfrak{G}\xi\eta) \\ l_1 m_1 (\mathfrak{A}\eta^2 + \mathfrak{B}\xi^2 - 2\mathfrak{H}\xi\eta).$$

Returning now to the solution of the first problem, I shall for the sake of simplicity consider the formulæ obtained by taking for the equation of the conic,

$$ax^2 + \beta y^2 + \gamma z^2 = 0.$$

We see, therefore, that if this conic be intersected by the line



$lx + my + nz = 0$  in the points  $(x_1, y_1, z_1)$  and  $(x_2, y_2, z_2)$ , then

$$\begin{aligned} x_2 : y_2 : z_2 &= y_1 z_1 (\gamma m^2 + \alpha n^2) \\ &: z_1 x_1 (\alpha n^2 + \beta l^2) \\ &: x_1 y_1 (\beta l^2 + \alpha m^2). \end{aligned}$$

We have, in fact, *identically*

$$\begin{aligned} &ly_1 z_1 (\beta n^2 + \gamma m^2) + mz_1 x_1 (\gamma l^2 + \alpha n^2) + nx_1 y_1 (\alpha m^2 + \beta l^2) \\ &= (\alpha mn x_1 + \beta nl y_1 + \gamma lm z_1) (lx_1 + my_1 + nz_1) \\ &\quad - lmn (\alpha x_1^2 + \beta y_1^2 + \gamma z_1^2) \\ &\alpha y_1^2 z_1^2 (\beta n^2 + \gamma m^2)^2 + \beta z_1^2 x_1^2 (\gamma l^2 + \alpha n^2)^2 + \gamma x_1^2 y_1^2 (\alpha m^2 + \beta l^2)^2 = \\ &\alpha \beta \gamma \{ -l^3 x_1^3 - m^3 y_1^3 - n^3 z_1^3 + my_1 + nz_1 l^2 x_1^2 + nz_1 + lx_1 m^2 y_1^2 \\ &\quad + lx_1 + my_1 n^2 y_1^2 - 2lmn x_1 y_1 z_1 \} (lx_1 + my_1 + nz_1) \\ &\quad - (l^4 \beta \gamma x_1^2 + m^4 \gamma \alpha y_1^2 + n^4 \alpha \beta z_1^2) (\alpha x_1^2 + \beta y_1^2 + \gamma z_1^2) = 0; \end{aligned}$$

which show that if  $lx_1 + my_1 + nz_1 = 0$  and  $\alpha x_1^2 + \beta y_1^2 + \gamma z_1^2 = 0$ , then also  $lx_2 + my_2 + nz_2 = 0$  and  $\alpha x_2^2 + \beta y_2^2 + \gamma z_2^2 = 0$ : this is, of course, as it should be.

I shall now consider  $l, m, n$  as *given functions* of  $x_1, y_1, z_1$  satisfying identically the equations

$$\begin{cases} lx_1 + my_1 + nz_1 = 0 \\ l^2 bc + m^2 ca + n^2 ab = 0, \end{cases}$$

equations which express that  $lx + my + nz = 0$  is the tangent from the point  $x_1, y_1, z_1$  to the conic  $ax^2 + by^2 + cz^2 = 0$ . And I shall take for  $\alpha, \beta, \gamma$  the following values, viz.

$$\begin{aligned} \alpha &= ax_1^2 + by_1^2 + cz_1^2 - a(x_1^2 + y_1^2 + z_1^2) \\ \beta &= ax_1^2 + by_1^2 + cz_1^2 - b(x_1^2 + y_1^2 + z_1^2) \\ \gamma &= ax_1^2 + by_1^2 + cz_1^2 - c(x_1^2 + y_1^2 + z_1^2); \end{aligned}$$

so that  $x_1, y_1, z_1$  continuing absolutely indeterminate, we have *identically*  $\alpha x_1^2 + \beta y_1^2 + \gamma z_1^2 = 0$ . Also taking  $\Theta$  as a function of  $x_1, y_1, z_1$ , the value of which will be subsequently given, I write

$$\begin{aligned} x_2 &= \Theta y_1 z_1 (\beta n^2 + \gamma m^2) \\ y_2 &= \Theta z_1 x_1 (\gamma l^2 + \alpha n^2) \\ z_2 &= \Theta x_1 y_1 (\alpha m^2 + \beta l^2); \end{aligned}$$

so that  $x_1, y_1, z_1$  are arbitrary, and  $x_2, y_2, z_2$  are taken to be determinate functions of  $x_1, y_1, z_1$ . The point  $(x_2, y_2, z_2)$  is geometrically connected with the point  $(x_1, y_1, z_1)$  as follows, viz.  $(x_2, y_2, z_2)$  is the point in which the tangent through  $(x_1, y_1, z_1)$  to the conic  $ax^2 + by^2 + cz^2 = 0$  meets the conic passing through the point  $(x_1, y_1, z_1)$  and the points of intersection of the conics

$ax^2 + by^2 + cz^2 = 0$  and  $x^2 + y^2 + z^2 = 0$ . Consequently, in the particular case in which  $(x_1, y_1, z_1)$  is a point on the conic  $x^2 + y^2 + z^2 = 0$ , the point  $(x_2, y_2, z_2)$  is the point in which this conic is met by the tangent through  $(x_1, y_1, z_1)$  to the conic  $ax^2 + by^2 + cz^2 = 0$ .

It has already been seen that  $lx_1 + my_1 + nz_1 = 0$  and  $ax_1^2 + \beta y_1^2 + \gamma z_1^2 = 0$  identically; consequently we have identically  $lx_2 + my_2 + nz_2 = 0$  and  $ax_2^2 + \beta y_2^2 + \gamma z_2^2 = 0$ . The latter equation, written under the form

$$(ax_1^2 + \beta y_1^2 + \gamma z_1^2)(x_2^2 + y_2^2 + z_2^2) - (x_1^2 + y_1^2 + z_1^2)(ax_2^2 + \beta y_2^2 + \gamma z_2^2) = 0,$$

shows that if  $x_2, y_2, z_2$  are such that  $x_2^2 + y_2^2 + z_2^2 = x_1^2 + y_1^2 + z_1^2$ , then that also  $ax_2^2 + \beta y_2^2 + \gamma z_2^2 = ax_1^2 + \beta y_1^2 + \gamma z_1^2$ . I proceed to determine  $\Theta$  so that we may have  $x_2^2 + y_2^2 + z_2^2 = x_1^2 + y_1^2 + z_1^2$ . We obtain immediately

$$\frac{1}{\Theta^2}(x_2^2 + y_2^2 + z_2^2) = (l^4x_1^2 + m^4y_1^2 + n^4z_1^2)(a^2x_1^2 + \beta^2y_1^2 + \gamma^2z_1^2) - (\alpha^2l^4x_1^4 + \beta^2m^4y_1^4 + \gamma^2n^4z_1^4 - 2\beta\gamma m^2n^2y_1^2z_1^2 - 2\gamma\alpha n^2l^2z_1^2x_1^2 - 2\alpha\beta l^2m^2x_1^2y_1^2).$$

Write for a moment  $ax_1^2 + \beta y_1^2 + \gamma z_1^2 = p$ ,  $x_1^2 + y_1^2 + z_1^2 = q$ , so that  $\alpha = p - aq$ ,  $\beta = p - bq$ ,  $\gamma = p - cq$ , then

$$\begin{aligned} \alpha^2x_1^2 + \beta^2y_1^2 + \gamma^2z_1^2 &= qp^2 - 2p^2q + (a^2x_1^2 + b^2y_1^2 + c^2z_1^2)q^2 \\ &= q\{(a^2x_1^2 + b^2y_1^2 + c^2z_1^2)q - p^2\} \\ &= q(\overline{b - c^2y_1^2z_1^2} + \overline{c - a^2z_1^2x_1^2} + \overline{a - b^2x_1^2y_1^2}), \\ \alpha^2l^4x_1^4 + \beta^2m^4y_1^4 + \gamma^2n^4z_1^4 - 2\beta\gamma m^2n^2y_1^2z_1^2 - 2\gamma\alpha n^2l^2z_1^2x_1^2 - 2\alpha\beta l^2m^2x_1^2y_1^2 \\ &= p^2(l^4x_1^4 + m^4y_1^4 + n^4z_1^4 - 2m^2n^2y_1^2z_1^2 - 2n^2l^2z_1^2x_1^2 - 2l^2m^2x_1^2y_1^2) \\ &\quad - 2pq(\overline{al^4x_1^4 + bm^4y_1^4 + cn^4z_1^4 - b + cm^2n^2y_1^2z_1^2 - c + an^2l^2z_1^2x_1^2} \\ &\quad \quad \quad - \overline{a + bl^2m^2x_1^2y_1^2}) \\ &\quad + q^2(\overline{a^2l^4x_1^4 + b^2m^4y_1^4 + c^2n^4z_1^4 - 2bcm^2n^2y_1^2z_1^2 - 2can^2l^2z_1^2x_1^2} \\ &\quad \quad \quad - \overline{2abl^2m^2x_1^2y_1^2}), \end{aligned}$$

the first line of which vanishes in virtue of the equation  $lx_1 + my_1 + nz_1 = 0$ , we have therefore

$$\begin{aligned} \frac{1}{\Theta^2}(x_2^2 + y_2^2 + z_2^2) \div (x_1^2 + y_1^2 + z_1^2) &= \\ (l^4x_1^2 + m^4y_1^2 + n^4z_1^2)(\overline{b - c^2y_1^2z_1^2} + \overline{c - a^2z_1^2x_1^2} + \overline{a - b^2x_1^2y_1^2}) &+ \\ + 2(ax_1^2 + \beta y_1^2 + \gamma z_1^2)(\overline{al^4x_1^4 + bm^4y_1^4 + cn^4z_1^4 - b + cm^2n^2y_1^2z_1^2} &- \\ - \overline{c + an^2l^2z_1^2x_1^2 - a + bl^2m^2x_1^2y_1^2}) & \\ - (x_1^2 + y_1^2 + z_1^2)(\overline{a^2l^4x_1^4 + b^2m^4y_1^4 + c^2n^4z_1^4 - 2bcm^2n^2y_1^2z_1^2} &- \\ - \overline{2can^2l^2z_1^2x_1^2 - 2abl^2m^2x_1^2y_1^2}). & \end{aligned}$$

Hence reducing the function on the right-hand side, and putting  $x_2^2 + y_2^2 + z_2^2 \div x_1^2 + y_1^2 + z_1^2 = 1$ , we have

$$\frac{1}{\Theta^2} = a^2 l^4 x_1^6 + b^2 m^4 y_1^6 + c^2 n^4 z_1^6$$

$$+ (c^2 m^4 - 2b^2 m^2 n^2) y_1^4 z_1^2 + (a^2 n^4 - 2c^2 n^2 l^2) z_1^4 x_1^2 + (b^2 l^4 - 2a^2 l^2 m^2) x_1^4 y_1^2$$

$$+ (b^2 n^4 - 2c^2 m^2 n^2) y_1^2 z_1^4 + (c^2 l^4 - 2a^2 n^2 l^2) z_1^2 x_1^4 + (a^2 m^4 - 2b^2 l^2 m^2) x_1^2 y_1^4$$

$$+ \{ l^4 (b-c)^2 + m^4 (c-a)^2 + n^4 (a-b)^2$$

$$+ 2m^2 n^2 (bc - ca - ab) + 2n^2 l^2 (-bc + ca - ab)$$

$$+ 2l^2 m^2 (-bc - ca + ab) \} x_1^2 y_1^2 z_1^2$$

The value of  $\Theta$  might probably be expressed in a more simple form by means of the equations  $lx_1 + my_1 + nz_1 = 0$  and  $l^2 bc + m^2 ca + n^2 ab = 0$ , even without solving these equations; but this I shall not at present inquire into.

Recapitulating,  $l, m, n$  are considered as functions of  $x_1, y_1, z_1$  determined (to a common factor *près*) by the equations

$$lx_1 + my_1 + nz_1 = 0$$

$$l^2 bc + m^2 ca + n^2 ab = 0.$$

$\Theta$  is determined as above, and then writing

$$\alpha = ax_1^2 + by_1^2 + cz_1^2 - a(x_1^2 + y_1^2 + z_1^2)$$

$$\beta = ax_1^2 + by_1^2 + cz_1^2 - b(x_1^2 + y_1^2 + z_1^2)$$

$$\gamma = ax_1^2 + by_1^2 + cz_1^2 - c(x_1^2 + y_1^2 + z_1^2),$$

we have

$$x_2 = \Theta y_1 z_1 (\beta n^2 + \gamma m^2)$$

$$y_2 = \Theta z_1 x_1 (\gamma l^2 + \alpha n^2)$$

$$z_2 = \Theta x_1 y_1 (\alpha m^2 + \beta l^2).$$

And these values give

$$lx_2 + my_2 + nz_2 = 0$$

$$x_2^2 + y_2^2 + z_2^2 = x_1^2 + y_1^2 + z_1^2$$

$$ax_2^2 + by_2^2 + cz_2^2 = ax_1^2 + by_1^2 + cz_1^2.$$

In connexion with the subject I may add the following transformation, viz. if

$$3 \sqrt{\alpha x} = \sqrt{3\beta}(y-z) + \sqrt{(3\alpha - 2\beta)(x^2 + y^2 + z^2) + 2\beta(yz + zx + xy)}$$

Then reciprocally

$$3 \sqrt{\beta x} = -\sqrt{3\alpha}(y' - z') + \sqrt{(3\beta - 2\alpha)(x'^2 + y'^2 + z'^2) + 2\alpha(y'z' + z'x' + x'y')}.$$

Also

$$x^2 + y^2 + z^2 = x'^2 + y'^2 + z'^2$$

$$\beta(x^2 + y^2 + z^2 - yz - zx - xy) = \alpha(x'^2 + y'^2 + z'^2 - y'z' - z'x' - x'y').$$

Suppose  $1 + \rho + \rho^2 = 0$ , then

$$x^2 + y^2 + z^2 - yz - zx - xy = (x + \rho y + \rho^2 z)(x + \rho^2 y + \rho z);$$

and in fact

$$3 \sqrt{\alpha}(x' + \rho y' + \rho^2 z') = - \sqrt{3\beta}(1 + 2\rho)(x + \rho y + \rho^2 z)$$

$$3 \sqrt{\alpha}(x' + \rho^2 y' + \rho z) = \sqrt{3\beta}(1 + 2\rho)(x + \rho^2 y + \rho z).$$

The preceding investigations have been in my possession for about eighteen months.

2 Stone Buildings,  
April 18, 1855.

LXXII. *Contributions to our Knowledge of the Nature of the so-called Coercive Force.* By Professor PLÜCKER\*.

1. **I**N a former memoir I proved that the magnetism excited by paramagnetic or diamagnetic induction in various substances is not proportional to the inducing force, but in each particular substance, according to a peculiar law, approaches a maximum which is the point of saturation of the substance. I have shown empirically, that for each of the substances examined by me, the induced magnetism, after assuming the unit of inducing force, could be determined by means of two other constants which are independent of each other. One of these constants I have named the *constant of induction*, and the other the *constant of resistance*†. If it should be found that the constant of resistance in any substance is zero, then for this substance the induced magnetism would be proportional to the inducing force, and the receptivity of the substance for magnetism would be shown by the constant of induction alone. The induced magnetism of the substances examined by me (nickel, cobalt, iron, oxide of iron, oxide of nickel, bismuth, phosphorus, oxygen, hydrated oxide of cobalt) always falls short, in different degrees, of the proportionality mentioned; and the more so, the greater is the constant of resistance. The few observations made at the time rendered it to me probable that for those substances which, after the inducing force to which they were exposed had ceased to act upon them, still retained a considerable portion of their magnetism, the constant of resistance was comparatively small; so that the magnetism of such substances, when the inducing force is augmented, increases more quickly, and more slowly approaches the point of saturation. As far as regards hard steel in comparison with iron, I could not doubt the correctness of

\* Extract from a paper in Poggendorff's *Annalen*, vol. xciv. p. 28.

† Some other term might have been chosen with advantage.—ED. PHIL. MAG.

this conclusion. This was proved by a simple experiment which I mentioned several years ago, and which showed that two bars, a thick one of iron and a thin one of steel, formed into a cross and suspended horizontally before the pole of a magnet, were directed at a distance by the bar of iron, but near to the magnet by the bar of steel. Numbers, however, were wanting which I could not obtain from this experiment. The solid bodies examined by me in the memoir referred to were all in the form of powder. It appeared to me therefore very desirable to bring the above question to the test of experiment, operating in a manner which permitted of the examination of iron and steel in a massive form.

2. For this purpose I had prepared a button of iron (A), and three buttons of steel (B, C, D), as nearly as possible alike in shape and form; the three latter were made as hard as possible, and afterwards the button B was tempered to blue, the button C to yellow, while the button D retained its full hardness. These three buttons were cut from the same bar of fine English steel, and were rounded hemispherically and polished at the ends. Their thickness was 8 millims.; their length, excluding a small hook for suspension at the other end, was 14 millims.; their weight about 6.3 grms. From the same bar of steel, still smaller bars E were cut 53 millims. long, and also hardened and rounded at the ends. The button A was formed from a bar of iron of equal thickness; and again from the same bar, a smaller one F was taken, equal in size and form to the steel bar E.

3. Five series of weighings were undertaken in order to determine the weights which were necessary to separate the different buttons from the inducing magnetic pole. For the three first series of weighings, three of the steel bars E were taken and carefully magnetized in different degrees. These bars were then successively fixed vertically by screws upon a foot; upon them the different buttons were caused to rest, and the weight which was necessary to effect their separation was determined. In the first series of weighings, in which the button was attracted by the bar which was most feebly magnetized, I made use of a fine Geissler's balance, and the weight was determined by a rider of platinum which was moved along the arm of a lever. In the second series, in which the different buttons were attracted by the bar of medium magnetic strength, the same balance was made use of, but the amount of the attraction was determined by pouring dry sand out of a small orifice upon the scale-pan at the opposite end of the balance-beam. The sand after each determination was weighed with another balance. In the third series of experiments, the glass balance of Geissler had to be changed for a larger chemical balance; the determinations were

made in the same manner as in the second series, only that weights were first laid on before the sand was added.

In the last two series of weighings the large electro-magnet was made use of; a very blunt cone, whose base was of the same diameter as the electro-magnet, and whose point was rounded, being placed upon the flat pole of the magnet. The buttons were hung upon one of the arms of a common balance, and after partially balancing the attraction by weights placed upon the opposite scale-pan, the separation was finally effected by the addition of small grains of shot. The electro-magnet was at one time excited by a single cell of Grove, and again by a battery of six cells.

#### 4. *First Series of Experiments.*

The numbers express the weights in grammes necessary to produce the separation; those side by side are the weighings of the same button, repeated immediately one after the other. The succession in which the different buttons were weighed is stated in the first vertical column, the substance of the button used in each case being indicated.

	grm.	grm.
Soft iron . . . .	0·395	0·395
Blue steel . . . .	0·33	0·34
Yellow steel . . . .	0·26	0·27
Glass hard steel . . . .	0·120	0·125
Yellow steel . . . .	0·26	0·27
Blue steel . . . .	0·35	0·34
Soft iron . . . .	0·415	0·405

#### 5. *Second Series.*

	grms.	grms.
Soft iron . . . .	2·60	2·70
Blue steel . . . .	2·10	2·20
Yellow steel . . . .	1·80	1·80
Glass hard steel . . . .	1·00	1·00
Yellow steel . . . .	1·70	1·60
Blue steel . . . .	2·00	2·10
Soft iron . . . .	2·55	2·65

#### 6. *Third Series.*

The attraction of the different buttons by the strongest of the three steel magnets was determined twice on different days.

	grms.	grms.	grms.	grms.
Soft iron . . . .	35·8	36·0	35·9	36·4
Blue steel . . . .	29·8	29·9	30·7	29·2
Yellow steel . . . .	25·4	25·8	25·8	25·0
Glass hard steel . . . .	18·0	18·1	19·0	18·5

	grms.	grms.	grms.	grms.
Soft iron . . .	34.0	34.9	35.0	
Blue steel . . .	29.2	29.8	30.2	
Yellow steel . . .	24.2	24.7	24.1	
Glass hard steel . . .	17.9	18.2	18.6	
Soft iron . . .	35.2	34.9	36.0	34.5

7. Fourth Series.

Soft iron . . . . .	205 +	grms. {	131
			131
			127
			124
Blue steel . . . . .	295 +	{	3
			2
			2.5
Yellow steel . . . . .	255 +	{	7
			5
			6
			2.5
Glass hard steel . . . . .	250 +	{	2.3
			3.0
			5.8
			7.6
Soft iron . . . . .	{	225 +	4.5
			4.7
		205 +	22
			25

8. Fifth Series.

Soft iron . . . . .	1553 +	grms. {	21
			18
			19
			24
Blue steel . . . . .	1423 +	{	9
			12
			20
Yellow steel . . . . .	1233 +	{	6.5
			6.5
			4
			15.5
Glass hard steel . . . . .	1133 +	{	14.5
			18.5
Soft iron . . . . .	1543 +	{	28
Yellow steel . . . . .	1233 +	{	16
			16.5

In the fourth and fifth series, the greater numbers which stand forward denote the weight not yet sufficient to effect separation, and the smaller numbers behind the brackets denote the weight of shot which was added.

9. Taking the mean values of the single determinations of each series we obtain the following result:—

	Glass hard steel. grms.	Yellow steel. grms.	Blue steel. grms.	Soft iron. grms.
I.	0.1225	0.268	0.340	0.405
II.	1.1000	1.725	2.210	2.625
III.	{ 18.4 18.2	{ 25.0 24.3	{ 29.9 29.7	{ 36.0 34.6
IV.	233	261	298	331
V.	1149	1245	1438	1572

Setting the attraction of the button of glass hard steel equal unity, we obtain for the attraction of the three other buttons the following proportionate numbers:—

	Yellow steel. grms.	Blue steel. grms.	Soft iron. grms.
I.	2.18	2.78	3.31
II.	1.72	2.21	2.62
III.	{ 1.36 1.34	{ 1.63 1.63	{ 1.96 1.90
IV.	1.12	1.28	1.42
V.	1.084	1.25	1.37

10. This tabular statement shows, as was to be expected, in a striking manner, that the harder the steel is, the more feebly it is magnetized; but that with increasing force, its magnetism increases in a so much quicker ratio. We see that when high power is applied, the yellow steel is not much more strongly attracted than the glass hard, but is more than twice as strongly attracted when the force is feeble; and that the magnetic department of the blue steel forms the transition to the attraction of the soft iron. By the feeblest of the forces applied this is attracted  $3\frac{2}{3}$  times, by the highest power only  $1\frac{1}{3}$  time more strongly than the glass hard steel\*.

\* This result seems to be an important contribution towards a physical theory of magnetism. The word "resistance" appears to us unhappily chosen by M. Plücker; in the case of iron it is an *exhaustion* of the quality on which its attraction depends, rather than a resistance to further magnetization, which seems to be indicated by the experiments.—J. T.



LXXIII. *On the Dynamical Theory of Heat.*—Part V. *On the Quantities of Mechanical Energy contained in a Fluid in Different States, as to Temperature and Density.* By WILLIAM THOMSON, M.A., Professor of Natural Philosophy in the University of Glasgow\*.

[Continued from vol. iv. p. 434.]

81. **A** BODY which is either emitting heat, or altering its dimensions against resisting forces, is doing work upon matter external to it. The mechanical effect of this work in one case is the excitation of thermal motions, and in the other the overcoming of resistances. The body must itself be altering in its circumstances, so as to contain a less store of work within it by an amount precisely equal to the aggregate value of the mechanical effects produced; and conversely, the aggregate value of the mechanical effects produced must depend solely on the initial and final states of the body, and is therefore the same whatever be the intermediate states through which the body passes, provided the *initial* and *final* states be the same.

82. The total mechanical energy of a body might be defined as the mechanical value of all the effect it would produce in heat emitted and in resistances overcome, if it were cooled to the utmost, and allowed to contract indefinitely or to expand indefinitely according as the forces between its particles are attractive or repulsive, when the thermal motions within it are all stopped; but in our present state of ignorance regarding perfect cold, and the nature of molecular forces, we cannot determine this "total mechanical energy" for any portion of matter, nor even can we be sure that it is not infinitely great for a finite portion of matter. Hence it is convenient to choose a certain state as standard for the body under consideration, and to use the unqualified term, *mechanical energy*, with reference to this standard state; so that the "mechanical energy of a body in a given state" will denote the mechanical value of the effects the body would produce in passing from the state in which it is given, to the standard state, or the mechanical value of the whole agency that would be required to bring the body from the standard state to the state in which it is given.

83. In the present communication, a system of formulæ founded on propositions established in the first part of my paper on the Dynamical Theory of Heat, and expressing relations between the pressure of a fluid, and the thermal capacities and mechanical energy of a given mass of it, all considered as functions of the temperature and volume; and Carnot's function of

\* From the Transactions of the Royal Society of Edinburgh, vol. xx. part 3; read December 15, 1851.

the temperature ; are brought forward for the purpose of pointing out the importance of making the *mechanical energy* of a fluid in different states an object of research, along with the other elements which have hitherto been considered, and partially investigated in some cases.

84. If we consider the circumstances of a stated quantity (a unit of matter, a pound, for instance) of a fluid, we find that its condition, whether it be wholly in the liquid state or wholly gaseous, or partly liquid and partly gaseous, is completely defined, when its temperature, and the volume of the space within which it is contained, are specified (§§ 20, 53, ... 56), it being understood, of course, that the dimensions of this space are so limited that no sensible differences of density in different parts of the fluid are produced by gravity. We shall therefore consider the temperature, and the volume of unity of mass, of a fluid, as the independent variables of which its pressure, thermal capacities, and mechanical energy are functions. The volume and temperature being denoted respectively by  $v$  and  $t$ , let  $e$  be the mechanical energy,  $p$  the pressure,  $K$  the thermal capacity under constant pressure, and  $N$  the thermal capacity in constant volume ; and let  $M$  be such a function of these elements, that

$$K = N + \frac{\frac{dp}{dt}}{-\frac{dp}{dv}} M \dots \dots \dots (1),$$

or (§§ 48, 20), such a quantity that

$$Mdv + Ndt \dots \dots \dots (2),$$

may express the quantity of heat that must be added to the fluid mass, to elevate its temperature by  $dt$ , when its volume is augmented by  $dv$ .

85. The mechanical value of the heat added to the fluid in any operation, or the quantity of heat added, multiplied by  $J$  (the mechanical equivalent of the thermal unit), must be diminished by the work done by the fluid in expanding against resistance, to find the actual increase of mechanical energy which the body acquires. Hence ( $de$  of course denoting the complete increment of  $e$ , when  $v$  and  $t$  are increased by  $dv$  and  $dt$ ) we have

$$de = J(Mdv + Ndt) - pdv \dots \dots \dots (3).$$

Hence, according to the usual notation for partial differential coefficients, we have

$$\frac{de}{dv} = JM - p \dots \dots \dots (4),$$

$$\frac{de}{dt} = JN \dots \dots \dots (5).$$

Lastly, if we denote by  $\mu$ , as formerly, Carnot's function of the temperature  $t$ , we have (§ 21)

$$\frac{dp}{dt} = \mu M \quad \dots \dots \dots (6).$$

86. The use that may be made of these formulæ in investigations regarding the physical properties of any particular fluid must depend on the extent and accuracy of the general data belonging to the theory of the mechanical action of heat that are available. Thus, if nothing be known by experiment regarding the values of  $J$  and  $\mu$ , we may, in the first place, use equations (4) and (5), or the following deduced from them (§ 20), by eliminating  $e$ ,

$$\frac{dp}{dt} = J \left( \frac{dM}{dt} - \frac{dN}{dv} \right) \quad \dots \dots \dots (7),$$

and equation (6), as tests of the accuracy of experimental researches on the pressure and thermal capacities of a fluid, on account of the knowledge we have from theory that  $J$  is *certainly* an absolute constant, and that in all probability, if not with absolute certainty, we may regard  $\mu$  as independent of  $v$ , and as the same for all fluids at the same temperature; and with experimental data of sufficient extent, we may use these equations as means of actually determining the values of  $J$  and  $\mu$ . No other way than this has yet been attempted for determining  $\mu$ ; and if we except a conceivable, but certainly not at present practicable mode of determining this element by experiments on thermo-electric currents, no other way is yet known. Carnot's original determination of  $\mu$  was effected by means of an expression equivalent to that of equation (6) applied to the case of a mass of air; and the determinations by Clapeyron, and those shown in Table I. of my 'Account of Carnot's Theory,' were calculated by the formula which is obtained when the same equation is applied to the case of a fluid mass, partly liquid and partly in the state of saturated vapour (§ 55).

87. As yet experiments have not been made on the pressure and thermal capacities of fluids to a sufficient extent to supply data for the evaluation, even in the roughest manner, of the expression given for  $J$  by equation (7); and it may be doubted whether such data can even be had with accuracy enough to give as exact a determination of this important element as may be effected by direct experiments on the generation of heat by means of friction. At present we may regard  $J$  as known, probably within  $\frac{1}{3000}$  of its own amount, by experiments of this kind.

88. The value of  $J$  being known, equations (4) and (5) may be used for determining the mechanical energy of a particular fluid mass in different states, from special experimental data

regarding its pressure and thermal capacities, but not necessarily comprehending the values of each of these elements for all states of the fluid. The theory of the integration of functions of two independent variables will, when any set of data are proposed, make it manifest whether or not they are sufficient, and will point out the methods, whether of summation or of analytical integration, according to the forms in which the data are furnished, to be followed for determining the value of  $e$  for every value of  $v$ . Or the data may be such, that while the thermal capacities would be derived from them by differentiation, values of  $e$  may be obtained from them without integration. Thus, if the fluid mass consist of water and vapour of water at the temperature  $t$ , weighing in all one pound, and occupying the volume  $v^*$ , and if we regard the *zero* or "standard" state of the mass as being liquid water at the temperature  $0^\circ$ , the mechanical energy of the mass in the given state will be the mechanical value of the heat required to raise the temperature of a pound of water from  $0^\circ$  to  $t$ , and to convert  $\frac{v-\lambda}{\gamma-\lambda}$  of it into vapour, diminished by the work done in the expansion from the volume  $\lambda$  to the volume  $v$ ; that is, we have

$$e = J \left( ct + L \frac{v-\lambda}{\gamma-\lambda} \right) - p(v-\lambda) \dots \dots (8).$$

The variables  $c$ ,  $L$ , and  $p$  (which depend on  $t$  alone) in this expression have been experimentally determined by Regnault for all temperatures from  $0^\circ$  to  $230^\circ$ ; and when  $\gamma$  is also determined by experiments on the density of saturated steam, the elements for the determination of  $e$  in this case will be complete. The expressions investigated formerly for  $M$  and  $N$  in this case (§ 54) may be readily obtained by means of (4) and (5) of § 85, by the differentiation of (8).

89. If Carnot's function has once been determined by means of observations of any kind, whether on a single fluid or on different fluids, for a certain range of temperatures, then according

to (6) of § 85, the value of  $\frac{dp}{M dt}$  for any substance whatever is

\* The same notation is used here as formerly in § 54, viz.  $p$  is the pressure of saturated vapour at the temperature  $t$ ,  $\gamma$  the volume, and  $L$  the latent heat of a pound of the vapour,  $\lambda$  the volume of a pound of liquid water, and  $c$  the mean thermal capacity of a pound of water between the temperatures  $0$  and  $t$ . A mass weighing a pound, and occupying the volume  $v$ , when at the temperature  $t$ , must consist of a weight  $\frac{v-\lambda}{\gamma-\lambda}$  of vapour, and  $\frac{\gamma-v}{\gamma-\lambda}$  of water.

known for all temperatures within that range. It follows that when the values of  $M$  for different states of a fluid have been determined experimentally, the law of pressures for all temperatures and volumes (with an arbitrary function of  $v$  to be determined by experiments on the pressure of the fluid at one particular temperature) may be deduced by means of equation (6); or conversely, which is more likely to be the case for any particular fluid, if the law of pressures is completely known,  $M$  may be deduced without further experimenting. Hence the second member of (4) becomes completely known, the equation assuming the following form, when, for  $M$ , its value according to (6) is substituted:—

$$\frac{de}{dv} = \frac{J}{\mu} \frac{dp}{dt} - p \quad . \quad . \quad . \quad . \quad . \quad (8).$$

The integration of this equation with reference to  $v$  leads to an expression for  $e$ , involving an arbitrary function of  $t$ , for the determination of which more data from experiment are required. It would, for instance, be sufficient for this purpose to have the mechanical energy of the fluid for all temperatures when contained in a constant volume; or, what amounts to the same (it being now supposed that  $J$  is known), to have the thermal capacity of the fluid in constant volume for a particular volume and all temperatures. Hence we conclude, that when the elements  $J$  and  $\mu$  belonging to the general theory of the mechanical action of heat are known, the mechanical energy of a particular fluid may be investigated without experiment, from determinations of its pressure for all temperatures and volumes, and its thermal capacity for any particular constant volume and all temperatures.

90. For example, let the fluid be atmospheric air, or any other subject to the "gaseous" laws. Then if  $v_0$  be the volume of a unit of weight of the fluid, and  $0$  the temperature, in the standard state from which the mechanical energy in any other state is reckoned, and if  $p_0$  denote the corresponding pressure, we have

$$p = \frac{p_0 v_0}{v} (1 + Et), \quad \frac{dp}{dt} = \frac{p_0 v_0 E}{v},$$

and

$$\int_{v_0}^v \left( \frac{J}{\mu} \frac{dp}{dt} - p \right) dv = p_0 v_0 \left\{ \frac{JE}{\mu} - (1 + Et) \right\} \log \frac{v}{v_0}.$$

Hence if we denote by  $N_0$  the value of  $N$  when  $v = v_0$ , whatever be the temperature, we have as the general expression for the mechanical energy of a unit weight of a fluid subject to the gaseous laws,

$$e = p_0 v_0 \left\{ \frac{JE}{\mu} - (1 + Et) \right\} \log \frac{v}{v_0} + J \int_0^t N_0 dt \quad . \quad . \quad (9).$$

91. Let us now suppose the mechanical energy of a particular fluid mass in various states to have been determined in any way, and let us find what results regarding its pressure and thermal capacities may be deduced. In the first place, by integrating equation (8), considered as a differential equation with reference to  $t$  for  $p$ , we find

$$p = \epsilon^{\frac{1}{J} \int_0^t \mu dt} \int_0^t \mu \frac{de}{dv} \epsilon^{-\frac{1}{J} \int_0^t \mu dt} dt + \psi(v) \epsilon^{\frac{1}{J} \int_0^t \mu dt} \dots (10),$$

where  $\psi(v)$  denotes a constant with reference to  $t$ , which may vary with  $v$ , and cannot be determined without experiment. Again, we have from (5), (4), and (1),

$$\left. \begin{aligned} N &= \frac{1}{J} \frac{de}{dt} \\ K &= \frac{1}{J} \frac{de}{dt} + \frac{1}{J} \left( \frac{de}{dv} + p \right) \frac{\frac{dp}{dt}}{-\frac{dp}{dv}} \end{aligned} \right\} \dots (11).$$

From the first of these equations we infer, that, with a complete knowledge of the mechanical energy of a particular fluid, we have enough of data for determining for every state its thermal capacity in constant volume. From equation (9) we infer, that with, besides, a knowledge of the pressure for all volumes and a particular temperature, or for all volumes and a particular series of temperatures, we have enough to determine completely the pressure, and consequently also, according to equation (11), to determine the two thermal capacities, for all states of the fluid.

92. For example, let these equations be applied to the case of a fluid subject to the gaseous laws. If we use for  $\frac{de}{dv}$  its value derived from (9), in equation (10), we find

$$p = \frac{p_0 v_0}{v} (1 + Et) + \chi(v) \epsilon^{\frac{1}{J} \int \mu dt} \dots (12),$$

where  $\chi(v)$ , denoting an arbitrary function of  $v$ , is used instead of  $\psi(v) - \frac{p_0 v_0}{v}$ . We conclude that the same expression for the mechanical energy holds for any fluid whose pressure is expressed by this equation, as for one subject to the gaseous laws. Again, by using for  $\frac{de}{dt}$  and  $\frac{de}{dv}$ , their values derived from (9), in equation (11), we have

$$N = N_0 + \frac{1}{J} p_0 v_0 \log \frac{v}{v_0} \frac{d \left\{ \frac{JE}{\mu} - (1 + Et) \right\}}{dt} \dots \dots (13),$$

$$K = N_0 + \frac{1}{J} p_0 v_0 \log \frac{v}{v_0} \frac{d \left\{ \frac{JE}{\mu} - (1 + Et) \right\}}{dt} + \frac{E^2 p_0 v_0}{\mu(1 + Et)} \quad (14).$$

The first of these equations shows, that unless Mayer's hypothesis be true, there is a difference in the thermal capacities in constant volume, of the same gas at the same temperatures for different densities, proportional in amount to the difference of the logarithms of the densities. The second, compared with the first, leads to an expression for the difference between the thermal capacities of a gas in constant volume, and under constant pressure, agreeing with results arrived at formerly. [Account of Carnot's Theory, Appendix III.; and Dynamical Theory of Heat, § 48.]

93. It may be that more or less information, regarding explicitly the pressure and thermal capacities of the fluid, may have been had as the data for determining the mechanical energy; but these converse deductions are still interesting, as showing how much information regarding its physical properties is comprehended in a knowledge of the mechanical energy of a fluid mass, and how useful a table of the values of this function for different temperatures and volumes, or an empirical function of two variables expressing it, would be, whatever be the experimental data from which it is deduced. It is not improbable that such a table or empirical function, and a similar representation of the pressure, may be found to be the most convenient expression for results of complete observations on the compressibility, the law of expansion by heat, and the thermal capacities of a vapour or gas.

94. The principles brought forward in a former communication "On a Means of discovering experimentally, &c." (which is now referred to as Part IV. of a series of papers on the Dynamical Theory of Heat), may be expressed in a more convenient and in a somewhat more comprehensive manner than in the formulæ contained in that paper, by introducing the notations and principles which form the subject of the present communication. Thus, let  $t$  be the temperature, and  $u$  the volume of a pound of air flowing gently in a pipe (under very high pressure it may be) towards a very narrow passage (a nearly closed stop-cock, for instance), and let  $p$  be its pressure. Let  $t'$ ,  $u'$ , and  $p'$  be the corresponding qualities of the air flowing gently through a continuation of the pipe, after having passed the "rapids" in

and near the narrow passage. Let  $Q$  be the quantity of heat (which, according to circumstances, may be positive, zero, or negative) emitted by a pound of air during its whole passage from the former locality through the narrow passage to the latter; and let  $S$  denote the mechanical value of the sound emitted from the "rapids." The only other external mechanical effect besides these two produced by the air, is the excess (which, according to circumstances, may be negative, zero, or positive) of the work done by the air in pressing out through the second part of the pipe above that spent in pressing it in through the first; the amount of which, for each pound of air that passes, is of course  $p'u' - pu$ . Hence the whole mechanical value of the effects produced externally by each pound of the air from its own mechanical energy is

$$JQ + S + p'u' - pu \quad . \quad . \quad . \quad (15).$$

Hence if  $\phi(v, t)$  denote the value of  $e$  expressed as a function of the independent variables  $v$  and  $t$ , so that  $\phi(u, t)$  may express the mechanical energy of a pound of air before, and  $\phi(u', t')$  the mechanical energy of a pound of air after passing the rapids, we have

$$\phi(u', t') = \phi(v, t) - \{JQ + S + p'u' - pu\} \quad . \quad . \quad (16).$$

95. If the circumstances be arranged (as is always possible) so as to prevent the air from experiencing either gain or loss of heat by conduction through the pipe and stopcock, we shall have  $Q=0$ ; and if (as is perhaps also possible) only a mechanically inappreciable amount of sound be allowed to escape, we may take  $S=0$ . Then the preceding equation becomes

$$\phi(u', t') = \phi(u, t) - (p'u' - pu) \quad . \quad . \quad (17).$$

If by experimenting in such circumstances it be found that  $t'$  does not differ sensibly from  $t$ , Mayer's hypothesis is verified for air at the temperature  $t$ ; and as  $p'u'$  would then be equal to  $pu$ , according to Boyle and Mariotte's law, we should have

$$\phi(u', t) = \phi(u, t),$$

which is, in fact, the expression of Mayer's hypothesis, in terms of the notation for mechanical energy introduced in this paper. If, on the other hand,  $t'$  be found to differ from  $t^*$ , let values of

\* If the values of  $\mu$  I have used formerly be correct,  $t'$  would be less than  $t$  for all cases in which  $t$  is lower than about  $30^\circ$  Cent.; but, on the contrary, if  $t$  be considerably above  $30^\circ$  Cent.,  $t'$  would be found to exceed  $t$ . (See 'Account of Carnot's Theory,' Appendix II.) It may be shown, that if they are correct, air at the temperature  $0^\circ$  forced up with a pressure of ten atmospheres towards a small orifice, and expanding through it to the atmospheric pressure, would go down in temperature by about  $4^\circ.4$ ; but that if it had the temperature of  $100^\circ$  in approaching the orifice, it would leave at a temperature about  $5^\circ.2$  higher, provided that in each case there is no appreciable expenditure of mechanical energy on sound.



$p, p', t,$  and  $t'$  be observed in various experiments of this kind, and, from the known laws of density of air, let  $u$  and  $u'$  be calculated. We then have, by an application of (13) to the results of each experiment, an equation showing the difference between the mechanical energies of a pound of air in two particular specified states as to temperature and density. All the particular equations thus obtained may be used towards forming, or for correcting, a table of the values of the mechanical energy of a mass of air at various temperatures and densities.

96. If, according to the plan proposed in my former communication (§ 72), the air on leaving the narrow passage be made to pass through a spiral pipe immersed in water in a calorimetric apparatus, and be so brought back exactly to the primitive temperature  $t$ , we should have, according to Boyle's and Mariotte's law,  $p'u' - pu = 0$ ; and if  $H$  denote the value of  $Q$  in this particular case (or the quantity of heat measured by means of the calorimetric apparatus), the general equation (16) takes the form

$$\phi(u', t) = \phi(u, t) - (JH + S) \dots (18).$$

If in this we neglect  $S$ , as probably insensible, and if we substitute for  $\phi(u, t)$  and  $\phi(u', t)$  expressions deduced from (9), we find

$$H = \left\{ \frac{1}{J} - \frac{E}{\mu(1 + Et)} \right\} pu \log \frac{u'}{u} \dots (19),$$

which agrees exactly with the expression obtained by a synthetical process, founded on the same principles, in my former communication (§ 76).

LXXIV. *On the Electro-static Capacity of a Leyden Phial and of a Telegraph Wire insulated in the axis of a cylindrical Conducting Sheath.* By Professor W. THOMSON\*.

THE principles brought forward in the preceding articles On the Uniform Motion of Heat, &c., enable us with great ease to investigate the "capacity†" of a Leyden phial with either air,

\* Communicated as an Additional Note to two papers "On the Uniform Motion of Heat in Homogeneous Solid Bodies, and its connexion with the Mathematical Theory of Electricity," and "On the Mathematical Theory of Electricity in Equilibrium;" only not in time to be appended to the reprints of those papers which appeared in the Philosophical Magazine, vol. vii. p. 502, June 1854, and vol. viii. p. 42, July 1854.

† Defined (Phil. Mag. June 1853) for any conductor (subject or not to the influence of other conductors), as the quantity of electricity which it takes to charge it to unit potential.

or any liquid or solid dielectric, and of other analogous arrangements, such as the copper wires in gutta-percha tubes under water, with which Faraday has recently performed such remarkable experiments\*.

Thus, for a Leyden phial; let us suppose a portion  $S$  of the surface of a conductor  $A$  to be everywhere so near the surface of a conductor  $A'$ , that the distance between them at any point is a small fraction of the radii of curvature of each surface in the neighbourhood; and let  $z$  be the distance between them at a particular position,  $P$ . Then, by the analogy with heat, it is clear that if the two surfaces be kept at different electrical potentials,  $V$  and  $V'$ , the potentials at equidistant points in any line across from one to the other will be in arithmetical progression.

Hence  $\frac{V-V'}{z}$  will be the rate of variation of the potential perpendicularly across in the position  $P$ . If, in the first place, the dielectric be air, the electric force in the air between the two about the position  $P$  will consequently be  $\frac{V-V'}{z}$ , and therefore the electrical density (according to the theorem proved in the first article) on one surface must be  $+\frac{1}{4\pi} \frac{V-V'}{z}$ , and on the other  $-\frac{1}{4\pi} \frac{V-V'}{z}$ . The quantity of electricity in the position  $P$ , on an area  $ds$  of the surface  $S$ , is therefore  $\frac{1}{4\pi} \frac{V-V'}{z} ds$ , and therefore the whole quantity on  $S$  is

$$\frac{V-V'}{4\pi} \int \frac{ds}{z},$$

which is Green's general expression for the electrification of either coating of a Leyden phial. If the thickness of the dielectric be constant and equal to  $\tau$ , it becomes

$$\frac{V-V'}{4\pi} \frac{S}{\tau}.$$

Now if  $A'$  be uninsulated, we have  $V'=0$ ; and then, to charge  $S$  to the potential  $V$ , it takes the quantity  $V \times \frac{S}{4\pi\tau}$ . Hence the "capacity" of  $S$  is

$$\frac{S}{4\pi\tau}.$$

If instead of air there be a solid or liquid dielectric of inductive capacity,  $k$ , occupying the space between the two surfaces, the

\* Described in a lecture at the Royal Institution, Jan. 20, 1854, and subsequently published in vol. vii. p. 197 of the *Philosophical Magazine*.

quantity of heat conducted across, in the analogous thermal circumstances, would be  $k$  times as great as in the case corresponding to the air dielectric, with the same difference of temperatures; and in the actual electrical arrangement, the quantity of electricity on each of the conducting surfaces would be  $k$  times as great as with air for dielectric and the same difference of potentials. The expression for the capacity of an actual Leyden phial is therefore

$$\frac{kS}{4\pi\tau}$$

$k$  being the inductive capacity of the solid non-conductor of which it is formed,  $\tau$  its thickness, and  $S$  the area of it which is coated on each side.

To investigate the capacity of a copper wire in the circumstances experimented on by Faraday, let us first consider the analogous circumstances regarding the conduction of heat; that is, let us consider the conduction of heat that would take place across the gutta-percha, if the copper wire in its interior were kept continually at a temperature a little above that of the water which surrounds it. Here the quantity of heat flowing outwards from any length of the copper wire, the quantities flowing across different surfaces surrounding it in the gutta-percha, and the quantity flowing into the water from the same length of gutta-percha tube, in the same time, must be equal. But the areas of the same length of different cylindrical surfaces are proportional to their radii, and therefore the flow of heat across equal areas of different cylindrical surfaces in the gutta-percha, coaxial with the wire, must be inversely as their radii. Hence, in the corresponding electrical problem, with air as the dielectric instead of gutta-percha, if  $R$  denote the resultant electrical force at any point  $P$  in the air between an insulated, electrified, infinitely long cylindrical conductor, and an uninsulated, coaxial, hollow cylindrical conductor surrounding it, and if  $x$  be the distance of  $P$  from the axis, we have

$$R = \frac{\Lambda}{x},$$

where  $\Lambda$  denotes a constant. But if  $v$  be the potential at  $P$ ; by the definition of "potential" we have

$$\frac{dv}{dx} = -R.$$

Hence

$$\frac{dv}{dx} = -\frac{\Lambda}{x};$$

and, by integration,

$$v = -\Lambda \log x + C.$$

Assigning the constants A and C so that the potential may have the value V at the surface of the wire, and may vanish at the hollow conducting surface round it, if  $r$  and  $r'$  denote the radii of these cylinders respectively, we have

$$v = V \frac{\log \frac{r'}{x}}{\log \frac{r'}{r}}$$

and

$$-\frac{dv}{dx} = R = \frac{V}{\log \frac{r'}{r}} \frac{1}{x}.$$

Taking  $x=r$ , we find by this the electric force in the air infinitely near the inner electrified conductor; and dividing the value found, by  $4\pi$  (according to the general theorem), we have

$$\frac{1}{4\pi} \frac{V}{r \log \frac{r'}{r}}$$

for the electrical density on the surface of the conductor. Multiplying this by  $2\pi r l$ , the area of a length  $l$  of the surface, we find

$$\frac{1}{2} \frac{Vl}{\log \frac{r'}{r}}$$

for the whole quantity of electricity on that length. Hence, if  $k$  be the specific inductive capacity of gutta-percha, the electricity resting on a length  $l$  of the wire in the actual circumstances will amount to

$$\frac{1}{2} \frac{kl}{\log \frac{r'}{r}} V.$$

Or if S denote the surface of the wire, we have, for the quantity of electricity which it holds,

$$V \cdot \frac{kS}{4\pi r \log \frac{r'}{r}};$$

and therefore its capacity is the same as that of a Leyden phial with an equal area of coated glass of thickness equal to  $\frac{1}{k} r \log \frac{r'}{r}$ , if I denote the specific inductive capacity of the glass. In the case experimented on by Mr. Faraday, the diameter of the wire was  $\frac{1}{16}$ th of an inch, and the exterior diameter of the gutta-

percha covering was about four times as great. Hence the thickness of the equivalent Leyden phial must have been

$$\frac{1}{k} \cdot \frac{1}{32} \log_e 4 = \frac{1}{k} \cdot \frac{1}{23.08}.$$

As the surface of the wire amounted to 8300 square feet, we may infer that if the gutta-percha had only the same (and it probably has a little greater) inductive capacity as glass, the insulated wire, when the outer surface of the gutta-percha was uninsulated, would have had an electrical capacity equal to that of an ordinary Leyden battery of 8300 square feet of coated glass  $\frac{1}{3}$ rd of an inch thick.

Inverclog, Arran,  
June 1854.

## LXXV. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 474.]

March 8, 1855.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

**T**HE following paper was read:—

“On Circumstances modifying the Action of Chemical Affinity.” By J. H. Gladstone, Ph.D., F.R.S.

The question intended to be solved in this communication is,—what takes place when two binary compounds AB and CD are brought together under such circumstances that both they themselves and the products of their mutual action remain free to react? Do they, according to a generally received opinion, remain unaltered, or, should the affinities so preponderate, become simply AB and CB? Or do A and C, according to Berthollet's view, divide themselves in certain proportions between B and D, the said proportions being determined not solely by the difference of energy in the affinities, but also by the difference of the quantities of the bodies? And, supposing the latter to be the correct view, do the amounts of AD and CB produced by the reaction, increase progressively with the relative increase of AB, or do sudden transitions occur, such as Bunsen and Debus have recently observed in certain cases where the products were removed at once from the field of action?

A reply was sought in the colours produced upon mixing different salts in aqueous solution. There were not many coloured salts suitable for the purpose, as it generally happens that a base gives the same colour with whatever acid it is combined, and *vice versa*; but the compounds of sesquioxide of iron were peculiarly adapted to the requirements of the experiment, as some are intensely coloured while others are nearly colourless.

The circumstances that attended the formation of the blood-red sulphocyanide were first fully examined. On mixing known quantities of different ferric salts with known quantities of different sul-

phocyanides, it was found that the whole of the iron was never converted into the red salt; that the amount of it so converted depended on the nature both of the acid combined with the ferric oxide, and of the base combined with the sulphocyanogen; and that it mattered not how the bases and acids had been combined previous to their mixture, so long as the same quantities were brought together into solution. The effect of mass was fully tried by mixing equivalent proportions of ferric salts and sulphocyanides, and then adding known amounts of either one or the other compound. It was found that in either case the amount of red salt was increased; and that when the numbers of equivalents of the salt added were taken as abscissæ, and the amounts of red sulphocyanide produced, as ordinates, the numbers observed in the experiments gave regular curves, though not belonging to the second order. The curves representing the experiments in which sulphocyanide of potassium was mixed with ferric nitrate, chloride, or sulphate, appeared to be the same, but hydrosulphocyanic acid gave a different curve. The deepest colour was given when nitrate of iron was mixed with the sulphocyanide, but even upon the admixture of one equivalent of the former with three of the latter, only 0.194 equiv. of the intensely red ferric salt was formed, and when 375 equivalents of sulphocyanide of potassium had been added there was still a recognizable amount of nitrate of iron undecomposed. It was found that the addition of a colourless salt not only reduced the colour of a solution of ferric sulphocyanide, but also that the reduction increased in a regularly progressive ratio according to the mass of the salt.

Other ferric salts were likewise examined. The black gallate gave results precisely analogous to those obtained by means of the sulphocyanide; the red meconate also confirmed Berthollet's views, but the action of mass was rendered obscure by the formation of double or of acid salts; the red pyromeconate resembled the meconate; the red acetate bore similar testimony; the blue solution of the ferric ferrocyanide in oxalic acid gave results fully corroborative of the influence both of the nature and of the mass of every substance present at the same time in the mixture; the purple and the red comenamate afforded similar results; while the red bromide (not the oxybromide), though somewhat indistinct in its testimony, corroborated to a certain extent the preceding observations.

Experiments were performed with a view to determine what effect the mass of water might have on the salts operated upon; its influence in reducing the colour of the ferric sulphocyanide was found to be very great, but the nature of it could not be exactly determined. As however it was uniform in its action in whatever manner the sulphocyanide had been produced, it could not affect the results of the preceding experiments. Water did not appear to act in any similar manner upon the other ferric salts.

From the mass of quantitative observations made during the investigation, it was possible to deduce not only the order of affinity of the various acids for sesquioxide of iron as compared with potash, but also to assign approximative numbers. Doubt may rest on the

position of some terms in the series, but hydrosulphocyanic acid certainly had the least affinity for ferric oxide in comparison with potash: it was represented by unity: the other acids followed in the order—nitric, 4; hydrochloric, 5; sulphuric, 7; gallic, 10; pyromeconic; meconic; acetic, 20; hydrobromic; comenamic; citric, 100; hydroferrocyanic, 170.

Other coloured salts were submitted to a more cursory investigation. The scarlet bromide of gold when treated with an alkaline chloride gave a striking instance of the effect of mass in gradually overcoming a strong affinity. The intensely red iodide of platinum afforded results which, though somewhat obscure, were not opposed in their testimony. So did the blue sulphate of copper when treated with different chlorides. The "manganoso-manganic oxide" dissolves in sulphuric or phosphoric acid of a red, and in other acids of a deep brown colour; and it was found that hydrochloric acid was capable of changing the colour of the sulphate according to its mass, while on the other hand sulphuric or phosphoric acid altered in like manner the tint of the chloride. Somewhat similar results were obtained by means of the green chloride and the purple fluoride of molybdenum; and the blue solution that forms when gallic acid is brought in contact with both the oxides of iron at once, bore testimony to the same general laws. The peculiar optical character of certain salts of quinine was also taken advantage of for determining what changes took place among the compounds in solution. The amount of fluorescence exhibited by a solution of acid sulphate of quinine was found to be affected by the admixture of a chloride, bromide, or iodide according to the nature and the mass of the salt added, and the addition of sulphuric, phosphoric, nitric and other acids was found to produce a fluorescence in solutions either of hydrochlorate of quinine, or of sulphate which had been rendered non-fluorescent by hydrochloric acid. Similar results were obtained with quinidine; and somewhat analogous ones with the organic bases contained in horse-chestnut bark, and in tincture of stramonium. An experiment is also narrated showing that the same laws hold good in respect to compound æthers as to salts having metallic bases, alcohol being employed as the solvent.

Beside the very diversified substances already mentioned in this abstract, several others, such as lead, mercury, zinc, potash, soda, baryta, lime, and ammonia, are shown by a more indirect proof to enter into compounds which obey the same laws. Hence it is concluded that what was observed in reference to the ferric salts holds good very generally, if not universally.

The bearing of certain other phenomena upon the question at issue was also examined. The fact that precipitation, when it occurs, gives rise to a perfect interchange of bases and acids, is equally consistent with either Bergmann's or Berthollet's theory; but not so is the fact that two soluble salts cannot be mixed without the occurrence of precipitation, if one of the products that may be formed is an insoluble salt. The only recorded exception to this law, which occurs with oxalate of iron in the presence of a salt of yttria, under peculiar circumstances, was found on close examination to be in

perfect accordance with the principles laid down by Berthollet. Besides the argument founded on this universal fact, several experiments were devised for the purpose of proving that the complete precipitation of an insoluble salt on the mixing of two soluble salts, was due to the insoluble compound being removed at once out of the field of action on the first distribution of the elements, thus necessitating a redivision, and so on until no more of it could possibly be formed. The phenomena attending volatilization have the same bearing as those connected with precipitation. If by the mutual action of two salts a substance be formed, which, though soluble in water, requires more water for its solution than is present, it crystallizes out: certain experiments were noted where this action occurs, and it was found that they gave testimony in favour of the same views as have been supported by the preceding observations. The bearing of the phenomenon of diffusion of salts upon the point at issue was also examined: Malaguti's experiments were discussed; and they, as well as some observations on the solution of certain bodies by others set at liberty, were found to bear testimony also in the same direction.

During the whole of the experiments on this subject, most of which were performed quantitatively, no unequivocal instance occurred of two substances having so strong an affinity for one another, that they combined to the exclusion of other bodies of like kind present in the same solution. After showing that some reputed exceptions are really not capable of being proved to be so, and after suggesting some probable limitations of the action of the general law, the paper concludes with the following deductions:—

I. That where two or more binary compounds are mixed under such circumstances that all the resulting compounds are free to act and react, each electro-positive element enters into combination with each electro-negative element in certain constant proportions.

II. That these proportions are independent of the manner in which the different elements were primarily arranged.

III. That these proportions are not merely the resultant of the various strengths of affinity of the several substances for each other, but are dependent also on the mass of each of the substances present in the mixture.

IV. That an alteration in the mass of any one of the binary compounds present alters the amount of every one of the other binary compounds, and that in a regular progressive ratio; sudden transitions only occurring where a substance is present which is capable of combining with another in more than one proportion.

V. That this equilibrium of affinities arranges itself in most cases in an inappreciably short space of time, but that in certain instances the elements do not attain their final state of combination for hours.

VI. That totally different phenomena present themselves where precipitation, volatilization, crystallization, and perhaps other actions occur, simply because one of the substances is thus removed from the field of action, and the equilibrium that was first established is thus destroyed.

VII. That consequently there is a fundamental error in all



attempts to determine the relative strength of affinity by precipitation,—in all methods of quantitative analysis founded on the colour of a solution in which colourless salts are also present,—and in all conclusions as to what substances exist in a solution, drawn from such empirical rules as, that “the strongest acid combines with the strongest base.”

March 15, 1855.—The Lord Wrottesley, President, in the Chair.

The following communications were read :—

“Researches on Organo-metallic Bodies.” By E. Frankland, Ph.D., F.R.S., Professor of Chemistry in Owens College, Manchester. Second Memoir.—Zincethyle.

This compound, whose existence was mentioned in a previous memoir\*, is formed by the action of zinc upon iodide of ethyle, or a mixture of iodide of ethyle and anhydrous ether, at a temperature exceeding  $100^{\circ}$  C. The materials are enclosed in a copper digester capable of resisting great pressure. When purified by rectification in an atmosphere of carbonic acid, zincethyle possesses the following properties :—At ordinary temperatures it is a colourless, transparent and mobile liquid, refracting light strongly and possessing a peculiar odour, rather pleasant than otherwise, and therefore differing greatly from that of zincmethyle. Its specific gravity is  $1.182$  at  $18^{\circ}$  C. Exposed to a cold of  $-22^{\circ}$  C. it exhibits no tendency to become solid. Zincethyle boils at  $118^{\circ}$  C., and distils unchanged. The specific gravity of its vapour is  $4.251$ . Several analyses of zincethyle prove its formula to be

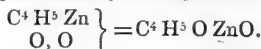


The vapour volume of zincethyle is highly remarkable, and almost compels us to conclude that the vapour volume of the double atom of zinc is only equal to that of oxygen, instead of corresponding with the volume of hydrogen, in accordance with the generally received supposition. Zincethyle, therefore, appears to belong to the so-called water type, and to consist of two volumes of ethyle and one volume of zinc vapour; the three volumes being condensed to two: for if we were to assume that an equivalent of zinc occupies the same vapour volume as an equivalent of hydrogen, we should then have the anomaly of the combination of equal volumes of two radicals being attended by condensation.

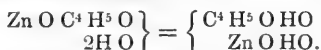
Although zincethyle is remarkable for the intense energy of its affinities, which place it nearly at the head of the list of electro-positive bodies, yet it does not appear to be capable of forming any true compounds with electro-negative elements, its reactions being all double decompositions in which the constituents of the zincethyle separate. Zincethyle is spontaneously inflammable in atmospheric air or oxygen; but when a few drops, diluted with ether to prevent inflammation, are passed into a mercurial eudiometer containing dry atmospheric air, a rapid absorption of oxygen takes place, with the formation of a white amorphous solid composed of zinc, ethyle, and

\* Philosophical Transactions, 1852, p. 436.

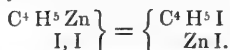
oxygen. This reaction, which is also common to zincmethyle and zincamyle, led me to suppose that, like cacodyle, these bodies combined directly with oxygen; but the results of a closer study of the action of oxygen upon zincethyle prove that no such compound is formed; the white body being ethylate of zinc, and containing no organo-metallic compound, in the strict sense of the term. The action of oxygen upon zincethyle is expressed in the following equation:—



The ethylate of zinc thus produced is decomposed by water into hydrated oxide of zinc and alcohol—



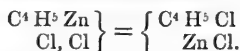
Zincethyle is acted upon with great energy by iodine; when the violence of the reaction is moderated, by the application of intense cold and the intervention of ether, the sole products are iodide of zinc and iodide of ethyle—



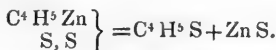
Bromine acts with explosive violence on zincethyle, but the action may be moderated by adding the bromine in the form of diffused vapour and cooling to 0° C. The sole products of the reaction are then bromide of ethyle and bromide of zinc—



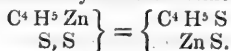
Zincethyle burns with a lurid flame spontaneously in chlorine gas; the zinc and hydrogen are converted into chlorides, whilst carbon is deposited in the form of soot. I have not studied the products of a more moderate action, as it is difficult to bring the materials together without too great an elevation of temperature. There can be no doubt, however, that the moderated action of chlorine would be analogous to that of bromine or iodine, and that the products would be chloride of ethyle and chloride of zinc—



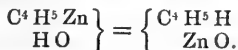
Carefully dried flowers of sulphur have only a slight action upon an ethereal solution of zincethyle, but the application of a gentle heat suffices to produce a brisk reaction; the sulphur gradually disappears, a white flocculent precipitate is formed, and a strong odour of sulphide of ethyle developed. The chief product of this reaction is the double sulphide of ethyle and zinc (mercaptide of zinc), which is produced as follows:—



A little free sulphide of ethyle is also formed—



Finally, zincethyle is decomposed by water into oxide of zinc and hydride of ethyle—



It is also similarly acted upon by the hydrated acids and by the hydrogen compounds of chlorine, bromine, iodine, fluorine and sulphur.

The behaviour of zincethyle in contact with the electro-negative elements is highly remarkable, and cannot fail to have an important influence upon our views of the condition of bodies at the moment of chemical change,—a subject so ably discussed by Brodie\*, whose ingenious views I consider to receive a new support in these reactions of zincethyle, by the singular way in which ethyle, a body low down in the electro-positive series, unites with oxygen, chlorine, &c., in the presence of a large excess of the intensely electro-positive zincethyle. This behaviour also strikingly confirms the suggestions I ventured to make in my former memoir†, relative to the moleculosymmetrical form of organo-metallic compounds. In the inorganic combinations of zinc, this metal unites with one atom only of other elements; a very unstable peroxide, not hitherto isolated, being the only exception. The atom of zinc appears, therefore, to have only one point of attraction, and hence, notwithstanding the intense affinities of its compound with ethyle, any union with a second body is necessarily attended by the expulsion of the ethyle.

“Note on the Magnetic Medium.” By Prof. A. W. Williamson.

In a letter to Mr. Faraday recently published in the Philosophical Magazine, Dr. Tyndall brings forward some important considerations on the subject of magnetic philosophy.

It has been known for some time that the phænomena of diamagnetism may be produced artificially in bodies which are usually considered magnetic. For this purpose it is only necessary to plunge the magnetic body into a yielding medium more magnetic than itself. When thus exposed to the action of a magnet it recedes from the poles, because the volume of the medium which it displaces is more powerfully attracted.

This fact naturally suggested the idea, that all repulsion by the magnet might be owing to the attraction exercised on the medium being stronger than that on the body repelled;—just as balloons are driven upwards by the superior weight of the displaced volume of air. And as phænomena of diamagnetism are observed in a so-called vacuum, it was thought that some “magnetic medium” might be present there.

I do not purpose on this occasion to enter upon the general question of the evidence which may be adduced for or against this important conclusion; for it could only be proved satisfactorily by considerations including phænomena of the most varied kind, such as electricity, light, chemical action, &c., to which it must necessarily apply. But it might be disproved by any one well-understood fact contradictory to it.

Now it appears to me, that the facts adduced by Dr. Tyndall are

\* Philosophical Transactions, 1850, p. 789.

† Ibid. 1852, p. 438.

not inconsistent with the notion of a magnetic medium, but follow naturally from it; and that his argument involves a tacit assumption foreign to the theory under consideration.

The first fact adduced is, that compression increases the attraction of magnetic bodies, and the repulsion of diamagnetic bodies by the magnet, in the direction of the line of compression. Now it is evident, that a variation of pressure on a number of particles surrounded by a magnetic medium may alter the attraction of the mass by a magnet in two ways;—first, by altering the density of the matter\*; secondly, by altering the density of the medium.

In a cubical mass of carbonate of iron the material particles are more magnetic than the medium which they displace, and the force with which it is attracted is proportional to this excess.

If it becomes more magnetic by compression, we must conclude that the loss of magnetic medium *from its interstices* is more than supplied by the magnetic matter which takes its place.

Carbonate of lime is less magnetic than the quantity of medium which its particles displace, and when these particles are brought closer together by pressure, with diminution of the intervening spaces occupied by the medium, the mass becomes more diamagnetic, because a certain quantity of the magnetic medium is thus replaced by the less magnetic matter.

Dr. Tyndall seems to have assumed, that on the compression of an aggregate of particles of a diamagnetic substance, the medium is not displaced by the particles in their change of position;—in which case, his conclusion, that compression must increase the magnetic functions of *every* substance, would no doubt follow from the notion of a magnetic medium.

The second fact adduced differs chiefly in form from the one just considered. Crystals of carbonate of iron are attracted most strongly by a magnet acting in the direction of the crystallographic axis. Crystals of carbonate of lime, possessing the same form, are most strongly repelled in the direction of the same axis. In this direction the functions of the matter predominate more over those of the medium than in other directions of the crystal; so that with carbonate of iron, we have the strongest magnetism; with carbonate of lime, the strongest diamagnetism in this axis. One crystal consists of magnetic medium with strongly magnetic matter; the other consists of the medium with matter of very slight magnetic force.

The crystallographic axis is in both crystals the direction in which the function of matter predominates most strongly over that of the medium; so that in the iron salt it is the most magnetic, in the lime salt the feeblest magnetic direction in the crystal.

March 22.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

“Further observations on the Anatomy of *Macgillivraya*, *Chele-tropis*, and allied genera of pelagic Gasteropoda.” By John Denis Macdonald, Esq., R.N., Assistant-Surgeon H.M.S.V. ‘Torch.’

The author states, that in a late voyage from Sydney to Moreton

\* The word “matter” is here used for brevity to denote *ponderable* matter.

Bay, specimens of *Macgillivraya*, *Cheletropis*, and a few other genera of minute pelagic Gasteropoda, apparently undescribed, were daily taken in the towing-net, and afforded him an opportunity of more precisely determining the mode of attachment of the ciliated arms which he had at first presumed to be naked branchiæ.

In his former paper\* it was stated, more particularly of *Cheletropis Huxleyi*, that the gills were of two kinds, viz. "covered" and "naked;" the former, corresponding to those of the pectinibranchiate Gasteropoda generally, he has never found to be absent in any of the genera; but from further observation of the so-called naked gills, while the animals were alive in their native element, he is disposed to think that they are chiefly employed for prehension, and probably as auxiliary organs of natation. When these ciliated appendages are fully extended, the line of cilia is perfectly straight, so that the frilled border, noticed in the previous account, turns out to be a character depending simply on the partial contraction of the longitudinal muscular fibres, preparatory to complete retraction of the organs. They have no connexion with the mantle, but encircle the mouth together with the tentacula and eyes, and coalesce at their bases like the segments of a deeply-cleft calyx. In the specimens of *Macgillivraya* examined the arms were quite transparent, but marked at irregular intervals with cross streaks of brownish purple. In the extended state they were several times the length of the shell, and, like the arms of a polype, they rolled themselves up when touched, and started back into the shell with surprising rapidity. They appeared also to be exquisitely sensitive, exhibiting short twitching movements when minute particles suspended in the water came in contact with them.

In the specimens of *Macgillivraya* now referred to, the respiratory siphon consisted of a process of the mantle converted into a tube by the mere apposition of its borders without organic union; it was moreover much shorter than had been usually observed in previous examples, and the author thinks that those now under consideration may be a variety, if not a distinct species.

In his former examinations of this tribe of Gasteropoda, the author had never found more than four arms encircling the head, but he has since discovered six in a single genus with which he had been long familiar by external characters. In this case the operculigerous lobe of the foot is quite cylindrical and of some length, bearing the peculiar operculum on its truncated extremity with the clawed process pointing to the left side. The sucker-disc is very small, and presents an anterior and posterior lobe. The two tentacula bear each an ocellus on the outer side near the base, and the ciliated arms, in every respect save number, resemble those of *Macgillivraya* and its congeners. The clawed operculum is developed from a spiral nucleus situate near the internal thickened border; it seems to be a weapon of defence, and is wielded with great dexterity by the little animal, which makes skips and jerks by means of its complex foot, after the manner of *Nassa* or *Strombus*.

The author notices another member of this diminutive tribe which

\* Phil. Mag. p. 297.

is very commonly met with in the South Pacific, and has almost an indefinite range. As regards both animal and shell, it in many points resembles a miniature *Natica*. The shell is few-whorled, with small compressed spire and ventricose mouth; the operculum paucispiral and well-marked with the lines of growth. The foot is not unlike a broad and square-toed shoe in form, receiving or bearing the remainder of the animal and the shell. The shoe-upper, as it were, presents two rounded lateral lobes which lie over the anterior part of the shell, like the mentum of *Natica*. The little animal creeps on its foot with great rapidity, appearing rather to slide along than progress by a vermicular movement, and by spreading out and hollowing this organ at the surface of the water, as a freshwater Lymnæad forms a boat of its foot, it buoys up its tiny body and is cast abroad on the face of the ocean.

### LXXVI. *Intelligence and Miscellaneous Articles.*

EXPERIMENTS RELATING TO ENDOSMOSE. BY M. LHERMITE.

THE term *endosmose* has been applied by Dutrochet to the passage of one liquid towards another through a septum affording less resistance to the passage of one liquid than to that of the other.

From analogies which were erroneously supposed to exist between this phænomenon and the experiment made by Porret, endosmose was attributed to electrical action.

Poisson explained the phænomenon as a result of capillarity, but he did not ascribe to the attraction of solids for liquids any other function than that of determining the occupation of the numerous small channels, of which the septum may be regarded as constituted, by one liquid in preference to another, and of preventing the interruption of the liquid threads. He referred the ulterior action to the mutual attraction of the two liquids.

Dutrochet attributed, in his later memoirs on this subject, much to the reciprocal chemical action of the liquids, but he never attempted to explain the mode in which the membrane acted, always being inclined towards the electrical theory.

Mr. Graham has recently been led to the conclusion, that the alteration of the septum appears to be a condition indispensable for the manifestation of the osmotic force. According to him, one of the surfaces of the membrane is acid, the other basic, and this state is connected with the progressive decomposition of its substance.

M. Lhermite considers that he has demonstrated, by means of his own observations and those of his predecessors, that endosmose is not produced by any peculiar force, but that it is a result of affinity (chemical action), taking the widest signification of that term, and including capillary attraction.

Poisson, who treated the question as a mathematician, and not experimentally, admits that when the movement has once commenced, the septum has no further share in the action; but if the action takes place solely between the liquid A, which is actually absorbed

by the septum, and the liquid B, which attracts it with a force superior to that exercised between the molecules of A, there is no reason why the movement should not take place as well in one direction as the other.

At the moment when the pores of the septum are charged with the liquid A, and if the passage of the liquid threads A towards the liquid B is considered to be more rapid than the passage of B towards the liquid threads A, on account of the smaller mass of A, it follows that when the septum is charged with the liquid B, the threads will be attracted by the mass of A, and the movement will take place in a contrary direction.

Dutrochet and Mr. Graham have pointed out, in opposition to this theory, that the elevation of liquids in capillary tubes is very slight compared with the height of the columns of liquid supported by endosmose; but these observers do not appear to have taken into account the circumstance that, in reality, the height of the liquid in ordinary tubes indicates merely the action of the liquid upon itself. This is proved by the circumstance, that the liquid column is always the same height in tubes of equal diameter, and made of any substance that is wetted by the liquid, as if the layer of liquid nearest to the surface of the tube were the seat of the action; whereas the capillary force essential in endosmose results from the action of the solid substance on the liquid.

In order that this may produce its greatest effect, the diameter of the tubes must not be more than double the distance at which the attraction is exercised; and in ordinary capillary tubes the conditions requisite for the production of an appreciable osmotic motion are far from being fulfilled.

The only weak point of Poisson's theory, so far at least as regards the porous septum, consists in the disregard of the elective action of the solid substance just at the point where its consideration becomes necessary. When the surfaces of the septum are in contact with two liquids which are miscible, one being more strongly attracted by the substance of the septum than the other, there would at the moment of contact between the liquids be an expulsion of one by the other, and a consequent motion which would cease so soon as the liquid was in contact with the septum throughout its entire extent.

Porous clay vessels have channels whose minuteness is evident from the slowness with which water passes through them, nevertheless they are inferior to liquids in the osmotic faculty. There is no doubt but that this is owing to the difference in the distance between the molecules of the latter compared with the mechanical or accidental interstices of the former. Liquids are osmotic agents *par excellence*.

When two liquids of different densities are placed in a cylindrical test tube, and separated by a third liquid of intermediate density which dissolves, in appreciable quantity, only one of the other liquids, this one will pass into the other. When, for instance, chloroform is at the bottom, above it a layer of water, and then a layer of æther, the chloroform layer gradually increases in volume; the æther layer diminishes and at last disappears; while the water layer is scarcely

at all altered. Other analogous experiments might be given, and in all instances the result may be predicted according to the solubility of the substances.

It is true that the increase of pressure, which it is customary to regard as a character of osmose, is wanting in this experiment. In order that this may take place, it is necessary to render the intermediate layer fixed. For this purpose a porous vessel is saturated with the liquid intended to act as septum, and the experiment is made as if the porous vessel were the septum, the liquid which mixes most readily with the intermediate liquid being in preference placed outside, so that the principal osmotic movement may be more appreciable from the accumulation of liquid in the osmometer.

When the porous vessel is impregnated with castor oil, filled with water and immersed in alcohol, the osmose is towards the water; while in a vessel not charged with oil, the principal movement is in the contrary direction.

The absorption of a liquid by a tissue is the inverse of the solution of a solid by a liquid. Animal membrane, by virtue of the capability of absorbing water and mixing it with other liquids, is comparable with liquids themselves; but with the advantage that it may be fixed between two liquids without being separated and dispersed, as in the case of liquids with which a porous vessel is saturated.

The direction of the osmotic movement may be predicted equally well for porous vessels or animal membrane when the rapidity with which they are traversed by the respective liquids is known. The rapidity of filtration is not always proportionate to the mobility of the liquids. Membranes and even porous clay vessels are permeated by water more readily than by alcohol, notwithstanding the far greater fluidity of the latter.

M. Lhermite considers that his experiments furnish an evident demonstration of the error into which Mr. Graham has fallen in attributing osmotic motion to the chemical decomposition of the membrane, and in supposing that the motion always carries the acid towards the base. M. Lhermite has indeed found that a solution of oxalic acid, which produces the greatest effect, is a conservative agent; while, on the other hand, with a solution of alkali in alcohol and a very dilute solution of acid in water, the base is carried towards the acid through either animal membrane or porous clay impregnated with castor oil.—*Comptes Rendus*, No. 25, 1855.

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ON THE LUMINOUS COAT OF THE INDUCTION SPARKS OF RUHM-KORFF'S APPARATUS. BY T. DU MONCEL.

When examining these sparks in the dark, the author observed that they are surrounded with a greenish-yellow luminous coat, the thickness and form of which varies according to the strength of the current, and the nature of the wires forming the poles; when the sparks are not very long, however, the luminous coat is almost always of an oval form. It appears to belong principally to the negative pole, and is of a reddish colour on the side of this pole. It is remarkably intense, and resembles a flame when the poles are moistened with an essential oil, and its connexion with the negative pole is also then very distinct.



If the wires of the pole (which should be rather thick) be placed about five millimetres apart, and the intervening space be blown upon rather strongly with a bellows, the greenish-yellow atmosphere is seen to be drawn towards the opposite side, where it forms a large flame of a violet colour. It may also be blown along the wires of the pole, and it is then seen to be furrowed by a number of very sinuous, more or less white jets of fire, which are usually disposed in strata. The violet light appears to be circumscribed by two bundles of violet rays, which rise from the points of the poles and unite in irregular curves, like those of a flame driven by the wind. The true spark does not appear to be affected by the current of air.—*Comptes Rendus*, Feb. 5, 1855, p. 313.

ON COMPLEMENTARY COLOURS. BY H. MEYER.

Without going into the consideration of the proposed explanations of the complementary colours, I give a few experiments by which the observation of these subjective colours is much facilitated, reserving a further consideration of these phænomena, and the conclusions to be drawn from them, for a subsequent memoir.

If a narrow strip of gray paper be laid upon a coloured surface, this strip appears tinged with the complementary colour of the body on which it is lying. This experiment does not, however, always succeed equally well, and is best performed with a green surface; if the strip of paper be white and a little broader, the complementary colour is only observed after long watching, or perhaps not at all. The complementary colour may, however, be produced *immediately and quite distinctly*, even with white strips of such breadth, that, without further assistance, they cannot show the complementary colour (several inches broad) by laying a leaf of fine, transparent writing-paper over the coloured surface and white paper. The white strip *immediately* appears covered with a tolerably uniform pale tinge of the complementary colour.

A sheet of coloured paper laid beside one of white paper does not allow the complementary colour to be observed upon the latter; and it is only when the coloured sheet has been looked at for some time, and the eyes are then alternately directed from the coloured surface to the white, that the strip of the latter lying next to the coloured surface is tinged with a somewhat intense complementary colour. But if a transparent sheet of writing-paper be laid over the coloured and white surfaces, the complementary colour immediately makes its appearance upon the latter, without any previous removal of the eye from one surface to the other. If the eye be directed to the margin of the white and coloured surfaces, the portion of the white surface lying next to the coloured surface appears more intensely tinged with the complementary colour than the parts lying at a greater distance; but if the white surface be examined by moving the eye, so that the different parts of the white surface may be represented one after the other upon the same part of the retina, the surface appears covered with a more uniform tint of the complementary colour.—Poggen-dorff's *Annalen*, vol. xcv. p. 170.

## INDEX TO VOL. IX.

- ÆTHER**, on the existence of an electrical, through space, 457.
- Æthyle**, action of iodide of, on picoline, 214; on pyridine, 219; on collidine, 221.
- Airy (G. B.)** on the pendulum experiments lately made in the Harton Colliery, for ascertaining the mean density of the earth, 309; on the computation of the effect of the attraction of mountain masses, 394.
- Alum**, researches into the manufacture of, 413.
- Amylaceous foods**, micro-chemical researches on the digestion of, 459.
- Anderson (Dr. T.)** on the products of the destructive distillation of animal substances, 145, 214.
- Angström (A. J.)**, optical researches, 327.
- Animal substances**, on the products of the destructive distillation of, 145, 214.
- Antimony**, on a peculiar phenomenon in the electro-deposition of, 73.
- Arithmetical progressions**, on the formation of powers from, 59.
- Asteroid**, on the discovery of a new, 152.
- Ayres (Dr. P. B.)**, micro-chemical researches on the digestion of starch and amylaceous foods, 459.
- Ball (J.)** on a doubtful point in climatology, 363.
- Barlow (the late W. F.)** on the respiratory movements of insects, 229.
- Becquerel (E.)** on the magnetic power of oxygen, 474.
- Benzole series**, on the determination of boiling-points of the, 256; researches on the, 453.
- Bertagnini (C.)** on phillyrine, 78.
- Binney (E.)** on the structure of some limestone nodules enclosed in seams of bituminous coal, 235.
- Books, new:**—Hopkins on the Atmospheric Changes which produce Rain and Wind, and the Fluctuations of the Barometer, 54; Riess's Treatise on Frictional Electricity, 150.
- Boole (Prof. G.)** on certain propositions in algebra connected with the theory of probabilities, 165.
- Brachiopoda**, on the anatomy of the, 395.
- Brame (C.)** on the limit of vaporization of mercury, 157.
- Bridge (J.)** on the oblique aberration of lenses, 342.
- Brine-springs of Worcestershire**, on the, 27.
- Brooke (C.)** on the structure of certain microscopic test-objects, and their action on the transmitted rays of light, 56.
- Buff (Prof.)** on galvanic circuits in which perchloride of iron enters as an element, 139.
- Bunsen (R.)** on the law of absorption of gases, 116, 181.
- Callan (Rev. N. J.)** on a new single fluid galvanic battery, 260.
- Carmichael (Rev. R.)** on the quadrature of surfaces and the rectification of curves, 209.
- Cayley (A.)** on the porism of the in- and-circumscribed triangle, 513.
- Challis (Prof.)** on the eccentricity of the moon's orbit, 130; on the theory of the moon's motion, 201, 370; on the aberration of light, 430.
- Chapman (Prof.)** on the object of salt in the sea, 236.
- Cheletropis***, on the anatomy of, 297, 542.
- Chemical affinity**, on circumstances modifying the action of, 535.
- Church (A. H.)** on the benzole series, 256; on some derivatives of xylene, 453.
- Climatology**, on a doubtful point in, 363.
- Coercive force**, on the nature of the so-called, 518.
- Collidine**, analysis of, 148; action of iodide of æthyle on, 221.

- Collodion, on sensitive, 398.  
 Colours, on complementary, 547.  
 Connell (A.) on the new hygrometer or dew-point instrument, 143.  
 Cooper (E. J.) on the perihelia and nodes of the planets, 472.  
 Crookes (W.) on a more convenient form of applying gallic acid as a developing agent in photography, 225.  
 Crystalline lens, on the optical structure of the, 306.  
 Crystals, on the adjustment of, for measurement with the reflective goniometer, 138.  
 Daubrée (M.) on the artificial production of minerals belonging to the family of silicates and aluminates, 315.  
 Diatomaceæ, on the dots upon the valves of the, 383.  
 Differential transformation, researches on, 391.  
 Dobell (H.) on the applicability of gelatine paper as a medium for colouring light, 155.  
 Du Moncel (T.) on the luminous coat of the induction sparks of Ruhmkorff's apparatus, 546.  
 Earth, experiments to determine the mean density of the, 309.  
 Edingtonite, analysis of, 179.  
 Eisenlohr (W.) on the action of the violet and ultra-violet invisible light, 114.  
 Elasticity of solid bodies, on the mathematical theory of the, 305.  
 Electric induction, on associated cases in, of current and static effects, 161; on the action of non-conducting bodies in, 401.  
 Electricity, on a method of increasing certain effects of induced, 1.  
 Electroscope, on a new, 276.  
 Electro-static capacity of a Leyden phial and of a telegraph wire, on the, 531.  
 Elliptic motion, on a case of disturbed, 51.  
 Endosmose, experiments relating to, 544.  
 Euphrosyne, notice of the discovery of the new asteroid, 152.  
 Excrements of man and animals, on the immediate principles of the, 65.  
 Faraday (Prof.) on some points of magnetic philosophy, 81; on associated cases, in electric induction, of current and static effects, 161; magnetic remarks, 253.  
 Fats, on the composition and properties of, 74.  
 Felspars of the granite of the Dublin and Wicklow mountains, on the composition of the, 40, 511.  
 Felspathic rocks, examination of some, 354.  
 Forrester (J. J.) on the vine-disease, and the proposed remedies for its eradication, 67.  
 Frankland (Prof. E.) on organo-metallic bodies, 539.  
 Fusion, on the alteration of volume of some substances by, 477.  
 Galbraith (Rev. J. A.) on the composition of the felspars of the granite of the Dublin and Wicklow mountains, 40.  
 Gallic acid as a developing agent in photography, on a more convenient form of applying, 225.  
 Galvanic battery, description of a new single fluid, 260.  
 — circuits in which perchloride of iron enters as an element, researches on, 139.  
 Gases, on the law of absorption of, 116, 181.  
 Gasteropoda, on the anatomy of pelagic, 542.  
 Gelatine paper, on the applicability of, as a medium for colouring light, 155.  
 Gladstone (Dr. J. H.) on circumstances modifying the action of chemical affinity, 535.  
 Gold and mercury, on a new compound of, 458.  
 Gold fish of Franklin, remarks on the, 227.  
 Gore (G.) on a peculiar phænomenon in the electro-deposition of antimony, 73.  
 Gosse (P. H.) on the structure, functions and homology of the manducatory organs in the Class Rotifera, 470.  
 Greg (R. P.) on British Pectolites, 248; on the crystalline form of leucophane, 510.  
 Griffith (Dr. J. W.) on the dots upon the valves of the Diatomaceæ, 383.

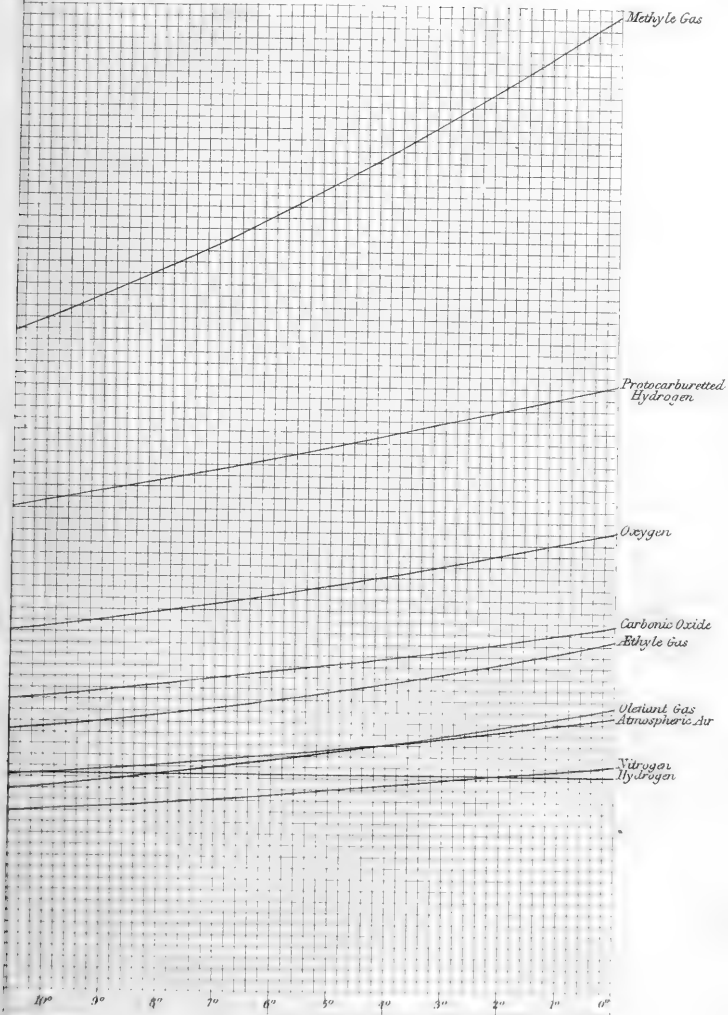
- Grove (W. R.) on a method of increasing certain effects of induced electricity, 1.
- Hamilton (Sir W. R.) on some extensions of quaternions, 46, 280.
- Hart (R.) on an appearance seen in the moon, 238.
- Haughton (Rev. S.) on the chemical composition and optical properties of the mica of the Dublin, Wicklow and Carlow granites, 272.
- Heat, on the alteration of volume of some substances by, 477; on the dynamical theory of, 523.
- Heddle (Dr. M. F.), analysis of the mineral Edingtonite, 179; on British Pectolites, 248; on table spar from the Morne Mountains, 452.
- Heintz (Dr.) on the composition and properties of fats, 74.
- Heny (T. H.) on a new compound of gold and mercury, 458.
- Herapath (W. B.) on the properties of the sulphate of iodo-quinine or Herapathite, 366.
- Hooker (Dr. J. D.) on the structure and functions of the rostellum in *Listera ovata*, 64; on the structure of some limestone nodules, 235.
- Hopkins (T.) On the Atmospheric Changes which produce Rain and Wind, and the Fluctuations of the Barometer, reviewed, 54.
- Horner (L.) on the geological history of the alluvial land of Egypt, 465.
- Hunt (T. S.), examinations of some felspathic rocks, 354; on the mineral Wilsonite, 382.
- Huxley (T. H.) on the anatomy of the Brachiopoda, 395.
- Hygrometer, description of a new, 143.
- Induction sparks of Ruhmkorff's apparatus, on the luminous coat of the, 546.
- Insects, on the respiratory movements of, 229.
- Integrals, on the theory of definite, 230.
- Invariants, on the theory of, 384.
- Iodo-quinine, on the properties of the sulphate of, 366.
- Jago (J.) on ocular spectres and structures as mutual exponents, 305.
- Jennings (F. M.) on an analysis of some potash and soda felspars, 511.
- Knox (G. J.) on the existence of an electrical æther through space, 457.
- Kopp (H.) on the alteration of the volume of some substances by heating and fusion, 477.
- Lenses, on the oblique aberration of, 342.
- Leucophane, on the crystalline form of, 510.
- Lhermite (M.) on some experiments relating to endosmose, 544.
- Light, on the action of the violet and ultra-violet invisible, 114; on the applicability of gelatine paper as a medium for colouring, 155; on the interference of, near a caustic, 321; researches on, 327; on the aberration of, 430.
- Limestone nodules enclosed in seams of bituminous coal, on the structure of some, 235.
- Listera ovata*, on the structure and functions of the rostellum in, 64.
- Lubbock (Sir J. W.) on the heat of vapours, 25.
- Luminiferous medium, on the possible density of the, 36.
- M. (W. H.) on the adjustment of crystals for measurement with the reflective goniometer, 138.
- Macdonald (J. D.) on the anatomy of the *Macgillivrayia pelagica* and *Cheletropis Huxleyi*, 297, 542.
- Macgillivrayia*, on the anatomy of, 297, 542.
- Magnet, on the nature of the force by which bodies are repelled from the poles of a, 387.
- Magnetic force, elementary demonstrations of propositions in the theory of, 241.
- induction, researches on, 518.
- medium, on the existence of a, in space, 205, 290, 541.
- philosophy, on some points of, 81, 253.
- power of oxygen, on the, 474.
- Magnetism, on the periodical variations of terrestrial, 432; on the optical properties developed in transparent bodies by the action of, 481.
- Magnus (Prof.) on the elastic forces of vapours of mixtures of two liquids, 44.

- Mammalia, on the development of muscular fibre in, 299.
- Marcet (Dr. W.) on the immediate principles of the excrements of man and animals, 65.
- Maury (Lieut.) on the discovery of a new asteroid, 152.
- Melloni (M.) on a new electroscope, 276.
- Mercury, on the limit of vaporization of, 157.
- and gold, on a new compound of, 458.
- Meteorological observations, 54, 79, 159, 239, 319, 399, 479.
- Meyer (H.) on complementary colours, 547.
- Mica of the Dublin, Wicklow and Carlow granites, chemical composition and optical properties of the, 272.
- Microscopic test-objects, on the structure of certain, 56.
- Mineralogy, notes on, 40, 179, 248, 272, 354, 382, 452, 510.
- Minerals, on the artificial production of, 315.
- Moon, on an appearance seen in the, 238; on the theory of the motion of the, 201, 370; on the eccentricity of the orbit of the, 130.
- Mountains, on the computation of the effect of the attraction of, 394.
- Muscular contraction, on the physical theory of, 293.
- fibre, on the development of, in Mammalia, 299.
- Nile-mud, analyses of, 469.
- Nitro-sulphoxylolic acid, on the preparation of, 455.
- Nitroxylene, on the preparation of, 453.
- Northcote (A. B.) on the brine-springs of Worcestershire, 27.
- Ocular spectres and structures as mutual exponents, remarks on, 305.
- Optical researches, 327.
- Organo-metallic bodies, researches on, 539.
- Ovum of the Stickleback, on the impregnation of the, 153.
- Oxygen, on the magnetic power of, 474.
- Pectolites, analysis of British, 248.
- Pendulum experiments, account of some, 309.
- Phillyrine, analysis of, 78.
- Picoline, action of iodide of æthyle on, 214.
- Planets, on the perihelia and nodes of the, 472.
- Plücker (Prof.) on the nature of the so-called coercive force, 518.
- Plumb-line, on the attraction of the Himalaya mountains upon the, in India, 231.
- Polymagnet, description of the, 425.
- Porism of the in-and-circumscribed triangle, on the, 513.
- Potter (Prof.) on the interference of light near a caustic, and the phenomena of the rainbow, 321.
- Pratt (Ven. J. H.) on the attraction of the Himalaya mountains upon the plumb-line in India, 231.
- Probabilities, on certain propositions in algebra connected with the theory of, 165.
- Pyridine, action of iodide of æthyle on, 219.
- Quadrature of surfaces, theorems on the, 209.
- Quaternions, on some extensions of, 46, 280.
- Radcliffe (Dr. C. B.) on the physical theory of muscular contraction, 293.
- Rainbow, on the phenomena of the, 321.
- Rankine (W. J. M.) on the general integrals of the equations of the internal equilibrium of an elastic solid, 301.
- Ransom (Dr. W. H.) on the impregnation of the ovum in the Stickleback, 153.
- Regnault (V.) on the elastic forces of vapours *in vacuo* and in gases, at different temperatures; and on the tensions of the vapours furnished by mixed or superposed liquids, 4.
- Retina, on the optical anatomy of the, 307.
- Riess's (P. T.) Treatise on Frictional Electricity, reviewed, 150; on the action of non-conducting bodies in electric induction, 401.
- Rotifera, on the structure, functions and homology of the manducatory organs in the, 470.
- Royal Institution of Great Britain, proceedings of the, 309.

- Royal Society, proceedings of the, 56, 150, 229, 293, 383, 459, 535.
- Russell (W. H. L.) on the theory of definite integrals, 230.
- Salt, on the object of, in the sea, 236.
- Savory (Dr. W. S.) on the development of muscular fibre in Mammalia, 299.
- Schlagintweit (A. H. and R.) on the temperature and density of the Mediterranean and Red Seas, 396.
- Sea-water, on the temperature and density of, 396.
- Secchi (A.) on the periodical variations of terrestrial magnetism, 432.
- Spottiswoode (W.) on the theory of invariants, 384.
- Starch, on the digestion of, 459.
- Steam, on the value of, in the decomposition of neutral fatty bodies, 235.
- Stickleback, on the impregnation of the ovum in the, 153.
- Sulphoxylic acid, on the preparation of, 455.
- Sunlight, on the mechanical value of a cubic mile of, 36.
- Sylvester (J. J.) on differential transformation and the reversion of serieses, 391.
- Table spar, examination of, 452.
- Thacker (Rev. A.) on a case of disturbed elliptic motion, 51.
- Thomson (Prof. W.) on the possible density of the luminiferous medium, and on the mechanical value of a cubic mile of sunlight, 36; elementary demonstrations of propositions in the theory of magnetic force, 241; on the magnetic medium, and on the effects of compression, 290; on the quantities of mechanical energy contained in a fluid in different states as to temperature and density, 523; on the electro-static capacity of a Leyden phial and of a telegraph wire insulated in the axis of a cylindrical conducting sheath, 531.
- Tyndall (Dr. J.) on the existence of a magnetic medium in space, 205; on the nature of the force by which bodies are repelled from the poles of a magnet, 387; on the polymagnet, 425.
- Vapours, on the elastic forces of, 4, 44; on the heat of, 25.
- Verdet (M.) on the optical properties developed in transparent bodies by the action of magnetism, 481.
- Vine-disease, on the, 67; on the proposed remedies for the eradication of the, 71.
- Vitreous body, on the optical structure of the, 306.
- Wheatstone (C.) on the formation of powers from arithmetical progressions, 59.
- Williamson (Prof. A. W.) on the magnetic medium, 541.
- Wilson (G.), on the value of steam in the decomposition of neutral fatty bodies, 235.
- Wilson (J.), researches into the alum manufacture, 413.
- Wilsonite, note on, 382.
- Woods (Dr. T.) on sensitive collodion, 398.
- Xylidine, on the preparation of, 453.
- Xylole, on some derivatives of, 453.
- Zincethyle, on the composition of, 539.

END OF THE NINTH VOLUME.

Gases for Water between 0°C. & 20°C.



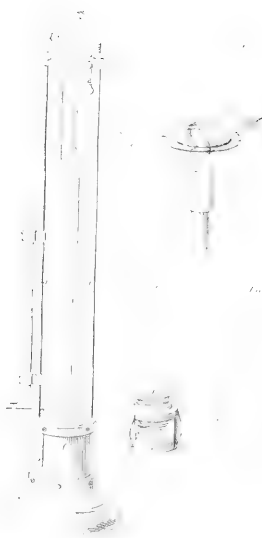
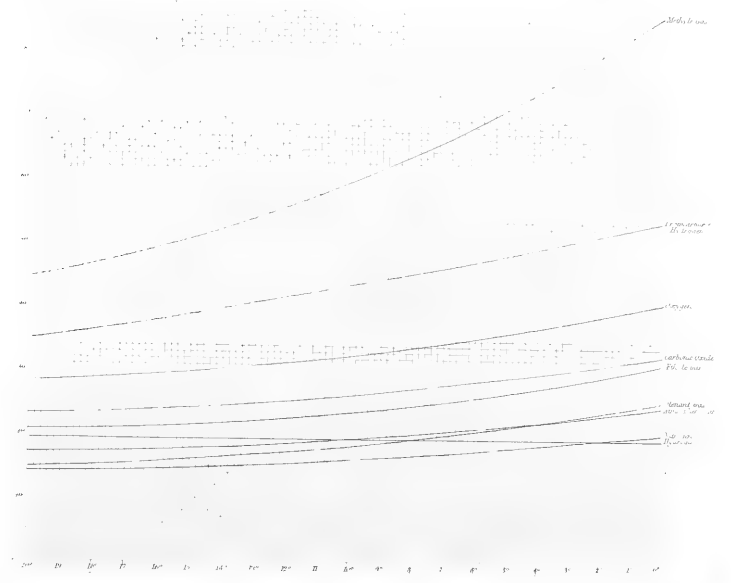
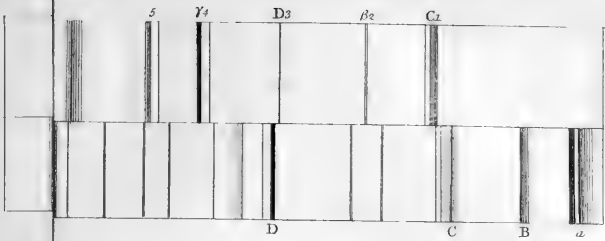


Fig. 1

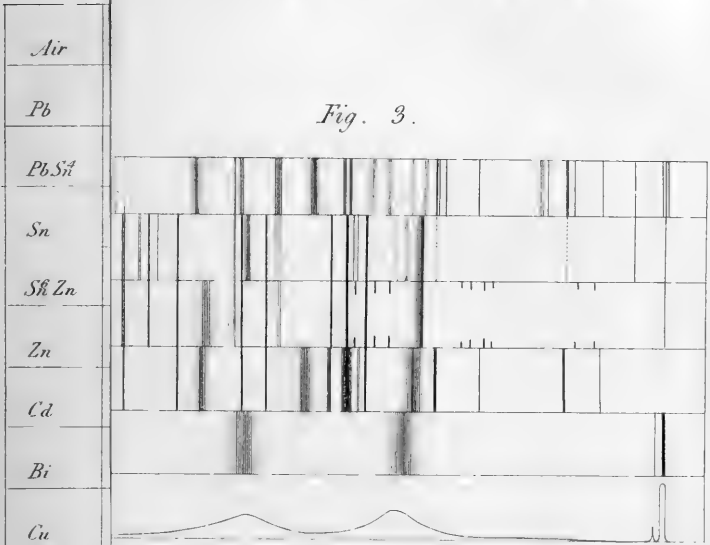
Fig. 2. Absorption curves of certain Gases for Water between 0 & 100 C.







*Fig. 3.*



*Fig. 4.*



*Fig. 5.*

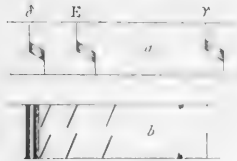


Fig. 1

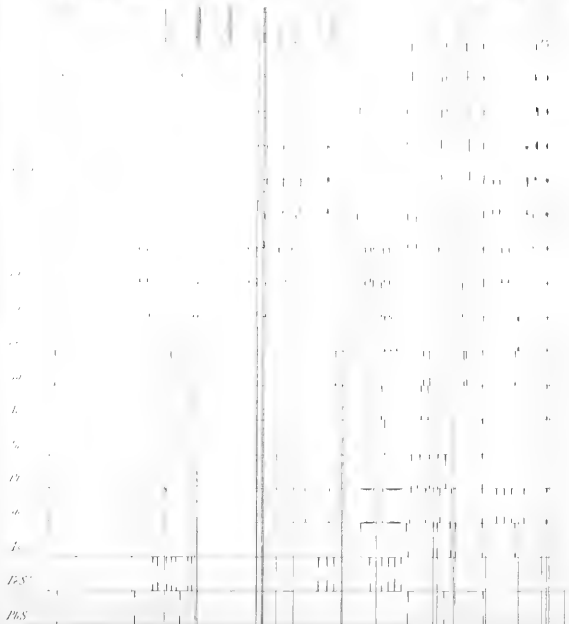


Fig. 2

Fig. 3



Fig. 4





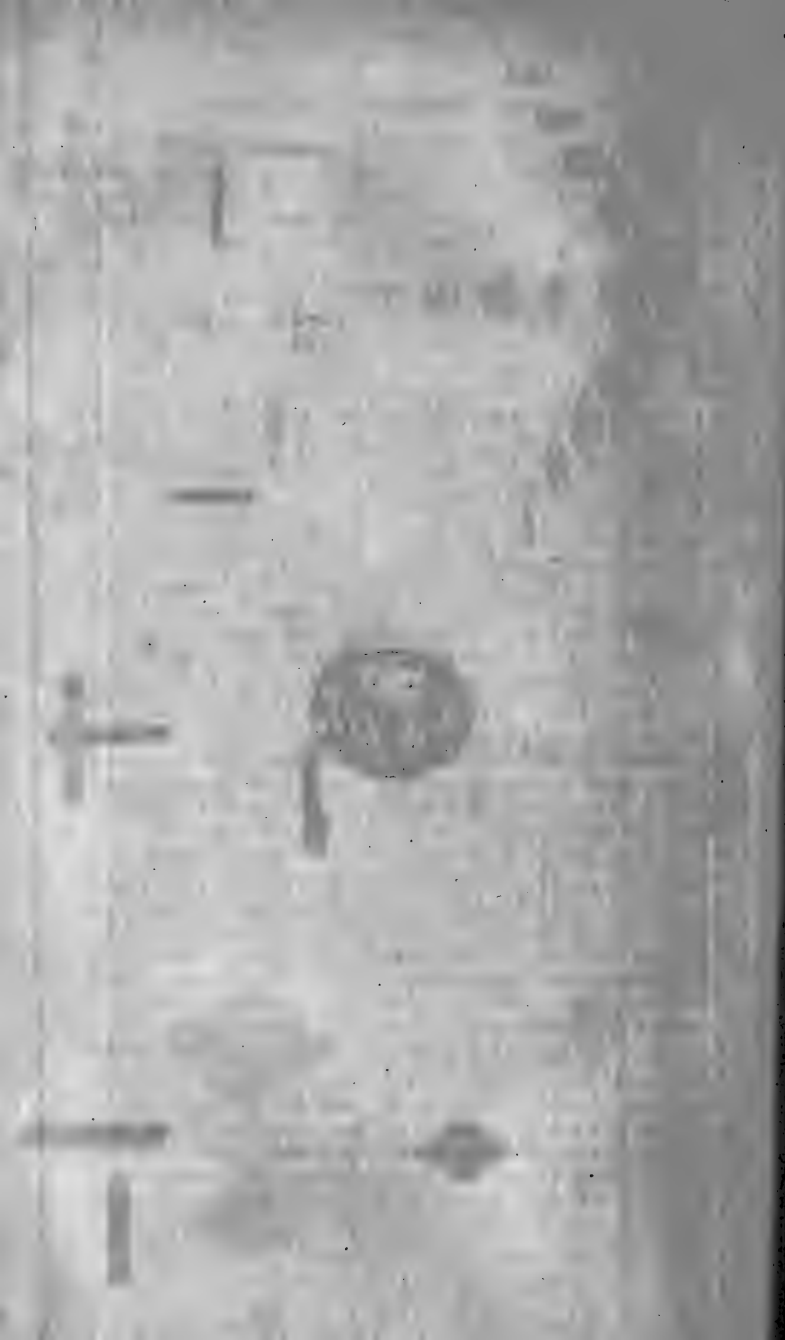


Fig. 8.

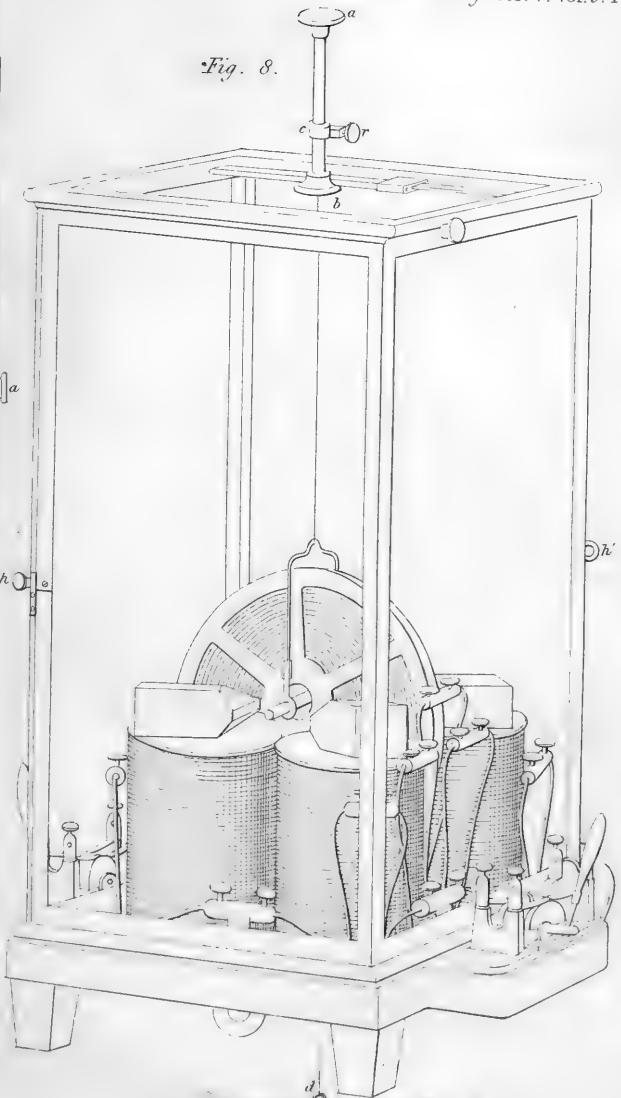


Fig. 8<sup>a</sup>





Fig. 1



